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S. I.
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S. I.

# NATURE

A WEEKLY

## ILLUSTRATED JOURNAL OF SCIENCE

VOLUME IV

APRIL 1871 to OCTOBER 1871

" *To the solid ground*
*Of Nature trusts the mind that builds for aye.*"—WORDSWORTH

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# INDEX

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"*To the solid ground
Of Nature trusts the mind which builds for aye.*"—WORDSWORTH

THURSDAY, MAY 4, 1871

## *THE SMALLER LECTURESHIPS AT THE LONDON MEDICAL SCHOOLS*

### I.—THE CONSERVATION OF FORCE

ABOUT sixty years ago the student who determined to enter the medical profession was usually bound as an apprentice to some respectable country practitioner, and spent several years in acquiring the rudiments of his profession, by bandaging bad legs, dressing simple wounds, bleeding freely everybody that presented himself and prescribing and dispensing for the poor. He then came to London, or attended one of the larger provincial towns provided with a hospital, and followed the practice of some celebrity, hearing an occasional lecture and much clinical discussion, and finally presented himself for examination before the Master and Court of Assistants of the College of Surgeons, and started in practice. Such training was solid and good; practice went before, and theory followed after; some thought, indeed, the cart went before the horse; yet the excellence of the plan was shown in the high scientific position and lucrative practice obtained by many a well-known name. As Shakespeare knew little Latin and less Greek, our student knew little anatomy and less physiology, but what he did know was substantial, and served him in good stead.

A few years after the time we are speaking of, systematic courses of lectures upon various subjects, as upon chemistry, botany, anatomy and physiology, medicine and surgery, began to be delivered at the larger schools, at the instigation of the Society of Apothecaries, who were constituted by the Act of 1815 the guardians of "general practice," two or even three subjects being given by the same lecturer; and attendance upon these soon came to be regarded as an important part of the student's education. So far all was well. The several subjects mentioned above were treated broadly by such men as Abernethy, Cooper, Babington, and others, generally speaking with direct reference to medicine or surgery; and the student underwent a training that possessed considerable value in relation to his future profession, whilst it furnished him with the rudiments of various sciences that he could pursue and extend in his leisure moments. A few years more passed away, and the advances made in every department of knowledge rendered it impossible for any man to undertake singly to lecture upon two different sciences, such as chemistry and botany, or even upon two such cognate subjects as anatomy and physiology. Each required its separate professor, who delivered from thirty to ninety lectures upon his special science, and attendance upon them was rigorously enforced both by the lecturer himself and by the examining bodies.

And now ensued a period that was undoubtedly opposed to all true intellectual training. The student, as soon as he entered the profession, saw little practice, but was everlastingly in attendance upon lectures. No mental effort was required, and, except in the case of first-rate lecturers, none, we are convinced, was ever exerted in acquiring and assimilating the information conveyed. Here and there a good lecturer, thoroughly master of his subject, chained his audience; but the substance of four out of five lectures either entered at one ear to pass out at the other, or was altogether refused admission to the brain by the locked portals of the slumbering student. The horses were indeed put before the cart, but the team was so strong that they often ran away with the cart before anything useful had been put into it. The requirements of the examining bodies in regard to these lectures rendered it imperative for every school, however small, to have as numerous a staff of lecturers as the largest. The senior officers of the medical staff consequently took the more important subjects of medicine and surgery, anatomy and physiology, whilst the younger ones divided amongst them chemistry and botany, materia medica, forensic medicine, and midwifery. In many instances these latter posts were filled by gentlemen who had received no special training, but who accepted them and often worked at them with praiseworthy energy, merely to secure the succession to the medical staff, upon obtaining which the minor lectureship was at once given up.

It is obvious that lectureships so obtained and so held must have been in many instances valueless alike to the lecturer himself and to the student who sat under him, yielding to the former a barren honour, and to the latter a signed schedule,—the advantage of the professor and not the advancement of the student being the point considered. During the last few years a reaction has been setting in against this perpetual lecturing, and the number required to be attended has been considerably reduced. The University of London deserves the credit of having been the first to break through this absurd system, by requiring attendance on only one or two courses, and this rather as evidence of the student being really engaged in the study of medicine than for any other purpose, leaving him free to acquire his information as best he can, but testing its extent and value by a searching examination.

No doubt many of the posts above alluded to are filled by men of great talent and ability, but their powers are crippled by the small means at their disposal, which prevents many illustrations or experiments from being exhibited which are almost essential for thorough teaching.

As a means of improving the system of education by supplying a better class of lectures on some subjects than those at present given, and at the same time obtaining better remuneration for the lecturers themselves, a scheme has recently been advanced by which it is proposed that certain medical schools in the metropolis should be amalgamated, a reduction in the number of lecturers being thus effected, whilst the pecuniary value of those that remain will undergo considerable augmentation. It is hoped that the value of these posts would then be sufficient to lead to their being accepted not by those who only use them as a stepping-stone for advancement, but by gentlemen who have devoted themselves exclusively to the study of the department of science on which they lecture.

At the present moment the lectureships in several of the smaller schools yield such small returns to their holders as would astonish many of their hearers. As a matter of fact we could mention an instance where the proceeds of an entire summer course of lectures has amounted on the average for the past three years to a sum not exceeding 6*l*. Can this for a moment be regarded as in any way proportionate to the intellectual labour, the time, and the money expended in their preparation, illustration, and delivery? It might be considered to be a moderate recompense for one lecture, but as payment for a course it is simply monstrous. Is it surprising that the lectures are often given without animation, and listened to without interest?

By amalgamating several schools, however, such chairs might, it is hoped, be so far increased in value as not only to lead men of high ability, and distinguished for their knowledge in particular branches of science, to accept them, but to provide ample funds to admit of their copious illustration, and for the purchase of expensive apparatus — apparatus which the smaller schools now find it difficult or impossible to procure. It would not be difficult, we imagine, to find room for those who at present hold appointments as demonstrators, with lighter but not less important duties than they have hitherto performed. At all events it seems to us that the amalgamation scheme, if fairly carried out, would prove the most splendid example of the Conservation of Force with which we are acquainted, and on that ground alone should receive the cordial support of the medical teachers throughout the metropolis. In a future article we shall suggest what appears to us a desirable and practical scheme for medical education.

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## *THE LITERATURE OF CHEMISTRY*

THE appearance of the April number of the "Journal of the Chemical Society" marks the commencement of a new era in English Chemical Literature, containing, as it does, besides the papers which have been read before the Society, the first instalment of the promised "abstracts." The papers selected for this purpose by the accomplished editor are ninety-one in number, comprising every branch of Chemical Science, Technology included, and are classified under six various headings, as "Physical Chemistry," "Inorganic Chemistry," &c. The abstracts themselves, made by the gentlemen whose names appear on the wrapper of the journal, are naturally of different degrees of literary merit, but seem to be carefully and conscientiously done; all the points of essential importance in the original papers being retained. The reader will thus not only have a good general notion of the extent of the researches made by any particular author, but also be able to repeat any of the experiments, or prepare any of the substances from the directions given. These abstracts are therefore really what they profess to be, and not merely notices of a few lines in length, from which but little more information can be gleaned than from the title of the paper.

The Council of the Chemical Society is to be congratulated on the energetic way in which it has endeavoured to supply a great defect in our scientific literature, by affording us the means of obtaining a general view of the progress of Chemistry both here and on the Continent. Chemists have hitherto had to depend chiefly on Will's "Jahresbericht," which, although useful in its way, has the double disadvantage incident upon its method of arrangement, first, in not being published until long after the end of the year, and, secondly, of being rather a *résumé* of the chemical work done, than a condensed account of particular researches. There is no doubt that these abstracts, if furnished with a full and comprehensive index, both of the subject-matter and the names of the authors, will become a standard work of reference, not only here but on the Continent.

It is to be hoped that other Scientific Societies will be induced to follow the example of the Chemical Society, and, by publishing abstracts of all papers connected with their particular branch of science, give an impetus to its cultivation, and render a knowledge of its general progress easily attainable. The value of such abstracts is greater than might at first sight appear; for the study of Science, both for its own sake, and in its application to the Arts, is extending so rapidly that it requires a considerable expenditure of time to acquire a knowledge of the numerous researches and discoveries which are now being made in any particular science, and leaves but little for the study of the sciences allied to it. If, then, each of the learned societies were to publish abstracts similar to those of the Chemical Society, it would render it comparatively easy for the workers in any one department of science to acquire something more than a superficial knowledge of the discoveries made in the others.

## *GLAISHER'S TRAVELS IN THE AIR*

*Travels in the Air.* By James Glaisher, Camille Flammarion, W. de Fonvielle, and Gaston Tissandier. Second and revised edition. With 125 illustrations. (London: R. Bentley, 1871.)

BOTH the scientific and the lover ot adventure will find abundance to interest them in this handsome volume. The terrestrial fields of enterprise are getting exhausted. Mont Blanc has long since been used up. We are getting tired of Central Africa and the Steppes of Tartary. Even the invitation "Try Lapland" fails to stimulate the jaded nerves of the zealous explorer of "fresh fields and pastures new." In the realms of air, however, there is still plenty of new ground, if we may be allowed the Hibernicism. Mr. Glaisher and the illustrious French trio can claim this field as almost exclusively their own, though, doubtless, they will not long be left in undisturbed possession of it. After a brief history of the rise and progress of aërostatics in England, Mr. Glaisher here recounts to us the particulars of ten of his most remarkable ascents; and the Frenchmen then follow suit. The volume is got up in drawing-room style, as a veritable *livre de luxe*; we wish we could transfer to our pages some of the beautiful chromo-lithographs by which it is illustrated, in particular, the wonderful mirage and luminous aureole which serves as frontispiece, and the falling stars as observed from the balloon, at p. 262. We must, however, content ourselves with two or three of the scarcely less effective woodcuts.

FIG. 1—MIRAGE IN THE SKY, AS SEEN FROM THE BALLOON

The scientific information contained in the volume is important, though rather as showing how little we know at present of even the fundamental principles of Meteorology, than as establishing any new laws. With regard to temperature, Mr. Glaisher remarks that the decrease as we ascend is far from constant, and we must entirely abandon the theory of a decline of 1° of temperature for every increase of 300 ft. of elevation. With reference to the colour of the sky, he states that, as viewed from above the clouds, it presents a deep blue colour, which deepens in intensity with increase of elevation regularly from the earth if the sky be free from clouds, or with the increase of elevation above the clouds if they be present, and on this subject he gives the following laws :—

"The azure colour of the sky, though resembling the blue of the first order when the sky is viewed from the earth's surface, becomes an exceedingly deep Prussian blue as we ascend, and, when viewed from the height of six or seven miles, is a deep blue of the second or third order. 2. The maximum polarising angle of the atmosphere, 45°, is the same as that of air, and not of water, which is 53°. 3. At the greatest height to which I have ascended, namely, at the height of five, six, and seven miles, where the blue is the brightest, the air is almost deprived of moisture. Hence it follows that the exceedingly deep Prussian blue cannot be produced by vesicles of water, but must be caused by reflection from the air, whose polarising angle is 45°. The faint blue which the sky exhibits at the earth's surface is therefore not the blue of the first order, but merely the blue of the second or third order rendered paler by the light reflected from the aqueous vapour in the lower regions of the atmosphere.

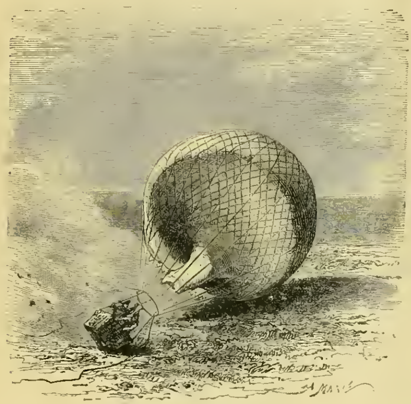

FIG. 2—DRAGGING

To appreciate all the beauty of cloud scenery when the air is loaded with moisture, an aërial voyage must be made on an autumn morning before sunrise, when the atmosphere is charged with the vapours of night."

Clouds were frequently met with to a height of 20,000ft. or nearly four miles, and heavy rain at almost as great an altitude; and on one occasion, while descending, rain fell on the balloon at a height of three miles, and then for the next 5,000ft. lower, it passed through a beautiful snowy scene; there were no flakes in the air, the snow was entirely composed of spiculæ of ice, of cross spiculæ at angles of 60°, and of an innumerable number of snow crystals, small in size, but of distinct and well-known forms easily recognisable as they fell and remained on the coat. The drawings show many a beautiful scene—sunrise from a balloon, moonlight effects, a lunar halo, the shadow of a balloon on the clouds, sometimes surrounded by an aureole, though, perhaps, none more remarkable than the mirage represented in our first illustration.

Humorous incidents occur here and there; as when the whole apparatus is taken by the French peasantry for "le diable" himself, or when the travellers approaching the earth are required by too zealous gensdarmes to show their passports! And the adventures are not without their serious attendant dangers. More than once the diminished pressure and the intense cold produced so great a numbness and tendency to sleep that it has required the greatest presence of mind for all control over the balloon not to be lost—and for ever. Life and limb were also not unfrequently endangered by the too sudden descents, sometimes to escape the imminent peril of an involuntary dip into the sea. Fig. 2 depicts the manner in which the "Swallow," having Tissandier and de Fonvielle on board as passengers, was dragged along the ground by a furious gale, and both those eminent aëronauts were considerably hurt and in danger of losing their lives.

There is not much contribution in the volume to the mechanics of aërostation, and that mostly from the French

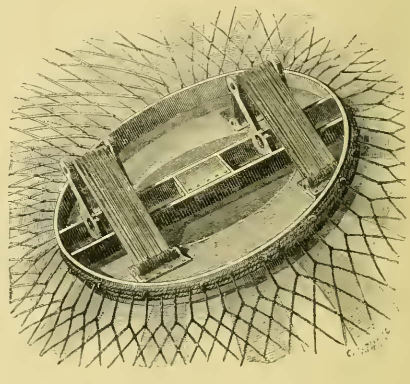

FIG. 3—THE VALVE OF THE "ENTREPRENANT" BALLOON

contributors. We have drawings of the weighing machine and pulleys of the great Captive balloon of Chelsea, and Fig. 3 represents the valve of the "Entreprenant" balloon from which M. de Fonvielle attempted to take photographs of an eclipse of the moon. The book is one which will doubtless find a large circle of readers, and will greatly increase the public interest in aërostatics.

## *OUR BOOK SHELF*

*A Text-Book of Elementary Chemistry, Theoretical and Inorganic.* By George F. Barker, M.D. (New Haven. Conn.: C. C. Chatfield and Co., 1870, pp. 336.)

THIS little book is evidently the result of much labour on the part of the author, and cannot fail to be of much value to students of chemistry. In the preface a list of books is given of which the author has made free use; consequently the peculiarities of the systems of many chemists are to be found in the book; but though it cannot be said that any school has been followed, yet all are more or less represented. The prevailing ideas are that each element has a definite combining power or equivalence, and that the arrangement of atoms in compounds is of as much importance as their kind or number. This work is remarkable for the conciseness of its definitions; one of the first is on chemical and physical changes, in which it is said that "physical changes in matter are those which take place outside the molecule; they do not affect the molecule itself, and therefore do not alter the identity of the matter operated on. Chemical changes take place within the molecule, and hence cause a change in the matter itself." Some of the definitions would not, however, find general acceptance; thus, an acid molecule is said to be "one which consists of one or more negative atoms united by oxygen to hydrogen;"—a definition which excludes hydrochloric acid and its analogues. And a saline molecule is defined to be one containing a "positive atom or group of atoms, united by oxygen to a negative atom or group of atoms," which removes sodic chloride from the list of salts. The term base is confined to the hydrates of positive elements or groups of elements, and the hydrates of the metals calcium, zinc, and iron are sometimes called calcic base, zincic base, ferrous base, and ferric base. The nomenclature of the acids is systematised, but peculiar names are the result: an ortho-acid is one containing as many atoms of oxygen and hydrogen as is equal to the equivalence of the negative atom or group; and a meta-acid is derived from an ortho-acid by the subtraction of molecules of water, thus orthophosphoric acid would be $P(OH)_5$, metaphosphoric acid $(PO)'''(OH)_3$, and dimetaphosphoric acid $(PO_2)'(OH)$. These names and those of most other acids are liable to some misunderstanding, as the compounds they represent have long been known by other designations. The theoretical part of the book contains chapters on elemental molecules and atoms, compound molecules, volume relations of molecules, and stoichiometry. The part on inorganic chemistry is divided into eleven chapters, on hydrogen, the negative monads, dyads, triads, boron, negative tetrads, the iron group, positive tetrads, triads, dyads and monads, thus treating of the elements according to their electro-chemical characters, commencing with the most negative. Each chapter is divided into sections containing the history, occurrence, preparation, and properties of the elements, and is followed by a series of questions intended as exercises for the students, a method now much adopted, and found to be of great assistance to teachers. This book is another of the evidences of the rapid progress of pure science in America.

*Czermak's Electric Double Lever. (Der Electrische Doppelhebel, von J. N. Czermak.)* (Leipzig: Engelmann. 1871. London: Williams and Norgate.)

A DESCRIPTION of a most ingenious little contrivance for marking the exact moment in which a movement begins or changes its direction. The old arrangement, by which a lever, forming part of a circuit, comes, when set in motion, in contact with a fixed point connected with the other part of the same circuit, and so closes the circuit and makes a signal, is modified by Prof. Czermak as follows. The fixed contact point is replaced by a secondary lever, whose axis of revolution is the same as that of the primary lever. This secondary lever bears at one end a contact point. The primary lever touches in its swing this contact point, and so closes the circuit; it then pushes the secondary lever before it, but having reached the limit of its oscillation, leaves the secondary lever at rest in a position marking the farthest point of the excursion. A counter contact-point, however, on the other arm of the primary lever (where the lever is a double-arm one; with single arm levers, a special arrangement is introduced), as the primary lever is returning into position gives to the secondary lever a movement in the same direction. Thus the two levers are continually following each other, making and breaking contact. The instrument is in this way made capable of being used for signalling all manner of movements. It is impossible fully to explain its construction in a few lines, and we therefore refer the reader to the pamphlet itself, which, we should say, is published in celebration of the Jubilee of the great Leipzig Professor, Ernst Heinrich Weber. By the invention of his delightful "Rabbit Holder," Czermak has endeared himself to every physiologist, and we may well share his hope that this new double lever will be found no less useful. M. FOSTER

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Pangenesis

IT appears from Mr. Darwin's letter to you in last week's NATURE,* that the views contradicted by my experiments, published in the recent number of the "Proceedings of the Royal Society," differ from those he entertained. Nevertheless, I think they are what his published account of Pangenesis (Animals, &c., under Domestication, ii. 374, 379) are most likely to convey to the mind of a reader. The ambiguity is due to an inappropriate use of three separate words in the only two sentences which imply (for there are none which tell us anything definite about) the *habitat* of the Pangenetic gemmules; the words are "circulate," "freely," and "diffused." The proper meaning of circulation is evident enough—it is a re-entering movement. Nothing can justly be said to circulate which does not return, after a while, to a former position. In a circulating library, books return and are re-issued. Coin is said to circulate, because it comes back into the same hands in the interchange of business. A story circulates, when a person hears it repeated over and over again in society. Blood has an undoubted claim to be called a circulating fluid, and when that phrase is used, blood is always meant. I understood Mr. Darwin to speak of blood when he used the phrases "circulating freely," and "the steady circulation of fluids," especially as the other words "freely" and "diffusion" encouraged the idea. But it now seems that by circulation he meant "dispersion," which is a totally different conception. Probably he used the word with some allusion to the fact of the dispersion having been carried on by eddying, not necessarily circulating, currents. Next, as to the word "freely." Mr. Darwin says in his letter that he supposes the gemmules to pass through the solid walls of the tissues and cells; this is incompatible with the phrase "circulate freely." Freely means "without retardation;" as we might say that small fish can swim freely through the larger meshes of a net; now, it is impossible to suppose gemmules to pass through solid tissue without *any* retardation. "Freely" would be strictly applicable to gemmules drifting along with the stream of the blood, and it was in that sense I interpreted it. Lastly, I find fault with the use of the word "diffused," which applies to movement in or with fluids, and is inappropriate to the action I have just described of solid boring its way through solid. If Mr. Darwin had given in his work an additional paragraph or two to a description of the whereabouts of the gemmules which, I must remark, is a cardinal point of his theory, my misapprehension of his meaning could hardly have occurred without more hesitancy than I experienced, but I certainly felt and endeavoured to express in my memoir some shade of doubt; as in the phrase, p. 404, "that the doctrine of Pangenesis, pure and simple, as I have interpreted it, is incorrect."

As I now understand Mr. Darwin's meaning, the first passage (ii. 374), which misled me, and which stands: " . . minute granules . . . which circulate freely throughout the system" should be understood as "minute granules . . . which are dispersed thoroughly and are in continual movement throughout the system;" and the second passage (ii. 379), which now stands: "The gemmules in each organism must be thoroughly diffused; nor does this seem improbable, considering . . . the steady circulation of fluids throughout the body," should be understood as follows: "The gemmules in each organism must be dispersed all over it, in thorough intermixture; nor does this seem improbable, considering . . . the steady circulation of the blood, the continuous movement, and the ready diffusion of other fluids,

* NATURE, vol. iii. p. 502.

and the fact that the contents of each pollen grain have to pass through the coats, both of the pollen tube and of the embryonic sack." (I extract these latter *addenda* from Mr. Darwin's letter.)

I do not much complain of having been sent on a false quest by ambiguous language, for I know how conscientious Mr. Darwin is in all he writes, how difficult it is to put thoughts into accurate speech, and, again, how words have conveyed false impressions on the simplest matters from the earliest times. Nay, even in that idyllic scene which Mr. Darwin has sketched of the first invention of language, awkward blunders must of necessity have often occurred. I refer to the passage in which he supposes some unusually wise, ape-like animal to have first thought of imitating the growl of a beast of prey so as to indicate to his fellow monkeys the nature of expected danger. For my part, I feel as if I had just been assisting at such a scene. As if, having heard my trusted leader utter a cry, not particularly well articulated, but to my ears more like that of a hyena than any other animal, and seeing none of my companions stir a step, I had, like a loyal member of the flock, dashed down a path of which I had happily caught sight, into the plain below, followed by the approving nods and kindly grunts of my wise and most-respected chief. And I now feel, after returning from my hard expedition, full of information that the suspected danger was a mistake, for there was no sign of a hyena anywhere in the neighbourhood. I am given to understand for the first time that my leader's cry had no reference to a hyena down in the plain, but to a leopard somewhere up in the trees; his throat had been a little out of order—that was all. Well, my labour has not been in vain; it is something to have established the fact that there are no hyenas in the plain, and I think I see my way to a good position for a look out for leopards among the branches of the trees. In the meantime, *Vive* Pangenesis. FRANCIS GALTON

### The Hylobates Ape and Mankind

THE readers of Mr. Mivart's communication in NATURE for April 20, on the affinity of the Hylobates genus of ape to the human species, may be interested to learn that the fact was well known to the author of the Ramayana, the earliest Sanscrit epic, probably contemporaneous with the Iliad. In this poem the demigod Rama subdues the demon Ravana, and regains his ravished bride Sita by the assistance of a host of apes, which may be identified with *Hylobates Hoolook*. The human characteristics of these semi-apes, their gentleness, affection, good humour, sagacity, self-importance, impressionability, and proneness to melancholy, are portrayed with the most vivid strokes, and evidently from careful observation. See Miss Frederika Richardson's charming volume, "The Iliad of the East," a selection of legends drawn from the Ramayana. (Macmillan and Co., 1870.)

April 27 R. G.

### Tables of Prime Numbers

WHEN a number is given, and it is required, without the aid of tables, to find its factors, there is not, I believe, any other method known except the simple but laborious one of dividing it by every odd number until one is found that measures it, and if the number should be prime, this can only be proved by showing that it is not divisible by any odd number less than its square root. Thus to prove that 6966007 is prime, it would be necessary to divide it by every odd number less than 2639, and even if a table of primes less than 2639 were at hand, about 380 divisions would be requisite.

On the other hand, there are few tables which are more easily constructed than tables of divisors, and it is the extreme facility of a systematic tabulation compared to the labour of isolated determinations, which has led to the construction of such elaborate tables on the subject as have been produced.

The principal tables are Chernac's, which give the factors of numbers from unity to a million; Burckhardt's, which extend as far as three millions, and Dase's, which form a continuation of Burckhardt's, and extend to ten millions.

The mode of formation of these tables was extremely simple. By successive additions, the multiples of 3, 5, 7, 11, 13, 17 . . . were formed up to the limit to which the table was intended to extend; this gave all the numbers having these numbers for factors, and the primes were recognised from the fact of their not occurring as multiples of another prime less than themselves.

Practically the work was rendered even simpler by mechanical means; thus, forms were printed containing, say, a thousand squares, and in these were written consecutive thousands of odd numbers in order; one number in each square, room being left for its divisors, if any, in the square. A pair of compasses was then taken and opened a distance corresponding to the prime whose multiples were to be obtained; for example, in marking the multiples of seven, the compasses were opened the width of seven squares, and then "stepped" along the lines starting from 7, thereby marking the numbers 7, 21, 35 . . . and the number 7 was written in each of the squares in which a leg of the compasses fell. When the factor was large it was more convenient to form a separate table of its multiples, and enter it in the square corresponding to the latter Many simplifications were introduced in the details of the construction; for instance, Burckhardt had a copper plate engraved with 77 ($=7\times 11$) squares one way and 80 the other; by this arrangement the multiples 7 and 11, which were of the most frequent occurrence (for all multiples of 2, 3, and 5 were rejected from the tables), occupied the same place on each sheet, and he was thus enabled to engrave the numbers 7 and 11 on the plate, so that these numbers were *printed* in all the squares containing the numbers they measured.

Dase, who originally applied himself to the construction of the tables at the suggestion of Gauss, left behind him in manuscript at the time of his death, in 1862, the seventh and part of the eighth million complete, besides a considerable portion of the ninth and tenth millions. The seventh, eighth, and ninth millions were completed by Dr. Rosenberg, and published by a committee at Hamburgh. In the preface to the ninth million (1865), which is the last I have seen, it is stated that the tenth million, which was nearly ready, was the last the committee intended to publish.

My object in writing this letter is not only to call attention to a most valuable series of tables, which seem to have scarcely excited so much interest as they deserve, but also to ask if any of your readers can inform me if the work is being continued, or if there is any chance of its continuation. It is not often that tables are so indispensable as in the present case, or that a want so pressing can be supplied with such comparative ease; and the cessation of the tables would be a real calamity. The tenth million has, I presume, been published.

At the British Association Meeting at Dundee in 1867, a list of 5,500 large prime numbers was communicated to Section A by Mr. Barrett Davis. A short discussion took place on the "reading" of the paper, in the course of which it was stated that Mr. Davis's table was unaccompanied by any explanation of how the numbers had been obtained, or on what grounds they were asserted to be prime; it was also asserted that Mr. Davis wished to keep his method secret.

Perhaps some reader of NATURE can say whether Mr. Davis's numbers have been printed. If they exceed Dase's limit, their publication (if they have not yet been published) is very desirable; and even supposing they are given in Dase's tables, it would be valuable to know how far the latter have been verified by them. The statement about Mr. Davis's method being secret was probably founded on some mistake, and no doubt Mr. Davis would not object to explain it. J. W. L. GLAISHER

Trinity College, Cambridge, April 29

### Units of Force and Energy

THE best root for the name of a unit of force is δύναμις. There is, therefore, no ground for Mr. Muir's complaint (NATURE, vol. iii. p. 426), and I now venture to propose that the name *dyne* be given to that force which, acting on a gramme for a second, generates a velocity of a metre per second. A thousand dynes to make one *kilodyne*, and a million dynes one *megadyne*.

Borrowing a hint from Mr. Muir, I would point out that the kilodyne may also be defined as the force which, acting on a *kilogramme* for a second, generates the velocity of a *metre* per second, or, as the force which, acting on a *gramme* for a second, generates a velocity of a *kilometre* per second.

The *kinit*, or pound-foot-second unit of force, is about 138¼ dynes. Very roughly expressed in terrestrial gravitation measure, the kinit is the gravitating force of half an ounce, the dyne of about 1½ grains, the kilodyne of about ¼ of a pound, and the megadyne of 2 cwt., the approximation being much closer in this last case than in the others, so that within one part in 400 we have 10 megadynes = the force of terrestrial gravity on a ton.

I have often felt the want of a name for an absolute unit of energy, or, what amounts to the same thing, an absolute unit of work. If the above names be adopted, they give us at once the foot-kinit as the unit of work based on the pound, foot, and second, the foot-pound (which varies with the value of $g$) being equal to $g$ foot-kinits.

In like manner we have, for the metrical system, the *metre-dyne* and its derivatives.

But it would, I think, be advantageous to have short and independent names for these units. For, in the first place, we are thus saved from such cumbrous names as *metre-kilodyne* and *metre-megadyne*, which would be necessary in expressing large quantities of work; in the second place, energy of motion depends directly upon mass and velocity, and is only indirectly connected with the unit of force; and, in the third place, the characteristics of energy are such as specially entitle it to names suggestive of simplicity rather than of compositeness.

I propose, therefore, to call the foot-kinit, whether of work or energy, the *erg*. A thousand ergs to make one *kilerg*, which will be about 31 terrestrial foot-pounds, and a million ergs to make one *pollerg*, which is a little less than the work done by one horse-power in a minute.

The kinitic energy of $m$ pounds, moving with a velocity of $v$ feet per second, is $\frac{1}{2} mv^2$ when expressed in ergs.

The energy value of a Fahrenheit unit of heat is $772 \times 32{\cdot}194 = 24{,}854$ ergs.

In the metrical system, let the metre-dyne of work or energy be called the *pone* (from πόνος). A thousand pones to make one *kilopone*, which is the work done by a *kilodyne* working through a *metre*, or by a *dyne* working through a *kilometre*, and is about $\frac{1}{9{\cdot}81}$ of the variable unit of work in common use among French engineers, called the kilogrammetre. A million pones to make one *megapone*, which is about 723 terrestrial foot-pounds.

In employing the prefix *mega* to denote a million, I have followed the excellent example set by the B. A. Committee on Electrical Standards. As *megerg* would be intolerable, and *megalerg* sounds like a confusion of genders, I have substituted *pollerg*.

In constructing a new nomenclature, the metrical system is entitled to the best names which can be found, but the pound and foot cannot be ignored. J. D. EVERETT

Rushmere, Malone Road, Belfast

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### The Name "Britain"

IN his remarks on the derivation of the name "Britain," "A. R. H." says that tin "is found only in one of the Britannias." This is incorrect, for tin occurs in Brittany, and also in Gallicia. The fact of the three Britains mentioned by "A. R. H." being all tin-bearing districts seems to confirm the derivation given by Mr. Edmonds in NATURE for February 16.

C. L. N. F.

Piedimulera, Val d'Ossola, Piedmont, April 25

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### Derivation of the Word "Britannia"

IF Mr. Edmonds considers himself right in his derivation of "Britannia" and "tin," he will have to explain on the same basis the conformable names, and this he will find difficult to do.

The name B-ritannia corresponds with S-ardinia, D-ardania, and possibly with Mauritania, and these again with a number of river names of the root RDN (=RND, BRN, &c.), such as Rotanus, Rhodanus, Drinus, Eridanus, Artanus, Triton, Orethus, &c. B-radanus, P-rytanis, P-arthenias, V-artanus, are examples of B. K-artenus, I-ordanes, I-ardanes, I-ardenus. Then there are examples of Aternus, &c., Tanarus, &c., Mæander, &c., Orontes, &c. These must all be explained on one principle.

In the same way as Britannia is allied to river names, so are many of the ancient (classic) names of countries (except such as are volcanic) allied to river names of various roots, as RBD, &c., RKN, &c., SBN, &c.

These names are not explainable in Phœnician, because they were given long before the Phœnicians entered on the stage of history. They are Palæogeorgian, in a language to which Georgian, Lesghian, and other Caucasian languages are allied. These names were given by the Caucaso-Tibetans.

This is explained in my paper lately read before the Anthropological Institute and recorded in NATURE, and the name of Britannia is illustrated in papers sent in to the Society of Antiquaries and the Royal Irish Academy.

32, St. George's Square HYDE CLARKE

### Aurora by Daylight

THAT the Aurora Borealis has been seen by daylight has never been doubted by me, although till now I have not been able to collect sufficient evidence to induce others to believe in the possibility of it. Your correspondent Mr. John Langton, in your last issue, gives two instances of the aurora having been seen during day time, which, I think, ought to dispel all further doubt. However, to satisfy the most sceptical of your readers, the following few cases have occurred to me:—

"A.D. 1122. . . . . . . This same year died Ralph, Archbishop of Canterbury; that was on the 13th of the kalends of November (October 20). After this were many shipmen at sea and on the water, and they said that they saw on the north-east along the earth a great and broad fire, and it increased speedily upwards in extent towards the sky, and the sky opened itself in four parts and fought there against it as if it would extinguish it: but nevertheless the fire extended up to heaven. They saw that fire in the dawn of the day, and it continued until it was quite light. This was on the 7th of the ides of December (December 7)."—*Anglo-Saxon Chronicle*.

It may seem bold to advance this as the record of an auroral appearance, but not to those who have studied this and other chronicles with their wearying vaguenesses. This passage gains clearness by the following lines from the "Prose Edda," concerning "The Twilight of the Gods and the Conflagration of the Universe," which I have elsewhere* supposed to be a description of the aurora borealis:—

"The fire-reek rageth
Around Time's nurse,
And *flickering flames*
With heaven itself playeth."

In the "Second Continuation of the History of Croyland," there is the following curious passage, under A.D. 1467:—"For one day . . . . . . . . . . horsemen and men in armour were seen rushing through the air; so much so, that St. George himself, conspicuous with the red cross, his usual ensign, and attended by a vast body of armed men, appeared visibly in great numbers. To show that we ought not to refuse our belief to what has been just mentioned, those persons to whom revelations of this nature were made were subjected to the most strict examination before the venerable Father Thomas, the Lord Archbishop of Canterbury."

I understand this occurrence to have taken place in the day between the rising and setting of the sun, because this passage is only part of a longer account of remarkable events which were said to have been observed in "one day." I do not put this instance forward as one of very great value,† as the Chronicle of Ingulf is undoubtedly spurious, as shown by Dr. Hickes and Sir Francis Palgrave, but the continuation, I think, can be safely said to date about the end of the 15th or the beginning of the 16th century, which, if correct, will place the phenomenon above referred to amongst the earliest notices of daylight Auroras in English History, and will come next to that mentioned in the *Anglo-Saxon Chronicle*. Of course I only speak here of my own acquaintance with the Chronicles, there may be other records, but I have not had the opportunity of searching through every monastic production.

Leaving this field of speculation, I come next to a more reliable record. I give the whole of the passage, as it is not very long:—"Aurora Borealis, seen in the Day-time at Canonmills." "The morning of Sunday, September 9, was rainy, with a light gale from the N.E. Before mid-day the wind began to veer to the west, and the clouds in the north-western horizon cleared away: the blue sky in that quarter assumed the form of a segment of a very large circle, with a well-defined line, the line above continuing dense, and covering the rest of the heavens. The centre of the azure arch gradually inclined to the north, and reached an elevation of 20°. In a short time, very thin fleecy clouds began to rise from the horizon within the blue arch; and through these very faint perpendicular streaks of a sort of milky light could be perceived shooting; the eye being thus guided, could likewise detect the same pale streaks passing over the intense azure arch, but they were extremely slight and evanescent. Between nine and ten in the evening of the same day, the aurora borealis was very brilliant, so that there is no reason to doubt that the azure

* Vide NATURE, vol. iii. p. 175.

† For a similar case to this see note to my letter on the aurora borealis in NATURE, vol. iii., p. 487.

arch in the morning, and the pale light seen shooting across it, were connected with the same phenomenon."*

I have just been informed by a friend whose veracity I would be the last to question, that he saw a very faint arch in the eastern sky on the afternoon of the 10th inst. (about 4.30 P.M.). There were no clouds near it, while the background was a beautiful azure. The colour of the arch was of a much fainter blue, or, as he calls it, "a whitish blue," and was almost a perfect semicircle. I have not the least doubt that it was a "daylight" aurora; it must be remembered that on the previous night there was a most magnificent aurora borealis.

In conclusion, after carefully examining the facts contained in the various communications to your journal, as well as those which I have collected, I cannot see any reason for doubting the possibility of the aurora borealis being seen by daylight. It will be interesting to know what those daylight phenomena are, if not auroras.

JOHN JEREMIAH

Red Lion Street

### The Irish Fern in Cornwall

YOUR correspondent having, much to my regret, so exactly informed the "ruthless collectors" where they are to look for this fern, I fear that after the ensuing autumnal ravages not a single frond will be left to speak for itself. Permit me, therefore, to state that the fern unquestionably grows, or did grow, at the place indicated, and was, I believe, first recognised in 1866 by Mr. Robert Were Fox, F.R.S., who has a plant he thus obtained still growing in his fernery at Penjerrick near this town.

W. P. DYMOND

Falmouth, April 29

### The Prevalence of West Winds

IN a letter with this heading in NATURE for February 16th, Mr. Murphy has very roundly objected to certain views which I have put forward regarding the predominance of westerly winds. In the paper read before the British Association, to the abstract of which he refers, and which was itself little more than a *résumé* of the propositions maintained at greater length in my "Physical Geography," reviewed by "A. B." in NATURE for March 16th, my object was not so much to show that westerly winds predominated in volume over easterly winds, as to show that all prevailing winds, not westerly, may be properly considered as deflected or secondary currents of air, and that more especially the trade winds may be so considered. I have supported this view by a detailed examination of the geographical circumstances, habitudes, and characteristics of the principal winds; but to have included every local exception—as "A. B." seems to consider I ought to have done—would have required more time than even the most industrious can spare, an amount of special topographical knowledge which is practically unattainable, and would have had no important bearing on the main question. I may go even further. I may say that, from a general point of view, isolated local registers have no value at all, unless the method of observing and the position of the vane are distinctly made known. It would be perfectly easy to name a dozen localities in Wales, in the Lake District, or in Scotland, where a vane would show a prevailing wind widely different from the W.S.W., which, however, we have no difficulty in accepting as the prevailing wind of the country; even at Liverpool the prevailing wind has been observed to be W.N.W., and at Valentia there is a marked difference between the wind in the northern and southern entrance. In Mr. Buchan's paper in the Transactions of the Royal Society of Edinburgh, December, 1869, I find that at Irkutsk the wind is almost always due north, or due south, would "A. B." imply that the Irkutsk observations afford any information as to the prevailing wind of Siberia?

In another paragraph, "A. B." considers that the preponderance of westerly winds cannot be very great. So far as the area over which westerly winds blow is concerned, I would partly agree with him; taking into account the constant interruptions to the west winds in the temperate zones, and on the other hand their frequent intrusion into latitudes considerably below 30°, more especially in the Pacific, and their prevalence during several months of the year over a large portion of the Indian Ocean, I am inclined to reckon the ratio of the area of westerly winds to the area of easterly winds as approximately 13 : 10. But such an estimate does not in any way include the velocity of the wind; and since the velocity of the west winds of temperate latitudes is, in the mean, about double that of the easterly winds of tropical, it would follow that the respective volumes of the winds bear to each other a much larger ratio, which, allowing freely for every reasonable reduction, cannot be less than 2 : 1. And this estimate still relates only to the lower strata of the atmosphere, through a height probably not exceeding 12,000 feet. Our knowledge of the winds above that height is very limited; but since, wherever observation extends, it points out to us a strong, frequently even a violent west wind, it seems to me that we have a fairly presumptive proof that the prevailing direction of the upper current is from the west. I base this belief entirely on the evidence which we have, defective as it is and as it almost necessarily must be; to explain the fact by a reference to a difference of barometric pressures, concerning which we have positively no evidence at all, is a task which I most willingly leave to my reviewer. But if, as I have maintained, we may fairly assume that the upper current has an almost invariable direction from the west, and that too with a comparatively high velocity, the ratio of the volumes of westerly and easterly winds is enormously increased, and if the upper part of the air, being quite half of the whole, is moving from the west with a mean velocity of 40 miles an hour, then, as we have already taken 20 miles, or the velocity of the trade winds, as the standard or unit of reference, we have the ratio of westerly to easterly winds as about 6 : 1.

The question which Mr. Murphy has suggested no doubt here arises: Must not this preponderance of westerly winds affect the rotation of the earth? I have throughout maintained the existence of this preponderance solely by geographical proof, and conceiving that the evidence is conclusive, whilst no meteorological theory points to any explanation of it, I am compelled to attribute it to the action of some force external to the earth; possibly, as I have endeavoured to show, to the attraction of the sun, moon, and other heavenly bodies; possibly also to some other force, magnetic or meteoric, of whose action we have as yet no knowledge or understanding: but supposing, as I do, that the force which produces this motion is external to the earth, it is impossible to avoid the conclusion that it does tend to increase the earth's velocity of rotation. On the other hand, there are forces, admitted by all naturalists, in constant action, which tend to decrease the velocity of rotation; and a certain amount of wonder that the decrease so caused is so small as observation proves it to be is implied, rather than expressed, in our most valuable works on Natural Philosophy. If it is impossible in the present state of our knowledge to show exactly what such decrease is and ought to be, it is certainly impossible to say that it is not to some extent counterbalanced by a contrary tendency towards an increase, such as I have shown probably exists. At any rate, I know of nothing connected with the rotation of the earth which in any way controverts or affirms the proposition which I have put forward, based on geographical evidence only.

I had written this before seeing Mr. Murphy's second letter on the subject in NATURE for March 30, but as he has in it merely repeated his former arguments, it is unnecessary to notice it more particularly.

J. K. LAUGHTON

Royal Naval College, Portsmouth

* Jameson's Journal, quoted in the "Arcana of Science and Art" for 1828.

## *SUBMARINE TELEGRAPHS*

IT may possibly be within the memory of some persons that, about the year 1840, Sir C. Wheatstone first conceived the idea of transmitting messages under the sea, and practically carried out at that time the first submarine telegraph cable. Selecting Swansea Bay, South Wales, as the chosen spot for his experiment, the great inventor sat in an open boat, about three miles from the Mumbles Lighthouse, with the lighthouse keeper as his assistant. A conducting wire, insulated with hemp and a resinous compound, served as the electric communication between his open boat and the shore. It is from the successful results of this first crude experiment, and Wheatstone's investigations into the laws that regulate the transmission of electric currents through metallic conductors, published shortly afterwards in the Philosophical Transactions of the Royal Society of London, that our present system of the testing of submarine cables is based, and the vast system of inter-oceanic communication that connects the civilised world together, has been framed.

At the date of Wheatstone's first experiment, gutta-percha was undiscovered, and its insulating power unknown. By the employment of this gum, the electrical condition of the submarine cable, up to a certain standard, has been under ordinary circumstances rendered secure. Such being the case, and for the purpose of comparison hereafter, it is well to examine a little into the properties of this gum and that of india-rubber, another vegetable substance possessing insulating properties of the most remarkable kind, as applied to the construction of submarine cables. Gutta-percha, as is well known, is a vegetable gum, which becomes plastic and soft at a comparatively low temperature, about 100° F. Subjecting the gum to repeated cleansing processes to free it from impurities and extraneous vegetable matter, it is rendered tolerably dense and homogeneous, and in this state it is applied in successive layers or coats round the copper conducting wire as the insulating material, forming the "core" of the submarine cable, which is then termed "insulated," that is, capable to a certain extent of preventing the lateral escape of any electric current or charge which may be passed into the wire. A short investigation is now necessary to be made of some of the circumstances which take place when a wire thus insulated is submerged and subjected to the charge of an electric current. If the wire were absolutely insulated, that is, if gutta-percha were a perfect insulator offering an indefinite resistance to the passage of the current through its substance, any given quantity of electricity passed into the wire would remain there for a given time without loss, in the same way as when water is poured into a vessel, the level remains intact so long as there is no leakage. The amount of this leakage through the gutta-percha, or, in other words, its "conductive resistance," determines the insulating power of the cable. But this is not all that has to be considered ; other circumstances affecting the value of the insulation come into play. The following analogous example will explain. When a leech is allowed to crawl through a glass tube, the head and body pass out first, while the tail—long and attenuated—is slowly withdrawn. So with the passing of an electric current through an insulated conductor, a portion of the current lags sluggishly behind, absorbed, as it were, into the substance of the insulating medium, and taking time to discharge itself in proportion to the amount of the sucking up, or "inductive capacity" of the insulator, for, in this respect, both gutta-percha and india-rubber may be regarded as a sponge, the current penetrating into the pores of the substance.

Without entering further into detail regarding the laws regulating the transmission of the current, it is sufficient to remember that the speed or power of transmitting a given number of messages in a given time over any cable depends materially upon the proportionate values of the "conductive resistance" and "inductive capacity" of the insulation. Thus there is at once established a measure by which the value of all known insulating materials may be determined and compared together, that is to say, if two cables of equal length and similar construction are taken—the one insulated with gutta-percha, and the other with india-rubber (Hooper's india-rubber)—the relative value and working speed of each can be accurately determined and compared. The successful employment of india-rubber as an insulating medium for submarine cables is of more recent date, and the estimation in which it is now held for that purpose is entirely due to the beautiful process employed in its manipulation by Mr. W. Hooper, of Mitcham. It is well known that india-rubber possesses a much higher insulating power than gutta-percha ; as a gum it is also denser, more homogeneous, and infinitely more pliable and elastic than gutta-percha, while it is not affected in any considerable degree by variation of temperature—all qualities of the greatest importance as connected with submarine cable insulation.

Before entering upon a comparative statement of the insulation and speed of gutta-percha and Hooper's insulation, a short notice of the mode by which this insulating material is manipulated will be interesting, and will serve to give value to the practical data hereafter stated. The copper conductor, after being tinned, is coated with an insulation of pure india-rubber applied in the shape of a ribbon, lapped spirally round it. Next, two strips (one laid above and the other below) of india-rubber, chemically prepared to resist the action of sulphur, and called the "separator," are applied so as to completely surround the first rubber covering, as it were with a tube ; a pair of grooved die-wheels giving the contour, and at the same time regulating accurately the guage of the core. Exterior strips are then similarly applied of a compound of rubber and a small percentage of sulphur. The whole is then lapped round with water-proof felt tape, and exposed for some hours in an oven to a heat of about 383° F. By this process the three successive coatings are welded into one solid, dense, homogeneous mass, having its distinctive features preserved as regards the individual character of the several layers. Thus the heat, in driving off the sulphur from the outside coating, has converted that envelope into an indestructible vulcanised rubber jacket. The second layer, or "separator," has intercepted the passing of the sulphur by reason of its chemical properties, while at the same time it has allowed an infinitesimal trace of the sulphur to combine with the internal coating of pure rubber round the conducting wire, sufficient to change its character into an indestructible and non-liquifying material, without its becoming in any way vulcanised. It is by this beautiful chemical affinity between the several layers, each performing its special part towards the production of one individual whole, that the "Hooper insulation" has succeeded in establishing the durability of the preparation, the comparative value of which, as compared with that of gutta-percha, will now be given.

First as regards temperature—it has been already stated that gutta-percha became plastic at about 100° F. At this temperature it loses also almost entirely its insulating properties ; that is to say, if at a temperature of 32° F. the insulation of gutta-percha is taken as representing 100, at 75° it is reduced to 5·51, or little more than a twentieth part, while at the increased temperature of 100°, its insulating power has further decreased to 1·43, or about one seventieth part. Gutta-percha as an insulator is therefore unsuited for hot climates, or any exposed position where the temperature rises above 70°. Taking now Hooper's india-rubber insulation at 32° F. to be the same, 100, at 75° we find its insulation to be 24·50, or about one-fourth part, while at 100° it is 10·60, or about one-tenth part. Thus at the ordinary temperature of 75°, Hooper's core establishes its superior insulating properties under temperature in the proportion of four to one. The "inductive capacity" of Hooper's core, from its superior density, is only about two-thirds that of gutta-percha, while its insulation or resistance of the dielectric is fully twenty times greater than that of gutta-percha core, as exemplified in the tests given of some of the best known cables now at work.

The following is a list of some of the more important cables insulated with Hooper's core laid up to the present time :—

| | | |
|---|---|---|
| 1. Cable crossing rivers in India, laid in 1865, | length | 46 nauts. |
| 2. Ceylon Cable, India, laid in 1866 . . . | ,, | 35 ,, |
| 3. India Cable . . . . . . . | ,, | 40 ,, |
| 4. Persian Gulf Cable . . . . . | ,, | 500 ,, |
| 5. Danish-English Cable . . . . . | ,, | 363 ,, |
| 6. Scotch-Norwegian Cable . . . . | ,, | 247 ,, |
| 7. Danish-Norwegian Cable. . . . . | ,, | 73 ,, |
| 8. Orkney and Shetland Islands Cable . . | ,, | 103 ,, |

| | | |
|---|---|---|
| 9. Pentlands Cable | length | 11 nauts. |
| 10. Scilly Islands Cable | ,, | 27 ,, |
| 11. Swedish-Russian Cable | ,, | 103 ,, |
| 12. Moen Bornholm Cable | ,, | 80 ,, |
| 13. Hong-Kong-Shanghae Cable | ,, | 1200 ,, |
| 14. Shanghae-Possiette Cable | ,, | 1100 ,, |

These two latter cables have recently been completed, and the Shanghae-Possiette cable is now in course of submergence; the Hong-Kong-Shanghae cable was successfully laid last month. These lines give a total distance of over 3,978 nautical miles of submarine cable with Hooper's indiarubber insulation. The following observations as regards the electrical conditions of these cables as compared with well-known gutta-percha insulated cables is remarkable. The electrical tests of well-known cables with both the gutta-percha and the Hooper core are taken at the temperature of 75° Fahr., and in terms of British Association (B.A.) units, the standard measure now most generally adopted in England for comparison:

| | | | |
|---|---|---|---|
| | | Gutta-percha. | |
| England and Hanover Cable, laid | 1866 | 239 million B.A. units | |
| Persian Gulf Cable | ,, 1864 | 190 ,, | ,, |
| Atlantic Cable | ,, 1865 | 349 ,, | ,, |
| Atlantic Cable | ,, 1866 | 342 ,, | ,, |
| Placentia Bay Cable | ,, 1866 | 455 ,, | ,, |
| Cuba and Florida Cable | ,, 1867 | 464 ,, | ,, |
| | | Hooper Core. | |
| Ceylon Cable (Hooper's Core) | ,, 1865 | 7949 ,, | ,, |
| India Cable | ,, 1865 | 8064 ,, | ,, |
| India Cable | ,, 1866 | 8526 ,, | ,, |
| Danish-English Cable | ,, 1868 | 8123 ,, | ,, |
| Scotch-Norwegian Cable | ,, 1869 | 7923 ,, | ,, |
| Scilly Islands Cable | ,, 1870 | 7819 ,, | ,, |

With such results, it is not to be wondered at that the relative speed of two cables of similar length and construction, the one employing a gutta-percha core and the other a Hooper core, should be found greatly in favour of the latter, in the proportion of 130 to 100; that is to say, in any given time the Hooper core, from its superior insulating properties, will transmit thirty per cent. more words than a gutta-percha core, a most important circumstance when it is considered that the earnings or dividend upon each cable is dependent upon the work it can perform in a given period. As regards the apparatus employed for transmitting the currents through submarine conductors, the "Wheatstone" automatic recording system is the most successful. By this apparatus an average speed of over thirty words a minute is regularly maintained upon the Danish-English cable, a distance of 363 nautical miles, exclusive of a further land circuit of over 140 miles, making a total distance of about 500 miles. This speed must be compared with that of seventeen words per minute, the highest result recorded over the same circuit by the most improved Morse system. From the results of the "Wheatstone" apparatus working over this circuit since September 1868, it appears that to obtain maximum speed, the currents through a submarine cable require to be transmitted of equal duration, at equal intervals, in alternate directions, and the line discharged to earth between each successive reversal or current to neutralise the charge, all of which conditions are fulfilled in the "Wheatstone" Automatic Jacquard arrangement, which can only be compared to a loom weaving the currents into the line, the sequence of the currents representing the pattern on the cloth. This apparatus is now organised as the transmitting and recording register upon the vast system of submarine circuits belonging to the Great Northern Telegraph Company, and the extensions from Possiette Bay (Russian-Chinese frontier) to Nagasaki, Shanghae, and Hong-Kong. The subject of high speed transmission through insulated conductors, both by land and sea, is one which demands special attention, now that the telegraph is daily encroaching upon the postal service, a service in which both speed and accuracy are more than ever demanded by the public.

NATH. J. HOLMES

## PFLÜGER'S NERVE-ENDINGS IN GLANDS

IN his "Archiv für die Gesammte Physiologie" (Bonn, 1871), E. Pflüger gives a short and summary answer to those many observers who have thrown doubt on the accuracy of his remarkable discoveries as to the continuity of nerves with the secreting cells of the salivary glands and liver. Pflüger's opponents in this matter have been Mayer, Hering, Krause, Henle, and Schweigger-Seidel. The objections which have been made are divided by him into three heads. 1st. It was said that the nerves he had seen were capillary vessels. 2nd. That they were threads of mucus. 3rd. They were disintegrated fat. These objections are successively shown to be groundless, and Pflüger stoutly maintains his original position. What is far more important in this short paper than these answers to objections is that the professor at length publishes an account of some of his methods as to which he has so long left every one in the dark. They are certain to be interesting to some of our readers. *Salivary glands.* A fresh submaxillary gland from the ox must be taken, and very fine sections made; these must be at once teased out in perosmic acid sp. gr. 1003, and covered with a thin glass in a shallow cell. A great many preparations should be made, and the best picked out. They will be sufficiently stained in 24 hours. As the water dries up it may be replaced by glycerine. *Liver.* A great number of very fine sections must be made from the fresh liver of a dog or pig. These sections must be placed 10 or 12 together in watch-glasses filled with Beale's carmine solution, and thus kept in a moist chamber 14 days. The sections must then be taken out, washed one by one in a drop of perosmic acid, sp. gr. 1003, transported to a fresh drop of the same on a slide, and carefully teased out, covered, and examined.

## NOTES

ST. BARTHOLOMEW'S HOSPITAL has, we learn from the *British Medical Journal*, sustained a great loss in the resignation by Mr. Paget of his active duties as Surgeon to the Hospital. Mr. Paget will, of course, receive the appointment of Consulting Surgeon to the Institution which he has served long and faithfully, and on which he has conferred lustre.

THE following excursions have been arranged by the Geologists' Association to take place in May:—To Oxford on Friday, 12th May. On arriving at Oxford the New University Museum will be visited. Subsequently the party, accompanied by the President, Prof. Phillips, and Prof. Morris, will walk to Shotover Hill, where the Middle and Upper Oolites are well exposed. To Grays, Essex, on Saturday, 20th May. Exposures of the Mammaliferous beds of the Thames Valley, and afterwards sections of the Upper Chalk will be visited, under the guidance of Prof. Morris. A four days' excursion to Yeovil, Weymouth, and Portland is proposed for Whitsuntide. Particulars of arrangements will be duly announced.

THE Edinburgh Naturalists' Field Club, which has since its formation carried on active operation only from April to July inclusive, held its adjourned annual meeting and conversazione on Saturday, the 22nd April, when Mr. Robert Scot-Skirving, the president, delivered an introductory address, enlarging mainly on entomology as a fit summer field study. The business meeting was held in November last, when, in addition to the present president and committee, Prof. Liston was elected vice-president, and Mr. Andrew Taylor honorary secretary and treasurer.

THE following is the programme of the lectures on the Experimental and Natural Sciences in Trinity Term, in Trinity College, Dublin. Mineralogy, 11 A.M., on Mondays, Wednesdays, and Fridays. Demonstrations in Organic Chemistry, 12, Mondays, Wednesdays, and Fridays. Magnetism, 2 P.M., Mondays, Wednesdays, and Fridays. Comparative Anatomy, 11 A.M., Mondays, Wednesdays, and Fridays. Demonstrations in Botany, 11 A.M., Tuesdays, Thursdays, and Saturdays. Applied Geology, 1 P.M., Tuesdays, Thursdays, and Saturdays.

THE grace for allowing French and German as an alternative for Greek, was submitted to the Senate of the University of Cambridge on Thursday last, and rejected by a majority of three only. The subject will doubtless be reopened, and probably some slight modification of the original scheme will ultimately be accepted.

THE following gentlemen have been elected, by the Senate of the University of London, Examiners in Science and Medicine for the ensuing year :—Logic and Moral Philosophy : Prof. G. Croom Robertson and Rev. John Venn. Political Economy : Prof. W. Stanley Jevons and Prof. T. E. Cliff Leslie. Mathematics and Natural Philosophy : Prof. H. J. S. Smith, F.R.S., and Prof. Sylvester, F.R.S. Experimental Philosophy : Prof. W. G. Adams and Prof. G. Carey Foster, F.R.S. Chemistry : Henry Debus, F.R.S., and Prof. Odling, F.R.S. Botany and Vegetable Physiology : Dr. J. D. Hooker, F.R.S., and Dr. Thos. Thomson, F.R.S. Geology and Palæontology : Prof. Duncan, F.R.S., and Prof. Morris. Practice of Medicine : John S. Bristowe, M.D., and Prof. J. Russell Reynolds, M.D., F.R.S. Surgery : Prof. John Birkett and Prof. John Marshall, F.R.S. Anatomy : Prof. Geo. Viner Ellis and Prof. John Wood. Physiology, Comparative Anatomy, and Zoology : Prof. M. Foster, M.D., and H. Power. Obstetric Medicine : Rob. Barnes, M.D., and Prof. W. M. Graily Hewitt, M.D. Materia Medica and Pharmaceutical Chemistry : Thos. R. Fraser, M.D., and Prof. A. Baring Garrod, M.D., F.R.S. Forensic Medicine : E. Headlam Greenhow, M.D., F.R.S., and Thos. Stevenson, M.D.

MR. C. T. CLOUGH, of Rugby School, has been elected to an exhibition at St. John's College, Cambridge, of 50*l.* per annum, for proficiency in Natural Science. There were ten candidates.

SCIENCE appears to have penetrated even into the recesses of Christ's Hospital. Since October 1869, there has been a Chemistry class of about fifty boys, in connection with St. Bartholomew's Hospital. The work done is both practical and theoretical ; at the first, Dr. Matthiessen was the lecturer, and at his lamented death, Dr. H. E. Armstrong. Since Christmas the class has been under the care of Dr. W. T. Russell, F.C.S. For some weeks past, Prof. Tennant has been lecturing on Mineralogy, and next week commences on Geology. This class is very well attended. There has been established a permanent class for Natural Philosophy, under the care of Mr. James Noon, B.A. We believe also that those boys who are intended for the Navy are instructed in theoretical and practical Astronomy. There have been wishes expressed for a Museum, and numerous specimens are constantly brought to Prof. Tennant for information. It is much to be wished also that some sort of a Natural History Society might be established, notwithstanding the city-site of the Hospital.

WE continue to receive intelligence from the French Academy, and are in a position to give the full list of members who were present at the sitting on the 21st April, eighteen in number, viz., three astronomers, Yvon Villareau, Mathieu, and Langier ; one mathematician, Chasles ; one physicist, Jamin, three chemists, Chevreul, Payer, Peligot ; one mechanician, Ameral Paris ; and the others medical men or naturalists, Milne-Edwards, Blanchard, Robin, Trécul, Bienaymè, Duchartre, and Quatrefages. M. Egger, of the Academy of Inscriptions, sat with his colleagues, and M. Simon Newcomb, the American astronomer, sat at the place allotted to foreign learned men.

THE seventh part of the illustrated work on the butterflies of North America, by Mr. Wm. H. Edwards, has just been published, containing numerous well-engraved and coloured plates of butterflies.

THE Commune has its own balloons, twelve in number, but they are kept apart for the private use of members when the final exit shall take place. One was sent up into the air, as it was said in the political newspapers for carrying away the masonic proclamation, but it was a little one without aëronaut.

THE *Gardener's Chronicle* for last Saturday prints an interesting letter from Dr. Hooker, dated Tetuan, April 12. In the journey from Tangier to Tetuan, Dr. Hooker notices that the general features of the flora of the low grounds and moderate hills in that part of North Morocco coincide with those of South-western Spain. Whole tracts are covered with masses of broom, so that the hills precisely resemble those of Scotland or Jersey. The previous day a guard had been obtained in order to ascend Beni-Hosmar, which mountain had only been visited previously by one botanist, Mr. Webb, some forty years since. The party ascended to 3,500 feet, and obtained a superb view across the Mediterranean to the Spanish coast, and south to the snowy crest of Beni-Hassan. It is a splendid rugged mass of limestone peaks, separated by very steep narrow-floored valleys, the flanks of which are crested with rifted white precipices. The whole is clothed with stunted shrubs up to 3,000 feet. They found some rare, and some probably new plants, but at a height of 3,400 feet no signs of a sub-alpine flora. The party did not succeed in reaching the summit.

THE principal object of interest at the *soirée* of the Linnean Society on the 26th ult., was again Mr. Wilson Saunders's collection of mimetic plants, which was even more remarkable than last year. The following is a list of the pairs exhibited :

| | |
|---|---|
| Olea europœa | Oleaceæ |
| Swammerdamia antennata | Compositæ |
| Anemone coronaria | Ranunculaceæ |
| Pelargonium triste | Geraniaceæ |
| Osmanthus heterophyllus | Oleaceæ |
| Ilex aquifolium *var* | Aquifoliaceæ |
| Gnaphalium orientale | Compositæ |
| Lavendula lanata | Labiatæ |
| Iris pulchella | Iridaceæ |
| Dicrypta iridoides | Orchidaceæ |
| Pothos argyrea | Araceæ |
| Peperomia arifolia | Piperaceæ |
| Adonis autumnalis | Ranunculaceæ |
| Pyrethrum inodorum | Compositæ |
| Heterotropa asaroides | Aristolochiaceæ |
| Cyclamen persicum *var* | Primulaceæ |
| Oxalis Plumieri | Oxalidaceæ |
| Crotalaria laburnifolia | Leguminosæ |
| Gentiana lutea | Gentianaceæ |
| Veratrum viride | Melanthaceæ |
| Gymnostachyum Verschaffeltii | Acanthaceæ |
| Echites rubro-venosa | Apocynaceæ |
| Grevillea *sp.* | Proteaceæ |
| Acacia *sp.* | Leguminosæ |
| Rosa *sp.* | Rosaceæ |
| Xanthoxylon *sp* | Xanthoxylaceæ |
| Euphorbia mammillaria | Euphorbiaceæ |
| Apteranthes Gaponiana | Asclepiadaceæ |
| Dancus Carota | Umbelliferæ |
| Pelargonium rutæfolium | Geraniaceæ |

A SERIES of scientific lectures, in connection with the School of Science and Art, has been for some time in contemplation at Taunton. It has recently commenced with a course of botanical lectures, by the Rev. W. Tuckwell, headmaster of the College School, which attract a large and diligent audience, consisting both of artisans and amateurs.

AT the annual dinner of the Institution of Civil Engineers held on April 22, Prof. Huxley, in responding to the toast of the learned societies in this country, gave the company some very sound advice as to the duty of the body of civil engineers in enforcing upon the public mind the truth that there can be no technical education of any value or soundness which is not based on a thorough preliminary training in abstract or theoretical practical science.

MR. JOHN GIBBS, of the Essex and Chelmsford Museum, publishes, at the price of a shilling, "A First Catechism of Botany," which has received the sanction of the Committee of Selection for the International Exhibition. Although the catechismal form always seems to us a needlessly cumbrous and circumlocutory one, for those who think otherwise a large amount of useful elementary information will be found in this little publication.

WE learn from Trübner's *American and Oriental Literary Record* that the American Ethnological Society was permanently reorganised under the name of "The Anthropological Institute of New York," on the 9th of March, and the following officers were elected:—E. Geo. Squier, president; J. C. Nott and Geo. Gibbs, vice-presidents; J. G. Shea, J. K. Merrill, E. H. Davis, C. C. Jones, jun., and W. H. Thomson, executive committee; A. J. Cotteral, treasurer; Charles Raw, foreign corresponding secretary; H. T. Drowne, domestic corresponding secretary; H. R. Stiles, recording secretary; and Geo. H. Moore, custodian. The objects of the institute are declared to be:—1. The study of man in all his varieties, and under all his aspects and relations. 2. Its special object the study of the history, conditions, and relations of the aboriginal inhabitants of America, and the phenomena resulting from the contact of the various races and families of men on this continent, before and since the discovery. 3. The physical characteristics, religious conceptions, and systems of men; their mythology and traditions; their social, civil, and political organisations and institutions; their language, literature, arts, and monuments; their mode of life and their customs are specifically within the objects of the Institute. 4. The collection of manuscripts, books, and relics illustrating these several subjects; the stimulation and encouragement of inquiry and research, particularly in unexplored American fields; and by means of such publications as may be deemed proper, to utilise the results of its investigations and efforts for the benefit of science and of mankind. 5. It recognises the widest range of discussion, and a complete tolerance of individual opinions on all subjects within the scope of the Institute's objects. The Institute proposes to publish a series of Memoirs, and a Journal.

THE attention of astronomers throughout the world is directed toward the approaching transit of Venus, to occur on the 18th of December, 1874, and it is hoped, we learn from *Harper's Weekly*, that the United States Congress, with the same liberality that induced it to make an appropriation for the observation of the solar eclipse of December last, and for the polar exploration under Captain Hall, will also, at the proper time, advance the funds necessary for the research in this case. Professor Hall, of the Washington Observatory, in a late communication to the *Journal of Science*, expresses the hope that a concert of action will be settled upon by American astronomers, in order that they may not be behind their European *confrères* in the attempt to secure satisfactory results. A committee has been appointed by the National Academy of Sciences to take into consideration a general plan of operations, and it is expected that a report will be made on the subject at the approaching meeting in Washington city.

THE annual report of Professor Cooke, State Geologist of New Jersey, for 1870, has just been published; and although less in bulk than some of its predecessors, it contains some important information in regard to fertilisers used in the State, the marshes and tracts of land subject to protracted freshets, the soils, the iron and zinc ores, and other miscellaneous topics. The subject of drainage has attracted Professor Cooke's especial attention, on account of the vast tracts of land in the eastern portion of the State now either regularly overflowed at certain periods of the tide, or liable to freshets or inundations. In order more properly to qualify himself for this inquiry, Professor Cooke paid an extended visit to the drained lands of Holland and England, the results of which he presents in his report.

M. LONGET, the celebrated physiologist, member of the French Institute and of the French Academy of Medicine, died at the age of sixty-eight, at Bordeaux, a few days since. M. Longet is the author of works on the nervous system, which explain many of his own discoveries. His death was sudden, and was referred by his friends to the horror with which he was stricken when hearing the sad news from Paris.

IN the forthcoming number of the *American Journal of Science* will be found an article, by Professor Marsh, upon some new serpents of the Tertiary deposits of Wyoming. It will be remembered that in a previous notice of Professor Marsh's discoveries in the Rocky Mountains, we called attention to the difference observed by him between the contents of the Tertiary beds in the vicinity of Fort Bridger and those of the Mauvaises Terres of the Upper Missouri, the former being especially characterised, as compared with the latter, by the presence of reptiles in great variety. Among these are many terrestrial species, including several kinds of land lizards; and among the forms generally serpents appear to be quite predominant. Of the latter, Professor Marsh has already determined the existence of five new species, belonging to three new genera; and others will probably be yet brought to light.

AT the present day, when the columns of our newspapers teem with advertisements of various preparations for promoting the growth or changing the colour of the hair, the following account of the results of the use of a preparation of boxwood for that purpose may be of interest. Boxwood, according to the old herbalists, was used from a remote period to render the hair auburn; and we are told by Phillips that a young woman in Lower Silesia, whose hair had fallen off after a severe attack of dysentery, was advised to wash her head with a decoction of boxwood, in order to induce it to grow again. This she did; and "hair of a chestnut colour grew on her head, as she was told it would do; but, having used no precaution to secure her face and neck from the lotion, they became covered with red hair to such a degree that she seemed but little different from an ape or a monkey!"

MR. JAMES BOYD, of Panama, published some time ago in the *Panama Star and Herald*, under the head of "The Migration of Butterflies across the Isthmus," an account of the phenomenon of the migration in one direction of the *Urania Leilus*. This being republished in England has led to some correspondence with Mr. Boyd, particularly from a naturalist resident at Liverpool. This gentleman states that in January 1845, he observed the same habit of the Urania in the Island of Caripi, one of those near Para, in the Brazils. From an early hour in the morning until nearly dark these insects passed along the shore in amazing numbers, but most numerously in the evening. It was very seldom that one was seen in the opposite direction. The main course was from west to east. He also saw it at Pernambuco, at Rio Janeiro, and in the Southern States of America, but nowhere so abundant as on the Amazons. The Urania is scarcely a butterfly; but between the day and night butterflies, something between a skipper and a hawk moth. By Latreille they were called Hespero-Sphyngidæ. The larvæ and pupæ are supposed not to have been adequately examined. The Liverpool naturalist could not identify them, and as yet they have not been able to find them at Panama. In Central America the Urania is found as far north as Guatemala. Mr. Darwin observed a butterfly of similar habits, the *Papilio feronia,* which frequents orange groves.

## THE ROYAL SOCIETY'S LIST FOR 1871

THE following fifteen have been selected by the Council of the Royal Society out of the fifty candidates, and recommended to the Fellows for election:—*William Henry Besant*, M.A., Mathematical Lecturer at St. John's College; Senior Wrangler and First Smith's Prizeman in 1850, Moderator in 1856, Examiner in the University of London from 1860 to 1865; author of Treatises on "Hydromechanics and the Theory of Sound," 2nd ed. 1867; "Elementary Hydrostatics," 2nd ed. 1867; "Geometrical Conic Sections," 1869; "Roulettes and Glissettes," 1870. *William Budd*, M.D. (Edin.), physician, author of various medical papers, especially relating to contagious diseases. *George W. Callender*, F.R.C.S., lecturer on Anatomy at St. Bartholomew's Hospital School, and Assistant Surgeon to Bartholomew's Hospital; author of Anatomical papers. *William Carruthers*, F.L.S., F.G.S., keeper of the Botanical Department, British Museum; author of "Fossil Cycadean Stems from the Secondary Rocks of Britain;" "On the Structure and Affinities of Sigillaria and Allied Genera;" "The Cryptogamic Forests of the Coal Period;" "On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-measures;" "Revision of the British Graptolites," &c. *Robert Etheridge*, F.R.S.E., F.G.S., Palæontologist to H.M. Geological Survey of Great Britain; Demonstrator on Palæontology, Royal School of Mines; author of numerous geological papers. *Frederick Guthrie*, B.A., F.R.S.E., F.C.S., Professor of Physics in the Royal School of Mines; author of various papers on Chemistry and Physics. *Captain John Herschel*, R.E., of the Great Trigonometrical Survey of India. *Captain Alexander Moncrieff*, Militia Artillery, C.E., inventor of the Moncrieff gun-carriage, and author of the Moncrieff system of defence. *Richard Quain*, M.D. (Lond.), Fellow and late Censor of the Royal College of Physicians; author of a paper "On Fatty Degeneration of the Heart," which has exerted a marked influence on certain branches of Pathological Science; and of numerous communications published in the Transactions of the Pathological Society, of which Society he was President (1869-70). *Carl Schorlemmer*, Senior Assistant in Owens College Laboratory, Manchester; author of a series of papers on the Constitution of the Paraffins, chiefly published in the Proceedings of the Society since 1862. *Edward Thomas*, Treas. R.A.S., author of numerous papers on Indian Coins and Gems. *Edward Burnet Tylor*, author of "Researches into the Early History of Mankind;" "Primitive Culture;" and various memoirs on Savages and their Customs. *Cromwell Fleetwood Varley*, Civil and Telegraphic Engineer, M.I.C.E.; Consulting Electrician to the Electric and International Telegraph Company, the Atlantic Telegraph Company, la Société du Cable Transatlantique Français; author of many inventions in connection with the Electric Telegraph. *Viscount Walden*, President of the Zoological Society of London; author of various papers on Ornithology. *John Wood*, F.R.C.S., Examiner in Anatomy at the University of London; author of a number of anatomical papers published in the Phil. Trans.

## ON COLOUR VISION *

ALL vision is colour vision, for it is only by observing differences of colour that we distinguish the forms of objects. I include differences of brightness or shade among differences of colour.

It was in the Royal Institution, about the beginning of this century, that Thomas Young made the first distinct announcement of that doctrine of the vision of colours which I propose to illustrate. We may state it thus:—We are capable of feeling three different colour-sensations. Light of different kinds excites these sensations in different proportions, and it is by the different combinations of these three primary sensations that all the varieties of visible colour are produced. In this statement there is one word on which we must fix our attention. That word is, Sensation. It seems almost a truism to say that colour is a sensation; and yet Young, by honestly recognising this elementary truth, established the first consistent theory of colour. So far as I know, Thomas Young was the first who, starting from the well-known fact that there are three primary colours, sought for the explanation of this fact, not in the nature of light, but in the constitution of man. Even of those who have written on colour since the time of Young, some have supposed that they ought to study the properties of pigments, and others that they ought to analyse the rays of light. They have sought for a knowledge of colour by examining something in external nature—something out of ourselves.

Now, if the sensation which we call colour has any laws, it must be something in our own nature which determines the form of these laws; and I need not tell you that the only evidence we can obtain respecting ourselves is derived from consciousness.

The science of colour must therefore be regarded as essentially a mental science. It differs from the greater part of what is called mental science in the large use which it makes of the physical sciences, and in particular of optics and anatomy. But it gives evidence that it is a mental science by the numerous illustrations which it furnishes of various operations of the mind.

In this place we always feel on firmer ground when we are dealing with physical science. I shall therefore begin by showing how we apply the discoveries of Newton to the manipulation of light, so as to give you an opportunity of feeling for yourselves the different sensations of colour.

Before the time of Newton, white light was supposed to be of all known things the purest. When light appears coloured, it was supposed to have become contaminated by coming into contact with gross bodies. We may still think white light the emblem of purity, though Newton has taught us that its purity does not consist in simplicity.

We now form the prismatic spectrum on the screen. These are the simple colours of which white light is always made up. We can distinguish a great many hues in passing from the one end to the other; but it is when we employ powerful spectroscopes, or avail ourselves of the labours of those who have mapped out the spectrum, that we become aware of the immense multitude of different kinds of light, every one of which has been the object of special study. Every increase of the power of our instruments increases in the same proportion the number of lines visible in the spectrum.

All light, as Newton proved, is composed of these rays taken in different proportions. Objects which we call coloured when illuminated by white light, make a selection of these rays, and our eyes receive from them only a part of the light which falls on them. But if they receive only the pure rays of a single colour of the spectrum, they can appear only of that colour. If I place a disc containing alternate quadrants of red and green paper in the red rays, it appears all red, but the red quadrants brightest. If I place it in the green rays both papers appear green, but the red paper is now the darkest. This, then, is the optical explanation of the colours of bodies when illuminated with white light They separate the white light into its component parts, absorbing some and scattering others.

Here are two transparent solutions. One appears yellow, it contains bichromate of potash; the other appears blue, it contains sulphate of copper. If I transmit the light of the electric lamp through the two solutions at once, the spot on the screen appears green. By means of the spectrum we shall be able to explain this. The yellow solution cuts off the blue end of the spectrum, leaving only the red, orange, yellow, and green. The blue solution cuts off the red end, leaving only the green, blue, and violet. The only light which can get through both is the

* Lecture delivered before the Royal Institution, March 24th.

green light, as you see. In the same way most blue and yellow paints, when mixed, appear green. The light which falls on the mixture is so beaten about between the yellow particles and the blue, that the only light which survives is the green. But yellow and blue light when mixed do not make green, as you will see if we allow them to fall on the same part of the screen together.

It is a striking illustration of our mental processes that many persons have not only gone on believing, on the evidence of the mixture of pigments, that blue and yellow make green, but that they have even persuaded themselves that they could detect the separate sensations of blueness and of yellowness in the sensation of green.

We have availed ourselves hitherto of the analysis of light by coloured substances. We must now return, still under the guidance of Newton, to the prismatic spectrum. Newton not only

> Untwisted all the shining robe of day,

but showed how to put it together again. We have here a pure spectrum, but instead of catching it on a screen, we allow it to pass through a lens large enough to receive all the coloured rays. These rays proceed, according to well-known principles in optics, to form an image of the prism on a screen placed at the proper distance. This image is formed by rays of all colours, and you see the result is white. But if I stop any of the coloured rays, the image is no longer white, but coloured; and if I only let through rays of one colour, the image of the prism appears of that colour.

I have here an arrangement of slits by which I can select one, two, or three portions of the light of the spectrum, and allow them to form an image of the prism while all the rest are stopped. This gives me a perfect command of the colours of the spectrum, and I can produce on the screen every possible shade of colour by adjusting the breadth and the position of the slits through which the light passes. I can also, by interposing a lens in the passage of the light, show you a magnified image of the slits, by which you will see the different kinds of light which compose the mixture.

The colours are at present red, green, and blue, and the mixture of the three colours is, as you see, nearly white. Let us try the effect of mixing two of these colours. Red and blue form a fine purple or crimson, green and blue form a sea-green or sky-blue, red and green form a yellow.

Here again we have a fact not universally known. No painter, wishing to produce a fine yellow, mixes his red with his green. The result would be a very dirty drab colour. He is furnished by nature with brilliant yellow pigments, and he takes advantage of these. When he mixes red and green paint, the red light scattered by the red paint is robbed of nearly all its brightness by getting among particles of green, and the green light fares no better, for it is sure to fall in with particles of red paint. But when the pencil with which we paint is composed of the rays of light, the effect of two coats of colour is very different. The red and the green form a yellow of great splendour, which may be shown to be as intense as the purest yellow of the spectrum.

I have now arranged the slits to transmit the yellow of the spectrum. You see it is similar in colour to the yellow formed by mixing red and green. It differs from the mixture, however, in being strictly homogeneous in a physical point of view. The prism, as you see, does not divide it into two portions as it did the mixture. Let us now combine this yellow with the blue of the spectrum. The result is certainly not green; we may make it pink if our yellow is of a warm hue, but if we choose a greenish yellow we can produce a good white.

You have now seen the most remarkable of the combinations of colours—the others differ from them in degree, not in kind. I must now ask you to think no more of the physical arrangements by which you were enabled to see these colours, and to concentrate your attention upon the colours you saw, that is to say, on certain sensations of which you were conscious. We are here surrounded by difficulties of a kind which we do not meet with in purely physical inquiries. We can all feel these sensations, but none of us can describe them. They are not only private property, but they are incommunicable. We have names for the external objects which excite our sensations, but not for the sensations themselves.

When we look at a broad field of uniform colour, whether it is really simple or compound, we find that the sensation of colour appears to our consciousness as one and indivisible. We cannot directly recognise the elementary sensations of which it is composed, as we can distinguish the component notes of a musical chord. A colour, therefore, must be regarded as a single thing, the quality of which is capable of variation.

To bring a quality within the grasp of exact science, we must conceive it as depending on the values of one or more variable quantities, and the first step in our scientific progress is to determine the number of these variables which are necessary and sufficient to determine the quality of a colour. We do not require any elaborate experiments to prove that the quality of colour can vary in three and only in three independent ways.

One way of expressing this is by saying, with the painters, that colour may vary in hue, tint, and shade.

The finest example of a series of colours varying in hue, is the spectrum itself. A difference in hue may be illustrated by the difference between adjoining colours in the spectrum. The series of hues in the spectrum is not complete; for, in order to get purple hues, we must blend the red and the blue.

Tint may be defined as the degree of purity of a colour. Thus, bright yellow, buff, and cream-colour, form a series of colours of nearly the same hue, but varying in tint. The tints corresponding to any given hue form a series, beginning with the most pronounced colour, and ending with a perfectly neutral tint.

Shade may be defined as the greater or less defect of illumination. If we begin with any tint of any hue, we can form a gradation from that colour to black, and this gradation is a series of shades of that colour. Thus we may say that brown is a dark shade of orange.

The quality of a colour may vary in three different and independent ways. We cannot conceive of any others. In fact, if we adjust one colour to another, so as to agree in hue, in tint, and in shade, the two colours are absolutely indistinguishable. There are therefore three, and only three, ways in which a colour can vary.

I have purposely avoided introducing at this stage of our inquiry anything which may be called a scientific experiment, in order to show that we may determine the number of quantities upon which the variation of colour depends by means of our ordinary experience alone.

Here is a point in this room: if I wish to specify its position, I may do so by giving the measurements of three distances—namely, the height above the floor, the distance from the wall behind me, and the distance from the wall at my left hand.

This is only one of many ways of stating the position of a point, but it is one of the most convenient. Now, colour also depends on three things. If we call these the intensities of the three primary colour sensations, and if we are able in any way to measure these three intensities, we may consider the colour as specified by these three measurements. Hence the specification of a colour agrees with the specification of a point in the room in depending on three measurements.

Let us go a step farther, and suppose the colour sensations measured on some scale of intensity, and a point found for which the three distances, or co-ordinates, contain the same number of feet as the sensations contain degrees of intensity. Then we may say, by a useful geometrical convention, that the colour is represented to our mathematical imagination by the point so found in the room; and if there are several colours, represented by several points, the chromatic relations of the colours will be represented by the geometrical relations of the points. This method of expressing the relations of colours is a great help to the imagination. You will find these relations of colours stated in an exceedingly clear manner in Mr. Benson's "Manual of Colour," one of the very few books on colour in which the statements are founded on legitimate experiments.

There is a still more convenient method of representing the relations of colours, by means of Young's triangle of colours. It is impossible to represent on a plane piece of paper every conceivable colour, to do this requires space of three dimensions. If, however, we consider only colours of the same shade, that is, colours in which the sum of the intensities of the three sensations is the same, then the variations in tint and in hue of all such colours may be represented by points on a plane. For this purpose we must draw a plane cutting off equal lengths from the three lines representing the primary sensations. The part of this plane within the space in which we have been distributing our colours will be an equilateral triangle. The three primary colours will be at the three angles, white or gray will be in the middle, the tint or degree of purity of any colour will be expressed by its distance from the middle point, and its hue will depend on the angular position of the line which joins it with the middle point.

Thus the ideas of tint and hue can be expressed geometrically on Young's triangle. To understand what is meant by shade, we have only to suppose the illumination of the whole triangle increased or diminished, so that by means of this adjustment of illumination Young's triangle may be made to exhibit every variety of colour. If we now take any two colours in the triangle and mix them in any proportions, we shall find the resultant colour in the line joining the component colours at the point corresponding to their centre of gravity.

I have said nothing about the nature of the three primary sensations, or what particular colours they most resemble. In order to lay down on paper the relations between actual colours, it is not necessary to know what the primary colours are. We may take any three colours, provisionally, as the angles of a triangle, and determine the position of any other observed colour with respect to these, so as to form a kind of chart of colours.

Of all colours which we see, those excited by the different rays of the prismatic spectrum have the greatest scientific importance. All light consists either of some one kind of these rays, or of some combination of them. The colours of all natural bodies are compounded of the colours of the spectrum. If, therefore, we can form a chromatic chart of the spectrum, expressing the relations between the colours of its different portions, then the colours of all natural bodies will be found within a certain boundary on the chart defined by the positions of the colours of the spectrum.

But the chart of the spectrum will also help us to the knowledge of the nature of the three primary sensations. Since every sensation is essentially a positive thing, every compound colour-sensation must be within the triangle of which the primary colours are the angles. In particular, the chart of the spectrum must be entirely within Young's triangle of colours, so that if any colour in the spectrum is identical with one of the colour-sensations, the chart of the spectrum must be in the form of a line having a sharp angle at the point corresponding to this colour.

I have already shown you how we can make a mixture of any three of the colours of the spectrum, and vary the colour of the mixture by altering the intensity of any of the three components. If we place a compound colour side by side with any other colour, we can alter the compound colour till it appears exactly similar to the other. This can be done with the greatest exactness when the resultant colour is nearly white. I have therefore constructed an instrument which I may call a colour-box, for the purpose of making matches between two colours. It can only be used by one observer at a time, and it requires daylight, so I have not brought it with me to-night. It is nothing but the realisation of the construction of one of Newton's propositions in his "Lectiones Opticæ," where he shows how to take a beam of light, to separate it into its components, to deal with these components as we please by means of slits, and afterwards to unite them into a beam again. The observer looks into the box through a small slit. He sees a round field of light, consisting of two semicircles divided by a vertical diameter. The semicircle on the left consists of light which has been enfeebled by two reflexions at the surface of glass. That on the right is a mixture of colours of the spectrum, the positions and intensities of which are regulated by a system of slits.

The observer forms a judgment respecting the colours of the two semicircles. Suppose he finds the one on the right hand redder than the other, he says so, and the operator, by means of screws outside the box, alters the breadth of one of the slits, so as to make the mixture less red; and so on, till the right semicircle is made exactly of the same appearance as the left, and the line of separation becomes almost invisible.

When the operator and the observer have worked together for some time they get to understand each other, and the colours are adjusted much more rapidly than at first.

When the match is pronounced perfect, the positions of the slits, as indicated by a scale, are registered, and the breadth of each slit is carefully measured by means of a gauge. The registered result of an observation is called a "colour equation." It asserts that a mixture of three colours is, in the opinion of the observer (whose name is given), identical with a neutral tint, which we shall call Standard White. Each colour is specified by the position of the slit on the scale, which indicates its position in the spectrum, and by the breadth of the slit, which is a measure of its intensity.

In order to make a survey of the spectrum we select three points for purposes of comparison, and we call these the three Standard Colours. The standard colours are selected on the same principles as those which guide the engineer in selecting stations for a survey. They must be conspicuous and invariable, and not in the same straight line.

In the chart of the spectrum you may see the relations of the various colours of the spectrum to the three standard colours, and to each other. It is manifest that the standard green which I have chosen cannot be one of the true primary colours, for the other colours do not all lie within the triangle formed by joining them. But the chart of the spectrum may be described as consisting of two straight lines meeting in a point. This point corresponds to a green about a fifth of the distance from *b* towards F. This green has a wave-length of about 510 millionths of a millimetre by Ditscheiner's measure. This green is either the true primary green, or at least it is the nearest approach to it which we can ever see. Proceeding from this green towards the red end of the spectrum, we find the different colours lying almost exactly in a straight line. This indicates that any colour is chromatically equivalent to a mixture of any two colours on opposite sides of it and in the same straight line. The extreme red is considerably beyond the standard red, but it is in the same straight line, and therefore we might, if we had no other evidence, assume the extreme red as the true primary red. We shall see, however, that the true primary red is not exactly represented in colour by any part of the spectrum. It lies somewhat beyond the extreme red but in the same straight line.

On the blue side of primary green the colour equations are seldom so accurate. The colours, however, lie in a line which is nearly straight. I have not been able to detect any measurable chromatic difference between the extreme indigo and the violet. The colours of this end of the spectrum are represented by a number of points very close to each other. We may suppose that the primary blue is a sensation differing little from that excited by the parts of the spectrum near G.

Now, the first thing which occurs to most people about this result is that the division of the spectrum is by no means a fair one. Between the red and the green we have a series of colours apparently very different from either, and having such marked characteristics that two of them, orange and yellow, have received separate names. The colours between the green and the blue, on the other hand, have an obvious resemblance to one or both of the extreme colours, and no distinct names for these colours have ever become popularly recognised.

I do not profess to reconcile this discrepancy between ordinary and scientific experience. It only shows that it is impossible, by a mere act of introspection, to make a true analysis of our sensations. Consciousness is our only authority; but consciousness must be methodically examined in order to obtain any trustworthy results.

I have here, through the kindness of Professor Huxley, a picture of the structure upon which the light falls at the back of the eye. There is a minute structure of bodies like rods and cones or pegs, and it is conceivable that the mode in which we become aware of the shapes of things is by a consciousness which differs according to the particular rods on the ends of which the light falls, just as the pattern on the web formed by a Jacquard loom depends on the mode in which the perforated cards act on the system of movable rods in that machine. In the eye we have on the one hand light falling on this wonderful structure, and on the other hand we have the sensation of sight. We cannot compare these two things; they belong to opposite categories. The whole of Metaphysics lies like a great gulf between them. It is possible that discoveries in physiology may be made by tracing the course of the nervous disturbance

Up the fine fibres to the sentient brain;

but this would make us no wiser than we are about those colour-sensations which we can only know by feeling them ourselves. Still, though it is impossible to become acquainted with a sensation by the anatomical study of the organ with which it is connected, we may make use of the sensation as a means of investigating the anatomical structure.

A remarkable instance of this is the deduction of Helmholtz's theory of the structure of the retina from that of Young with respect to the sensation of colour. Young asserts that there are three elementary sensations of colour; Helmholtz asserts that there are three systems of nerves in the retina, each of which has for its function, when acted on by light or any other disturbing agent, to excite in us one of these three sensations.

No anatomist has hitherto been able to distinguish these three systems of nerves by microscopic observation. But it is admitted in physiology that the only way in which the sensation excited by a particular nerve can vary is by degrees of intensity. The intensity of the sensation may vary from the faintest impression up to an insupportable pain; but whatever be the exciting cause, the sensation will be the same when it reaches the same intensity. If this doctrine of the function of a nerve be admitted, it is legitimate to reason from the fact that colour may vary in three different ways, to the inference that these three modes of variation arise from the independent action of three different nerves or sets of nerves.

Some very remarkable observations on the sensation of colour have been made by M. Sigmund Exner in Prof. Helmholtz's physiological laboratory at Heidelberg. While looking at an intense light of a brilliant colour, he exposed his eye to rapid alternations of light and darkness by waving his fingers before his eyes. Under these circumstances a peculiar minute structure made its appearance in the field of view, which many of us may have casually observed. M. Exner states that the character of this structure is different according to the colour of the light employed. When red light is used a veined structure is seen; when the light is green, the field appears covered with minute black dots, and when the light is blue, spots are seen, of a larger size than the dots in the green, and of a lighter colour.

Whether these appearances present themselves to all eyes, and whether they have for their physical cause any difference in the arrangement of the nerves of the three systems in Helmholtz's theory I cannot say, but I am sure that if these systems of nerves have a real existence, no method is more likely to demonstrate their existence than that which M. Exner has followed.

### Colour Blindness

The most valuable evidence which we possess with respect to colour vision is furnished to us by the colour-blind. A considerable number of persons in every large community are unable to distinguish between certain pairs of colours which to ordinary people appear in glaring contrast. Dr. Dalton, the founder of the atomic theory of chemistry, has given us an acccount of his own case.

The true nature of this peculiarity of vision was first pointed out by Sir John Herschel in a letter written to Dalton in 1832, but not known to the world till the publication of "Dalton's Life" by Dr. Henry. The defect consists in the absence of one of the three primary sensations of colour. Colour-blind vision depends on the variable intensities of two sensations instead of three. The best description of colour-blind vision is that given by Prof. Pole in his account of his own case in the "Phil. Trans.," 1859.

In all cases which have been examined with sufficient care, the absent sensation appears to resemble that which we call red. The point P on the chart of the spectrum represents the relation of the absent sensation to the colours of the spectrum, deduced from observations with the colour box furnished by Prof. Pole.

If it were possible to exhibit the colour corresponding to this point on the chart, it would be invisible, absolutely black, to Prof. Pole. As it does not lie within the range of the colours of the spectrum we cannot exhibit it; and, in fact, colour-blind people can perceive the extreme end of the spectrum which we call red, though it appears to them much darker than to us, and does not excite in them the sensation which we call red. In the diagram of the intensities of the three sensations excited by different parts of the spectrum, the upper figure, marked P, is deduced from the observations of Prof. Pole; while the lower one, marked K, is founded on observations by a very accurate observer of the normal type.

The only difference between the two diagrams is that in the upper one the red curve is absent. The forms of the other two curves are nearly the same for both observers. We have great reason therefore to conclude that the colour sensations which Prof. Pole sees are what we call green and blue. This is the result of my calculations; but Prof. Pole agrees with every other colour-blind person whom I know in denying that green is one of his sensations. The colour-blind are always making mistakes about green things and confounding them with red. The colours they have no doubts about are certainly blue and yellow, and they persist in saying that yellow, and not green, is the colour which they are able to see.

To explain this discrepancy we must remember that colour-blind persons learn the names of colours by the same method as ourselves. They are told that the sky is blue, that grass is green, that gold is yellow, and that soldiers' coats are red. They observe difference in the colours of these objects, and they often suppose that they see the same colours as we do, only not so well. But if we look at the diagram we shall see that the brightest example of their second sensation in the spectrum is not in the green, but in the part which we call yellow, and which we teach them to call yellow. The figure of the spectrum below Prof. Pole's curves is intended to represent to ordinary eyes what a colour-blind person would see in the spectrum. I hardly dare to draw your attention to it, for if you were to think that any painted picture would enable you to see with other people's vision I should certainly have lectured in vain.

### On the Yellow Spot

Experiments on colour indicate very considerable differences between the vision of different persons, all of whom are of the ordinary type. A colour, for instance, which one person on comparing it with white will pronounce pinkish, another person will pronounce greenish. This difference, however, does not arise from any diversity in the nature of the colour sensations in different persons. It is exactly of the same kind as would be observed if one of the persons wore yellow spectacles. In fact, most of us have near the middle of the retina a yellow spot through which the rays must pass before they reach the sensitive organ: this spot appears yellow because it absorbs the rays near the line F, which are of a greenish-blue colour. Some of us have this spot strongly developed. My own observations of the spectrum near the line F are of very little value on this account. I am indebted to Professor Stokes for the knowledge of a method by which any one may see whether he has this yellow spot. It consists in looking at a white object through a solution of chloride of chromium, or at a screen on which light which has passed through this solution is thrown. This light is a mixture of red light with the light which is so strongly absorbed by the yellow spot. When it falls on the ordinary surface of the retina it is of a neutral tint, but when it falls on the yellow spot only the red light reaches the optic nerve, and we see a red spot floating like a rosy cloud over the illuminated field.

Very few persons are unable to detect the yellow spot in this way. The observer K, whose colour equations have been used in preparing the chart of the spectrum, is one of the very few who do not see everything as if through yellow spectacles. As for myself, the position of white light in the chart of the spectrum is on the yellow side of true white even when I use the outer parts of the retina; but as soon as I look direct at it, it becomes much yellower, as is shown by the point W C. It is a curious fact that we do not see this yellow spot on every occasion, and that we do not think white objects yellow. But if we wear spectacles of any colour for some time, or if we live in a room lighted by windows all of one colour, we soon come to recognise white paper as white. This shows that it is only when some alteration takes place in our sensations that we are conscious of their quality.

There are several interesting facts about the colour sensation which I can only mention briefly. One is that the extreme parts of the retina are nearly insensible to red. If you hold a red flower and a blue flower in your hand as far back as you can see your hand, you will lose sight of the red flower, while you still see the blue one. Another is, that when the light is diminished red objects become darkened more in proportion than blue ones. The third is, that a kind of colour blindness in which blue is the absent sensation can be produced artificially by taking doses of santonine. This kind of colour blindness is described by Dr. Edmund Rose, of Berlin. It is only temporary, and does not appear to be followed by any more serious consequences than headaches. I must ask your pardon for not having undergone a course of this medicine, even for the sake of becoming able to give you information at first hand about colour-blindness.

J. Clerk Maxwell

## *SCIENTIFIC SERIALS*

The *Quarterly Journal of Science* for April commences with a very interesting account, by Dr. Hofmann, of the early days of the Royal College of Chemistry, under the title of "A Page of Scientific History." After tracing the influence of Liebig's school at Giessen on the progress of chemical science in this country, and the choice of himself, at the recommendation of Liebig, as the professor at the laboratory which it was determined to establish in London, Dr. Hofmann proceeds to a narrative of the difficulties experienced by the new school in the deficiency of the money received from the fees of students to meet the necessary expenses as well as the debt incurred by the outlay for building. At this stage the college narrowly escaped the entire abandonment of its primary object, the advancement of science by means of practical instruction and original researches, to sink into a mere commercial undertaking for conducting analyses. To the influence of Sir James Clark, one of the earliest friends of the College, was mainly due the ultimate success of the efforts of the Council to induce the Government to adopt the College as the chemical department of the Museum of Practical Geology; since which period its career of usefulness has been unchecked.—Dr. A. E. Sansom follows with an article on "The Theory of Atmospheric Germs," in which he records the investigations on this subject which have been conducted to the present time, especially those of Hallier and Bastian; and sums up adversely to the theory of abiogenesis.—Mr. Mungo Ponton, in his short paper on Molecules, Ultimates, Atoms, and Waves, suggests the use of the term "molecule" to denote the particles of chemical compounds; "ultimate," those of chemical elements; and "atom," the assumed constituents of those ultimates, themselves incapable of further analysis.—Prof. Piazzi Smyth occupies no less than thirty-eight pages with the conclusion of his article on "The Great Pyramid of Egypt from a modern scientific Point of View."—Sir William Fairbairn has some very practical remarks on Steam Boiler Legislation, in which he details the failure of voluntary associations for the purpose of diminishing the loss of life and property occasioned by the use of defective boilers, and advocates the enforced legal testing of boilers by competent authorities, maintaining that it is clearly the duty of the Government to interfere on behalf of those whose lives are jeopardised, and to enact that no boiler shall be worked unless periodically examined and certified.—The last article is an account of the Eclipse of last December, by Mr. R. A. Proctor. Notices of books and a record of the progress of science in the departments of light, heat, electricity, meteorology, mineralogy, mining, metallurgy, engineering, geology and palæontology, and botany, fill up a very good number.

THE numbers of the *American Naturalist* for March and April contain some good articles. The Polarity of the Compass Plant (*Silphium laciniatum*) is a subject which has recently attracted attention, and Mr. W. F. Whitney's short article under this title sums up what is at present known about its causes.—Mr. J. A. Allen's paper in a previous number on "The Flora of the Prairies" is followed by one on "The Fauna of the Prairies."—Dr. G. H. Perkins describes some interesting relics of the Indians of Vermont, illustrated with woodcuts.—Mr. F. W. Vogel has an article on the Principles of Bee Breeding.—Mr. E. L. Greene gives an account of the Spring Flowers of Colorado.—Mr. W. Wood has a valuable article on the Game Falcons of New England; and Dr. A. S. Packard, jun., one on Bristle-tails and Spring-tails, the Lepismas and Poduras, illustrated by plates, and containing a very full account of this interesting family. In both numbers are also reviews of recent works on natural history, and many interesting paragraphs of intelligence under the heads of botany, zoology, geology, anthropology, and microscopy, original or compiled, from American and foreign sources.

## SOCIETIES AND ACADEMIES

### LONDON

Royal Society, April 27.—"On the Increase of Electrical Resistance in Conductors with rise of Temperature, and its application to the Measure of Ordinary and Furnace Temperatures; also on a simple Method of measuring Electrical Resistances." By C. W. Siemens, F.R.S., D.C.L.

The first part of this paper treats of the question of the ratio of increase of resistance in metallic conductors with increase of temperature.

The investigations of Arndtson, Dr. Werner Siemens, and Dr. Matthiessen are limited to the range of temperatures between the freezing and boiling-points of water, and do not comprise platinum, which is the most valuable method for constructing pyrometric instruments.

Several series of observations are given on different metals, including platinum, copper, and iron, ranging from the freezing-point to 350° Cent.; another set of experiments being also given, extending the observations to 1000° Cent. These results are planned on a diagram, showing a ratio of increase which does not agree either with the former assumption of a uniform progression, or with Dr. Matthiessen's formula, except between the narrow limits of his actual observations, but which conforms itself to a parabolic ratio, modified by two other coefficients, representing linear expansion and an ultimate minimum resistance.

In assuming a dynamical law, according to which the electrical resistance of a conductor increases according to the velocity with which the atoms are moved by heat, a parabolic ratio of increase of resistance with increase of temperature follows; and in adding to this the coefficients just mentioned, the resistance $r$ for any temperature is expressed by the general formula,

$$r = \alpha T^{\frac{1}{2}} + \beta T + \gamma,$$

which is found to agree very closely both with the experimental data at low temperatures supplied by Dr. Matthiessen, and with the author's experimental results, ranging up to 1000° Cent. He admits, however, that further researches will be necessary to prove the applicability of the law of increase expressed by this formula to conductors generally.

In the second part of this paper it is shown that, in taking advantage of the circumstance that the electrical resistance of a metallic conductor increases with an increase of temperature, an instrument may be devised for measuring with great accuracy the temperature at distant or inaccessible places, including the interior of furnaces, where metallurgical or other smelting operations are carried on.

In measuring temperatures not exceeding 100° Cent., the instrument is so arranged that two similar coils are connected by a light cable containing three insulated wires. One of these coils, "the thermometer-coil," being carefully protected against moisture, may be lowered into the sea, or buried in the ground, or fixed at any elevated or inaccessible place whose temperature has to be recorded from time to time; while the other, or "comparison-coil," is plunged into a test-bath, whose temperature is raised or lowered by the addition of hot or cold water, or of refrigerated solutions, until an electrical balance is established between the resistances of the two coils, as indicated by a galvanoscope, or by a differential voltameter, described in the second paper, which balance implies an identity of temperature at the two coils. The temperature of the test-solution is thereupon measured by means of a delicate mercury thermometer, which at the same time tells the temperature at the distant place.

By another arrangement the comparison-coil is dispensed with, and the resistance of the thermometer-coil, which is a known quantity at zero temperature, is measured by a differential voltameter, which forms the subject of the second paper; and the temperature corresponding to the indications of the instrument is found in a table, prepared for this purpose, in order to save all calculation.

In measuring furnace temperatures the platinum-wire constituting the pyrometer is wound upon a small cylinder of porcelain contained in a closed tube of iron or platinum, which is exposed to the heat to be measured. If the heat does not exceed a full red heat, or, say, 1000° Cent., the protected wire may be left permanently in the stove or furnace, whose temperature has to be recorded from time to time; but in measuring temperatures exceeding 1000° Cent., the tube is only exposed during a measured interval of, say, three minutes, to the heat, which time suffices for the thin protecting casing and the wire immediately exposed to its heated sides, to acquire within a determinable limit the temperature to be measured, but is not sufficient to soften the porcelain cylinder upon which the wire is wound. In this way temperatures exceeding the welding-point of iron, and approaching the melting-point of platinum, can be measured by the same instrument by which slight variations at ordinary temperatures are told. A thermometric scale is thus obtained embracing without a break the entire range.

The leading wires between the thermometric coil and the measuring instrument, which may be under certain circumstances several miles in length, would exercise a considerable disturbing influence if this were not eliminated by means of the third leading wire before mentioned, which is common to both branches of the measuring instrument.

Another source of error in the electrical pyrometer would arise through the porcelain cylinder upon which the wire is wound becoming conductive at very elevated temperatures; but it is shown that the error arising through this source is not of serious import.

The third part of the paper is descriptive of an instrument for measuring electrical resistance without the aid of a magnetic needle or of resistance scales. It consists of two voltameter tubes fixed upon graduated scales, which are so connected that the current of a battery is divided between them, with one branch including a known and permanent resistance, and the unknown resistance to be measured. The resistance and polarisation being equal, and the battery being common to both circuits, these unstable elements are eliminated by balancing them from the circulation, and an expression is found for the unknown resistance X in the known resistances C and $\gamma$ of the voltameter, including the connecting wires and the volumes V and V′ of gases evolved in an arbitrary space of time within the tubes, viz. :—

$$X = \frac{V}{V'}(C + \gamma) - \gamma \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Changes of atmospheric pressure affect both sides equally, and do not therefore influence the results; but a reading of the atmospheric pressure is obtained at both sides by lowering the little supply reservoir with dilute acid to the level indicated in the corresponding tube. The upper ends of the voltameter tubes are closed by small weighted levers provided with cushions of india-rubber; but after each observation these levers are raised, and the supply reservoirs moved so as to cause the escape of the gases until the liquid within the tubes is again brought up to the zero line of the scale, when the instrument is ready for another observation. A series of measurements are given of resistances varying from 1 to 10,000 units, showing that the results agree within one per cent. with the independent measurements obtained of the same resistances by the Wheatstone method.

The advantages claimed for the proposed instrument are, that it is not influenced by magnetic disturbances, or the ship's motion if used at sea; that it can be used by persons not familiar with electrical testing; and that it is extremely simple and easily procured.

Royal Institution of Great Britain, Annual Meeting, Monday, May 1.—Sir Henry Holland, Bart., F.R.S., president, in the chair. The Annual Report of the Committee of Visitors for the year 1870 was read and adopted. Eighty-one new members were elected in 1870. Sixty-three lectures and nineteen evening discourses were delivered during the year 1870. The books and pamphlets presented in 1870 amounted to 118 volumes, making, with those purchased by the managers, a total of 307 volumes added to the library in the year, exclusive of periodicals. Thanks were voted to the president, treasurer, and secretary, to the committees of managers and visitors, and to the professors, for their services to the Institution during the past year. The following gentlemen were unanimously elected as officers for the ensuing year: President—Sir Henry Holland, Bart., F.R.S. Treasurer—Mr. William Spottiswoode, F.R.S. Secretary—Dr. Henry Bence Jones, F.R.S. Managers—Mr. John J. Rigsby, F.R.S., Mr. George Berkley, Mr. William Bowman, F.R.S., Mr. George Busk, F.R.S., Mr. Warren De la Rue, F.R.S., Capt. Douglas Galton, C.B., F.R.S., Mr. John Hall Gladstone, F.R.S., Mr. William Robert Grove, F.R.S., the Lord Lindsay, Mr. George Macilwain, the Duke of Northumberland, William Pole, F.R.S., Sir W. Frederick Pollock, Bart., Mr. Robert P. Roupell, Col. Philip James Yorke, F.R.S.

Geological Society, April 5.—Prof. Morris, vice-president, in the chair. The following communications were read :—1. "On a new Chimæroid Fish from the Lias of Lyme Regis," by Sir Philip Grey Egerton, Bart., M.P., F.RS. The fish for which the author proposed the name of *Ischyodus orthorhinus*, was represented by a specimen showing the anterior structures imbedded in a slab of Lias. It exhibited the characteristic dental apparatus of the Chimæroids, surrounded with shagreen, a very large prelabial appendage six inches long, and terminating in a hook abruptly turned downwards, and a process which the author regarded as representing the well-known rostral appendage of the male Chimæroid, but in this case attaining a length of 5½ inches, and covered more or less thickly with tubercles, bearing recurved central spines somewhat toothlike in their aspect. This appendage is attached to the head by a rounded condyle, received into a hollow in the frontal cartilage. The dorsal spine, which measured 6 inches in length, was articulated by a rounded surface to a strong cartilaginous plate projecting upwards from the notochordal axis, and was thus rendered capable of a considerable amount of motion in a vertical plane. This structure also occurred in *Callorhynchus* and *Chimæra*. Dr. Günther commented on the interest of this discovery, as in no other sharks is the same articulation of the dorsal spine as that described in the paper to be found. He inquired whether the granulated plate supposed to be dorsal might not be a part of the armature of the lateral line, as in sturgeons. He thought that the Chimæroids would eventually prove to be intermediate between the ganoid and shark types, and that all belonged to one subclass. Mr. Gwyn Jeffreys inquired what other remains were found with these fishes such as might represent the food, molluscan or otherwise, on which they lived. Sir P. Egerton replied that there was no deficiency of pabulum for any kind of fish in the sea represented by the Lias of Lyme Regis. He also made some remarks on another somewhat similar specimen in his own museum. The plate referred to by Dr. Günther, he stated, was symmetrical, and not like the lateral plates on the sturgeon, which are unsymmetrical. He therefore thought it dorsal.—2. "On the Tertiary Volcanic Rocks of the British Islands," by Archibald Geikie, F.R.S. In this communication the author gave the first of a series of papers which he proposes to lay before the society upon the volcanic rocks of Britain of later date than the chalk. In a general introduction to the whole subject, he pointed out the area occupied by the rocks, showing that they are chiefly developed along the broad tract which extends from the south of Antrim, between the chain of the Outer Hebrides and the mainland of Scotland, up into the Faroe Islands, and even to Iceland. The nomenclature of the rocks was discussed, and the following arrangement was proposed :—

| | Felspathic series | | | | | Pyroxenic, or Augitic. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Syenite | Felstone and Quartz porphyry. | Trachyte and Trachyte-porphyry. | Pitchstone | Porphyrite. | Dolerite | Basalt | Trachylite | Diallage-rock ? altered dolerite. | Felspathic tuffs. | Pyroxenic tuffs and agglomerate. |
| I. INTERBEDDED OR CONTEMPORANEOUS. | | | | | | | | | | | |
| A. *Crystalline.* | | | | | | | | | | | |
| Sheets or beds | ... | ... | ? | * | * | * | * | ... | * | | |
| B. *Fragmental.* | | | | | | | | | | | |
| Beds or layers | ... | ... | ... | ... | .. | ... | ... | .. | ... | ? | * |
| II. INTRUSIVE OR SUBSEQUENT. | | | | | | | | | | | |
| A. *Crystalline.* | | | | | | | | | | | |
| α. Amorphous masses | * | * | ? | ... | ... | ? | | | | | |
| β. Sheets | ... | ... | ... | ... | ... | * | * | | | | |
| γ. Dykes and veins | * | * | ? | * | ... | * | * | * | | | |
| δ. Necks | ... | ? | ... | ... | ... | ... | ? | | | | |
| B. *Fragmental* | | | | | | | | | | | |
| Necks | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | * |

The age of the rocks was shown to be included in the Tertiary period by the position of the volcanic masses above the chalk, and by their including beds containing Miocene plants. As an illustrative district, the author described the volcanic geology of the island of Eigg, one of the Inner Hebrides, and brought out the following points :—1. The volcanic rocks of this island rest unconformably upon strata of Oolitic age. 2. They consist almost wholly of a succession of nearly horizontal interbedded sheets of dolerite and basalt, forming an isolated fragment of the great volcanic plateau which stretches in broken masses from Antrim through the Inner Hebrides. 3. These interbedded sheets are traversed by veins and dykes of similar materials, the dykes having the characteristic north-westerly trend, with which they pass across the southern half of Scotland and the north of England. Veins of pitchstone and felstone, and intrusive masses of quartziferous porphyry, like some of those which in Skye traverse or overlie the lias, likewise intersect the bedded dolerites and basalts of Eigg. 4. At least, two widely separated epochs of volcanic activity are represented by the volcanic rocks of Eigg. The older is marked by the bedded dolerites and by the basalt veins and dykes which, though strictly speaking younger than the bedded sheets which they intersect, yet probably belong to the same continuous period of volcanic action. The later manifestations of this action are shown by the pitchstone of the Scùr. Before that rock was erupted the older doleritic lavas had long ceased to flow in this district. Their successive beds, widely and deeply eroded by atmospheric waste, were here hollowed into a valley traversed by a river, which carried southward the drainage of the wooded northern hills. Into this valley, slowly scooped out of the older volcanic series the pitchstone and porphyry *coulées* of the Scùr flowed. Vast, therefore, as the period must be which is chronicled in the huge piles of volcanic beds forming our dolerite plateaux, we must add to it the time needed for the excavation of parts of those plateaux into river-valleys, and the concluding period of volcanic activity during which the rocks of the Scùr of Eigg were poured out. 5. Lastly, from the geology of this interesting island we learn, what can be nowhere in Britain more eloquently impressed upon us, that, geologically recent as that portion of the Tertiary periods may be during which the volcanic rocks of Eigg were produced, it is yet separated from our own day by an interval sufficient for the removal of mountains, the obliteration of valleys, and the excavation of new valleys and glens where the hills then stood. The amount of denudation which has taken place in the Western Islands since Miocene times will be hardly credible to those who have not adequately realised the potency and activity of the powers of geological waste. Subterranean movements may be called in to account for narrow gorges, or deep glens, or profound sea-lochs; but no subterranean movement will ever explain the history of the Scùr of Eigg, which will remain as striking a memorial of denudation as it is a landmark amid the scenery of our wild western shores. Prof. Haughton inquired whether Mr. Geikie's attention had been called to the Morne Mountains in Ireland, which seemed to present some analagous phenomena to those described in the paper. In the Morne district were dykes of dolerite, pitchstone, and other volcanic rocks of the same constitution as those of Antrim. He believed that a chemical examination of these rocks in different districts would prove their common origin. The evidence in Antrim was conclusive as to their Tertiary age in Ireland, and he was glad to find that the view of their belonging to a different age in Eigg was erroneous. Prof. Ramsay had hitherto believed in the Oolitic age of these trap-rocks in Eigg, but accepted the author's views. The interbedding of volcanic beds among the Lower Silurian beds in Wales was somewhat analogous. He was glad to find the history of these igneous rocks treated of in so geological a manner, instead of their being regarded from too purely a lithological and mineralogical point of view. The great antiquity of these Middle Tertiary Beds had, he thought, been most admirably brought forward in the paper, as well as the enormous amount of denudation; and he would recommend it to the notice of those who had not a due appreciation of geological time. Mr. Forbes hoped that the geologist would remember that his father was a mineralogist. It was refreshing to find a paper of this kind brought before the Society, as it was to be regretted that the details of mineralogy were so little studied in this country when compared with the Continent; and this be attributed to the backward state of petrology (admitted by Mr. Geikie) in this country. He quite agreed in the view of the Tertiary age of these rocks. With regard to the terminology employed by the author, he objected to the use of the word dolerite, as distinct from basalt; basalt properly comprised, not only dolerite, the coarse-grained variety, and anamezite, the finely-grained variety, and the true basalt, but also trachylite, which was frequently confounded with pitchstone. All four names merely referred to structure, and not to composition. Mr. Geikie, in reply, stated that he had not examined the Morne Mountains. He had not in any way wished to disparage mineralogy, but, on the contrary, had attempted to classify the different rocks according to their petrological character. He used the term dolerite in the same sense as the German mineralogists, both as the generic name for the whole series, and also for the coarser variety of basalt. 3. "On the formation of 'Cirques,' and their bearing upon theories attributing the excavation of Alpine Valleys mainly to the action of Glaciers," by the Rev. T. G. Bonney, M.A., F.G.S. The paper described a number of these remarkable recesses, which, though not restricted to the limestone districts of the Alps, are best exhibited in them. The author gave reasons why he could not suppose them to have been formed either as craters of upheaval, or by the action of the sea, or by glacial erosion. With regard to the last he showed that, even if glaciers had been the principal agents in excavating valleys, there were some cirques which could not have been excavated by them; and then went on to argue from the fact that glaciers had occupied cirques, and from the relation between them and the valleys, that they could not be attributed to different agents. He also showed that commonly the upper part of the valley, where the erosive action is perhaps least, is very much the steepest, and urged other objections to the great excavatory powers often attributed to glaciers. He then described one or two cirques in detail, and showed that they were worked out by the joint action of many small streams, and of the usual mete ric agents working upon strata whose configuration was favourable to the formation of cliffs. Mr. Whitaker suggested an analogy between the cirques and the combes in our own limestone countries. Mr. Geikie regarded the cirques as analogous with the combes of Wales and the corries of Scotland. They were not, however, confined to limestone districts, but occurred also in gneiss and granite rocks. He thought that the shape was much influenced by the bedding and jointing of the rocks, as there was an evident connection between these and the shape of the combes. He could not, however, see his way to account for the vertical cliffs surrounding the cirques. The Rev. T. G. Bonney, in reply, observed that though cirques were not confined to limestones, the finest instances occurred in such rocks. When cirques occurred in crystalline rocks, the talus was usually much larger than in limestone.—The following specimens were exhibited: Specimens of Fossil Fish-remains from the Lias of Lyme Regis; exhibited by Sir P. de Malpas Grey Egerton, in illustration of his paper.

Royal Society of Literature, April 26.—Mr. Hyde Clarke read a paper on the "Classic Names of Rivers," more particularly in Greece, Asia Minor, and Italy. After referring to the discoveries in the stone period by Mr. Finlay and others, and to the megalithic and cyclopean structures, he proceeded to consider what evidence was afforded by topographical nomenclature of the populations which preceded the Hellenic. He showed that the river-names in the classic regions conformed with each other, and that this was not attributable, as supposed, to Hellenic colonisation. These names also conform to those of India, and of the ancient world generally; but the explanation was not to be found in Aryan etymologies, but that it was to be sought in earlier forms. These are represented in the languages of the Caucasus, of which the Georgian, Suan, Latian, and Lesghian afford examples now. With these the Thracian and the languages of Asia Minor corresponded. The local facts gave colouring to the legends of the occupation and invasion of Attica by the Amazons, and of the existence in Europe of a Thracian population allied to that of Asia. The eastern connection of the Etruscan and Italian populations, too, was to be accounted for as with the Caucasus, and not with America. He referred likewise to the influence of the river-names on classic mythology, and particularly on the nomenclature of Tartarus.

Linnean Society, April 20.—Notes on Mr. Murray's paper on the Geographical Relations of the chief Coleopterous Faunæ, by Dr. Roland Trimen. The author considered that the argument of a continuity of land at a previous epoch is too often resorted to to explain the occurrence of the same species of insects in widely remote countries. He entered in considerable detail into the chief features of the distribution of the genera and species of Coleoptera, especially at the Cape; laying much stress on the difficulty which introduced species find in establishing themselves in soil already well stocked.

Society of Biblical Archæology, April 4.—Dr. S. Birch, F.S.A., president, in the chair.—Mr. Henry Theodore Bagster, Mr. Richard Bosanquet, Mr. A. W. Franks, M.A., V.P.S.A., &c., and Mr. Burnett Tabrum were duly elected members of the society. The Secretary read a paper communicated by Mr. Henry Fox Talbot, F.R.S., &c., "On an Eclipse mentioned on an Assyrian Tablet." The tablet in question is preserved in the British Museum, and is marked 154 and 122*b*. The translation runs thus: "To the King of the World—My Lord, Thy servant, Kukuru, sends this:—May Assur, the Sun, and Marduk be propitious to my Lord the King in bis journey from his kingdom to the land of Egypt! I inform his Majesty that in the month of Su there was an Eclipse. Five portions of the full orb were obscured. Let the King be of tranquil mind, since the eclipse of the month of Su portends good fortune to the King." The translator proceeded to identify this eclipse thus recorded with one which took place in the seventh warlike expedition of Assurbanissal against Tiumman, King of Elam. The next meeting was then announced to take place on Tuesday, 2nd proximo, to which date the meeting was then adjourned.

### Dublin

Royal Irish Academy, April 10.—Rev. T. H. Jellett, president, in the chair. Prof. Hennessy, F.R.S., read a paper On the Floatation of Sand by the incoming tide at the Mouth of a Tidal River. During the course of a tour along our western coast, in the summer of 1868, the following incident came under my notice; and, although I made a note of the facts at the time, I have never hitherto made them the subject of a scientific communication: On July 26, when approaching the strand at the river below the village of Newport, County Mayo, I noticed what appeared to be extensive streaks of scum floating on the surface of the water. As it was my intention to bathe, I was somewhat dissatisfied with the appearance of the water, until I stood on the edge of the strand, and I then perceived that what was apparently scum, seen from a distance, consisted of innumerable particles of sand, flat flakes of broken shells, and the other small *débris* which formed the surface of the gently-sloping shore of the river. The sand varied from the smallest size visible to the eye up to little pebbles, nearly as broad and a little thicker than a fourpenny piece. Hundreds of such little pebbles were afloat around me, and it is probable that the flakes of floating matter seen farther off contained also a considerable proportion. The air during the whole morning was perfectly calm, and the sky cloudless, so that, although it was only half-past nine, the sun had been shining brightly for some hours on the exposed beach. The upper surface of each of the little pebbles was perfectly dry, and the groups which they formed were slightly depressed in curved hollows of the liquid. The tide was rapidly rising, and, owing to the narrowness of the channel at the point where I made my observations, the sheets of floating sand were swiftly drifting farther up the river into brackish and fresh water. On closely watching the rising tide at the edge of the strand, I noticed that the particles of sand, shells, and small flat pebbles, which had become perfectly dry and sensibly warm under the rays of the sun, were gently uplifted by the calm, steadily-rising water, and then floated as readily as chips or straws. I collected a few specimens of these little objects, but I regret that they have been since mislaid. This phenomenon, it is scarcely necessary to say, is due to molecular action, such as accompanies the familiar experiment of floating needles on the surface of a basin of water. Although the specific gravity of the floating objects exceeds that of the fluid on which they rest, the principle of Archimedes still holds good, because the displacement of liquid produced by the body is considerably greater than the volume of the body itself. In the case of a floating needle, the repulsion of the liquid from the polished surface of the metal presents a groove, whose magnitude is obviously many times greater than the needle; but in the case of the floating pebbles this was not so manifest. The attraction of the molecules of water for one another produces, as is well established, a tension at the surface of the liquid, which, although extremely feeble, and generally noticed only in connection with capillary phenomena, yet interposes some resistance to the intrusion of foreign substances. I have floated small flat pebbles, similar in size and appearance to the largest of those observed floating on Newport river, for more than six days, while fragments of shells, and thin pieces of slate as broad as a sixpenny-piece, have continued to float much longer. These little bodies occasionally sank from the gradual absorption of water, but much more frequently from some accidental motion of the vessel containing the liquid. It is manifest that the floatation of sand in a tidal estuary, as in the instance I have seen, can occur only under favourable conditions. The shores must be very gently inclined, the air perfectly calm, and the weather dry and warm. Under these circumstances thin cakes or sheets of sand may not only be uplifted by the water, but if the tide flows rapidly they may continue to float sufficiently long to allow many of them to be drifted far from their original place up to the higher limit of the brackish water. In this way fragments of marine shells and exuviæ might become mingled with those belonging to fresh water. The conditions favourable for sand floatation must exist during calm weather in a very high degree of perfection on the sandy shores of tidal rivers in tropical and subtropical districts of the earth. As this phenomenon can take place only with the rising tide, and never with the falling tide, the result must generally be favourable to the transport of sand and marine *débris* in the direction of the flow of flood tide; and this may sometimes hold good along a coast as well as on the shores of a tidal estuary. Geologists, as far as I am aware, have not hitherto noticed this phenomenon in connection with the formation of stratified deposits by the agency of tides and rivers, although they have paid great attention to the influence of the molecular resistance of water to the sinking of very minute solid substances, with the view of explaining the wide surface over which matter held in suspension by water may be spread when ultimately deposited over the sea bottom.—Prof. W. King read a paper, by himself and Prof. Rowney, "On the Mineral Origin of the so-called *Eozoon Canadense*." It was resolved to purchase the Bell and Bell-Shrine of St. Patrick, from Dr. C. Todd, for the sum of 500*l*.

## BOOKS RECEIVED

ENGLISH.—Travels in the Air: J. Glaisher, 2nd edition (R. Bentley).—The Natural History of Plants: H. Baillon, vol. 1, translated by N. Hartog (L. Reeve and Co.).—Primitive Culture, 2 vols.: E. B. Tylor (J. Murray).—On Aphaxia, or Loss of Speech: Dr. F. Bateman (Churchill).

FOREIGN.—(Through Williams and Norgate)—Archiv für Anthropologie, vol. iv.—Zeitschrift der oesterreichischen Gesellschaft für Meteorologie, vol. v.—Compendium der chirurgischen Pathologie u. Therapie: Dr. C. Heitzmann.

## DIARY

*THURSDAY*, MAY 4.

ROYAL SOCIETY, at 8.30.—On the Structure and Affinities of the Gwynia Annulata (Dunc.), with Remarks upon the Persistence of Palæozoic Types of Madreporaria: Prof. Duncan, F.R.S.—On Molybdates and Vanadates of Lead, and on a new Mineral from Leadhills: Dr. A. Schrauf.
SOCIETY OF ANTIQUARIES, at 8.30.—Roman Villa at Beddington: J. Addy.—Antiquities from Cyprus: J. B. Sandwith.
LINNEAN SOCIETY, at 8.—The phenomena of Protective Mimicry, and its bearing on the Theory of Natural Selection as illustrated by the Lepidoptera of the British Islands: Raphael Meldola, F.C.S.—On the Ascalaphidæ: R. McLachlan.
CHEMICAL SOCIETY, at 8.—On the Productive Powers of Soils in relation to the Loss of Plant Food by Drainage: Dr. Voelcker, F.R.S.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall.
LONDON INSTITUTION, at 7.30.—On Economic Botany: Prof. Bentley.

*FRIDAY*, MAY 5.

GEOLOGISTS' ASSOCIATION, at 8.—On the Fauna of the Carboniferous Epoch: H. Woodward, F.G.S.
ROYAL INSTITUTION, at 9.—On Russian Folk-Lore: W. R. S. Ralston.

*SATURDAY*, MAY 6.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.
ROYAL INSTITUTION, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

*MONDAY*, MAY 8.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.
ROYAL INSTITUTION, at 2.—General Monthly Meeting.
LONDON INSTITUTION, at 4.—On Astronomy: R. A. Proctor, F.R.A.S. (Educational Course.)

*TUESDAY*, MAY 9.

PHOTOGRAPHIC SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—On Force and Energy: Charles Brooke, F.R.S.

*WEDNESDAY*, MAY 10.

SOCIETY OF ARTS, at 8.—On the Application of Steam to Canals: Geo. Edward Harding, C.E.
GEOLOGICAL SOCIETY, at 8.—On the Ancient Rocks of the St. David's Promontory, South Wales, and their Fossil Contents: Prof. R. Harkness, F.R.S., and Henry Hicks.—On the Age of the Nubian Sandstone: Ralph Tate, F.G.S.—On the Discovery of the Glutton (*Gulo luscus* in Britain: W. Boyd Dawkins, F.R.S.

*THURSDAY*, MAY 11.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
MATHEMATICAL SOCIETY, at 8.—On the Singularities of the Envelope of a non-Unicursal Series of Curves: Prof. Henrici.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall.

## CONTENTS

THURSDAY, MAY 11, 1871

## *THE PROPOSED COLLEGE OF PHYSICAL SCIENCE AT NEWCASTLE-UPON-TYNE*

A FEW weeks ago * we gave some account of the initiation of a movement in the North of England, having for its object the establishment of a College of Physical Science in Newcastle-upon-Tyne. As the Executive Committee appointed at the public meeting had only then begun its work, the details entered upon were given as mere indications of the general form the institution might be expected to take. A letter from the Master of University College, Durham, which appeared in our columns more recently, added somewhat to our information, and a circular which has been issued, with commendable promptitude, by the Executive, is now before us, representing the views of the promoters as modified in committee.

We shall probably best further the intentions of the Committee, whom we are anxious to aid, and at the same time give our readers the most reliable information, if we reprint this document verbatim :—

"It is proposed to found at Newcastle-upon-Tyne, in connection with the University of Durham, a College for the teaching of Physical Science, especially as applied to Engineering, Mining, Manufactures, and Agriculture.

"The want of such an Institution has long been felt in the North of England, and it is believed that while it would be useful in all the above pursuits, it would be of especial value to all persons intended for the professions of Mining and Engineering.

"Such an Institution (which it appears desirable to limit at its commencement to purely scientific objects) would offer instruction in the following branches of scientific knowledge. 1. Pure and Applied Mathematics. 2. Chemistry. 3. Experimental Philosophy. 4. Geology, Mineralogy, and Biology. Professorships and Lectureships will be founded on these subjects. It is proposed that the course of study shall last for two years, that it shall consist partly in attending lectures and partly in the work of the laboratories, that there shall be examinations at the end of each year, conducted mainly by Examiners from the Universities, and that at the final examination the successful students shall receive the title of Bachelor or Associate of Science of the University of Durham, or, upon certain further conditions, the degree of B.A. It is hoped that classes of evening lectures for those who are unable to attend during the day may soon be formed.

"The Government of the Institution will be entrusted to a Council, of which one-third will be nominated by the University of Durham. The University has offered the sum of 1,000*l.* annually towards the establishment of Professorships and of ten Scholarships of 20*l.* each to assist students. It is believed that 2,000*l.* a-year is the lowest estimate at which it is possible to place the expenses of such a College, even at its commencement, and it is proposed to appeal to the public for a subscription to create a capital fund of at least 30,000*l.* If this amount be collected, the endowment from the University of Durham will be made a permanent one. When it is remembered that such an Institution will benefit a very large portion of the population of the Northern Counties, and be directly useful to nearly all branches of Manufacturing and Agricultural, as well as of Mining and Engineering pursuits, it is believed that no difficulty will be found in ultimately raising this sum, which, according to the experience of all similar institutions, will probably be increased by private donations both for Scholarships and Professorships. It is proposed to offer Subscribers the option either of paying their whole subscription at once or of extending it over a period of five or six years. Small as well as large subscriptions are invited towards the above-mentioned fund. Upwards of 100,000*l.* has been recently collected in a similar case, or is in the course of collection, in subscriptions ranging from 2,000*l.* to the very smallest sums."

The last paragraph pleases us most. Six years is perhaps long enough to look forward in arrangements of many sorts, but not in matters pertaining to finance. The adoption of a scheme such as that originally suggested, based on a preliminary terminable endowment, would have crippled the energies of the whole staff, by suggesting the possibility of the early demolition of the structure they were labouring to build. It can scarcely be known, until the trial is made, how much may have to be done in the way of *creating the demand* for scientific education in the locality. We do not for a moment doubt its existence to a considerable extent, but we cannot suppose that the present case will form any exception to the general rule, that educational facilities are only slowly and by degrees taken advantage of by the classes for whose benefit they have been primarily designed. If the proposed college *begins* to find an appreciative public, and to promise eventual success within the six years, we should regard it as a subject of congratulation, and a proof alike of energy and judgment in its management, rather than as a matter of course. Happily, for this reason, the guarantee principle is to be put upon its best basis—funded property. Instead of 1,000*l.* per annum for a term of six years, as at first proposed, the public is asked for 30,000*l.* in one sum. This, with the consequent permanent endowment from Durham University, which may be regarded as equal to another 30,000*l.* capitalised, will provide a substantial foundation to commence upon. Nor can we doubt that the amount required will be easily raised amongst the wealthy men of the North.

We may perhaps say one word more about the selection of subjects for professorships, as our former remarks are alluded to in the Rev. Mr. Waite's letter.

We adverted to the absence of any mention of Biology as a part of the scheme of education in the report of the speech of the Dean of Durham at the preliminary meeting. In the revised programme, above reprinted, biology is *not omitted*, but that is all that can be said. The subject is tacked on to geology and mineralogy, and the result is a complete anomaly. To teach mineralogy in any modern sense, a man must be more than an average chemist—hence no one who is not an expert in geology, mineralogy, chemistry, zoology, and botany, will have the requisite qualifications for the chair which it is now proposed to constitute. We trust that the Committee are prepared to pay pretty smartly for so handsome an instalment of omniscience. Our fear that biology of itself might be thought too large a subject for a single professorship, was at any rate groundless, but we doubt whether entire exclusion would be worse than the grant of a third of a chair.

The geological knowledge of first importance in a mining district is essentially "stratigraphical," in other words, that attained by practical field work. Just so much palæontology is necessary as will enable the student to recognise the more common characteristic fossils, and sufficient acquaintance with minerals to render him fami-

* See NATURE, vol. iii. p. 461.

liar with ordinary rock specimens. It would be better that Geology of this sort should be associated with a subject like mining, instead of being placed in the position it at present occupies.

Mineralogy, in any right sense, is only applied chemistry, and would be more in place as a recognised portion of the chemical curriculum in such an Institution than as a part of geology. Few geologists pretend to mineralogy beyond a sufficient knowledge of the general external characters of rocks for the recognition of the commoner varieties. Palæontology, on the other hand, as a subject of systematic study, is but a phase of biology, and cannot without violence be linked with subjects arising out of the laws which govern the inorganic world.

In thus enlarging upon our former remarks, we are actuated solely by a desire for the success of an undertaking which has our entire sympathy.

Just as we are going to press we learn that it has been determined to push forward the arrangements so as to enable the College to open its doors in October. This is a wise decision on many grounds. The first week in October has become the recognised time for the commencement of winter courses of lectures, and delay beyond that might easily entail the loss of a whole year. Of the 30,000*l.* required, upwards of 17,000*l.* has already been subscribed, without any systematic canvass, and we can scarcely doubt that the remainder will be forthcoming. On public grounds we would venture earnestly to second the appeal made by the Committee, and to express the hope that the liberality of the coal-owners, manufacturers, and merchants of the district will enable them to open the Institution free from pecuniary embarrassment, and clear of the manifold difficulties that beset an undertaking burdened at the outset with debt. We also hear at the same moment that the Committee has again debated the question of a biological professorship. That body seems to be undecided as to whether it would be less ridiculous to ignore biology entirely, or to include it with a number of quite distinct branches of science in a sort of miscellaneous professorship, and the prevailing view *now* seems to be that, on the whole, the former alternative is the least conspicuously absurd. Surely there is a third course open to the Committee. We trust wiser counsels will prevail, and that we may never have to record that in Newcastle—the home of Bewick and Selby, Fryer and Alder, Winch and Robertson, not to name a host of living biologists—in the focus of the Tyneside Naturalists' Field Club—a College of Natural Science has been established in which Natural History in its higher aspects is excluded as a subject of study.

## *STAVELEY'S BRITISH INSECTS*

*British Insects. A familiar Description of the Form, Structure, Habits, and Transformations of Insects.* By E. F. Staveley, Author of "British Spiders." (London: L. Reeve and Co., 1871.)

TO compose a work on so extensive and difficult a subject as "British Insects," which shall convey a large amount of useful and interesting information without being too much overloaded with bare facts,—which shall be accurate without being dry, and amusing without being flippant,—is no easy task, yet it is accomplished by the author of this work in a very creditable manner. The introductory chapters are condensed and clear, just giving enough information on the general structure and economy of insects to interest the uninitiated reader, and lead him on to the more detailed account of each order given in the succeeding chapters.

An excellent feature of the work is the clearness of the type, and the well-executed woodcuts which somewhat too sparingly illustrate the text, while sixteen coloured plates by Mr. Robinson contain admirably life-like portraits of nearly a hundred of our most conspicuous or most interesting insects. A few extracts will best illustrate the author's style. In the chapter on the larvæ of Lepidoptera it is remarked, that there is neither time nor place in which we may not find the traces of these creatures or the creatures themselves.

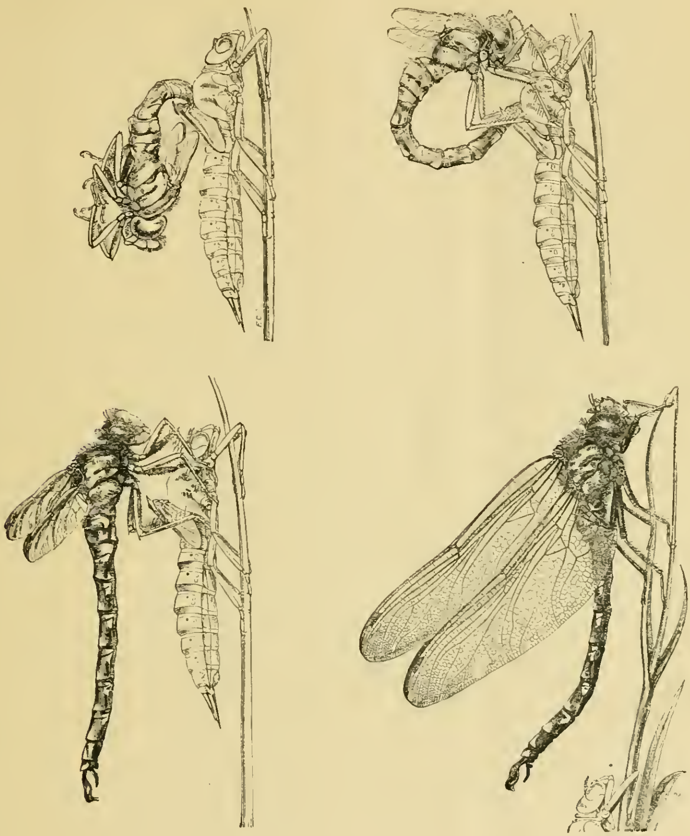
"If at one time of the year we tear a handful of moss from the trunk of a tree, out drop some little brown chrysalids; if at another we drag a tuft of grass up by the roots, there we find silken tubes, the homes of some small caterpillars. We find them in fungi, we find them in grain, we find them in teazle-heads, in fir-cones, in rose-buds, and in fruit; and the Hymenopterist, carefully watching the insect emerging from a gall, discovers that he has reared in it a moth! On the face of a lichen-covered rock we see a moving fragment, and lo! a little caterpillar, neatly encased like a caddis-worm in a tent of lichen, is moving and feeding, safe even from the bird's sharp eye. We open our drawers, and there, oh, sight of horror! What is that streak of white silk upon the best garment—the garment laid by, too good for common wear? We look farther; what is that dusty little roll? It is a great-coat on a microscopic scale. It matches our best garment ominously. It moves—a head peeps out—some little legs, and away it walks!—tell not the housekeeper!—away it walks in safety from the admiring Entomologist."

As an example of the woodcut illustrations we give the series showing the progressive stages in the transformations of the dragon-fly. The sluggish mud-coloured pupa ascends the stem of a grass or any other stalk of convenient size which rises above the surface of the water, after a time the skin cracks behind, between the wing cases, and the head and thorax of the enclosed fly are drawn out. The abdomen follows, the insect turning up and clinging to the pupa case, where it remains till the wings increase to the full size so rapidly that they can be seen to grow.

In the chapter on Diptera there are some good remarks on the many erroneous uses of the term "Fly."

"Being a 'popular name' the people have a right to mean what they choose by it, and they avail themselves of the right—some meaning by it one thing, some another, some every flying insect for which they know no other name. Thus the 'fly' of the former is usually the little hopping turnip beetle; the 'fly' of the hop-grower is an aphis; the 'fly' of the herdsman a gad; while to the citizen almost anything to be seen with wings (except pigeons and sparrows) is a fly. There are some, again, to whom flies are *flies*, one fly *the* fly, the common well-known little black house-fly. Here at last is something definite. No, not even now; for these will, at least, claim their young house-fly, and their full-grown house-fly, and expect you to believe that late in the year their house-fly takes to biting you, little dreaming that the little fly, and the big fly, and the fly which bites you, not only are different species but even belong to different genera; that the little fly never grows big, that the big fly never was little, and that their house-fly could not bite you if he would. What, then, are we to understand by the name fly? It is clear that the popular sense has no sense at all, or too many senses, and yet the word cannot be spared from our vocabulary. In any Latin dictionary we shall find *Musca* (fly), and the entomologist pounces upon it and says it shall mean the *tribe of two-winged insects.* Linnæus so used it, and his genus *Musca*, now broken up into many new genera, represented the greater number of those insects which the entomologist now claims as flies."

In some parts of the work there is rather a tendency to jump at conclusions, and to give explanations of very doubtful value. It is attempted, for instance, to explain why the bee has four wings instead of two, by the fact that it is necessary for them to fold up and pack into a small compass to avoid injury and be out of the way during work, and this it is said is "the purpose of the division of the wing." This conveys the entirely erroneous



TRANSFORMATIONS OF THE DRAGON-FLY

impression that the wings of insects are normally two, and that the four are formed by the "division" of these two, an impression which we feel sure a person so well informed as the author could not have meant to convey. It also seems carrying hypothetical life-history a little too far to say of a bee emerging from the pupa that "into his mind rushed a full sense of his responsibilities," and on finding himself, say, a worker, "he, or rather she, became aware that the duties of house-builder, housekeeper, nurse, and even soldier and sentinel, devolved upon her;" and accordingly she forthwith "addressed herself to the task of repaying to futurity that debt which the cares of a former generation had laid upon her, and daily she toiled in its fulfilment." To make this exposition of the mental state of the newly-born bee complete, we should have been told whether it regulated its conduct in doubtful cases according to the utilitarian or the intuitive theory of morality.

Such vagaries as the above are however rare, and we can conscientiously recommend this book as admirably adapted to lead its readers to observe for themselves the varied phenomena presented by insects, and thus to become true entomologists. ALFRED R. WALLACE

## *AMERICAN GEOLOGY*

*Preliminary Field Report of the United States Geological Survey of Colorado and New Mexico.* Conducted under the authority of the Hon. J. D. Cox, Secretary of the Interior. By F. V. Hayden, United States Geologist. 8vo. pp. 155. (Washington: Government Printing Office, 1869.)

THIS preliminary field report makes us acquainted with a vast tract of territory hitherto scarcely known, save to the more adventurous squatters and to the various tribes of Indians who have gradually been driven farther and farther west by the wonderful growth of the United States populations, fed as they are annually by streams of English, Irish, Scotch, and German emigrants. Unfortunately for the Red-skins, they are not only hemmed in on the one side by the United States, and on the other by the equally vigorous growth of California and its vast mining and agricultural population; but their territory, only hitherto invaded by the Mormons and the "Pony-Dispatch," is now cut in twain by the great Pacific Railroad, which, in its course, has sent forth geological reconnaissances right and left, discovering timber here, coal there, building stone in this spot, mines in that, until there is no space left for them save in the happy hunting-grounds above, to which they are fast going, aided by revolvers, alcohol, and disease.

The report refers to a line of country extending from British North America to New Mexico in a northerly and southerly direction, and from the Rocky Mountains to the Lower Missouri in an easterly and westerly one. Dr. Hayden explains the reason why he has been able in a very short time to cover so large a tract of territory—it is, that "there is great uniformity in the geology of the country, and when one has become familiar with the different geological formations over a small area, he can trace them with great rapidity over long distances" (p. 11).

First, we have the Rocky Mountain system forming the main ridges and the hills, composed of granite rocks. Resting on the flanks of these more elevated masses, the stratified deposits are exposed in succession, becoming less and less inclined as we recede from them and enter the plains.

The oldest stratified deposit met with is the Potsdam Sandstone, equivalent in geological position to our Upper Cambrian, or to the Primordial Zone of Barrande; this is followed by strata of Carboniferous age, but giving no promise of workable seams of coal. The Triassic series may be represented by certain red arenaceous deposits, sometimes containing gypsum and rocksalt; these pass upwards into undoubted Oolitic beds. Next follows a Cretaceous formation, some 4,000 feet in thickness, followed by a well-developed Tertiary series of vast geographical extent, and but very slightly inclined.

These Tertiary beds are rich in lignites, and evidence a long period of tranquil estuarine or lacustrine deposition in a region supporting dense forests of large trees, and a vegetation far exceeding in luxuriance anything now met with in these latitudes. Carnivores, Pachyderms, Proboscidea, &c., occur in great abundance. It is very interesting to know that in Tertiary times North America had its elephants, hippopotami, rhinoceroses, horses, lions, &c., and was, in the size and abundance of its Mammalia, in no way surpassed by the Continents of the Old World.

Two minor reports accompany Dr. F. V. Hayden's report, one on "Mines and Mining," by Mr. Persifor Frazer, jun., giving a most interesting account of the mining capabilities of the district; the other on the "Agriculture of Colorado," by Mr. Cyrus Thomas. There is every prospect of the Colorado territory becoming as rich an agricultural district as it has already proved to be a mining one. H. W.

## *OUR BOOK SHELF*

*Aunt Rachel's Letters about Water and Air.* (London: Longmans and Co., 1871.)

IN the form of a series of familiar letters from an aunt to a nephew and niece, we have here an account, in simple familiar language, of some of the commoner physical phenomena of nature. Recollecting the books with a similar aim that have passed through our hands, we feel grateful to find one free from conspicuous blunders. To the little book before us we need not however apply such negative praise. It is in all respects to be commended as a book to put into the hands of the young. And we fancy that even many well-educated people who are not young in years, will find a record and explanation of facts with which they are not familiar. They may learn here all about the formation of ice, latent and specific heat, the air-pump, the barometer and thermometer, the winds, combustion, and many other phenomena of daily life. A few well-executed woodcuts illustrate the text; and we would like to hear that a large circulation has rewarded the efforts of "Aunt Rachel" to popularise the elements of science.

*Handbuch der allgemeinen Himmelsbeschreibung vom Standpunkte der kosmischen Weltanschauung dargestellt.* Von Hermann J. Klein. Pp. 351. (Braunschweig, 1871. London: William and Norgate.)

*Theoretische Astronomie.* Von Dr. W. Klinkerfues. Erste Abtheilung. Pp. 256. (Ditto, ditto.)

THE first of these works is the second edition of the first part of a general description of the universe, and is devoted to the solar system: another part will be given to the fixed stars. The aim of the author is to afford a complete account of his subject, including the latest researches, which shall be at the same time thoroughly scientific, while it will not be beyond the comprehension of those who are possessed of only an elementary knowledge of astronomy, or more properly perhaps uranography. The first forty-nine pages contain a description of the sun; the next five are given to the zodiac. Then follow the planets Mercury, Venus, &c., in order, and finally we have a full and very interesting account of comets and meteorites.

Turning to the chapter on the sun, we find, after a general introduction, methods for calculating the distance between the centre of the sun and that of the earth. After this we have an account of the "spots," accompanied with tables of their numbers in different years, and their connection with the movements of the magnetic needle. The labours of Herschel, Airy, Lockyer, Huggins, and others are largely quoted, and the author begs any observer whose researches may have been omitted, to attribute the neglect to the disturbing influence of recent events. The earth and her satellite are treated at some length, and the questions of the moon's influence on the earth's atmosphere, the winds, weather, and magnets, are fully discussed. The chapter on meteorites is very interesting. We are told, on the authority of Miller and Haidinger, that the earliest mention of meteorites is probably in Iliad xv. 18—22, where the anvils spoken of by Jupiter are supposed to refer to these phenomena. Livy mentions a shower which some think may have been a star shower; and the famous black stone in the Kaaba, at Mecca, is said to be undoubtedly a meteorite of great antiquity. Numerous analyses of meteorites are given, and tables are added containing full details of all those which are recorded to have fallen from the earliest times. There are similar tables with regard to comets and star-showers; and finally we have two well-executed plates of the appearance of different sun-spots, and a chart of part of the moon's surface. We should like to see an English edition.

The Theoretical Astronomy of Dr. Klinkerfues, director of the Royal Observatory of Göttingen, is a reproduction of lectures delivered by him in that University. This is the first part of the work, and its object is to give an explanation of the means by which the courses and positions of heavenly bodies are determined. It is not adapted to the general reader, but will prove a useful companion to the mathematician who wishes to obtain an insight into astronomical methods of calculation. Several very good figures accompany the text.

G. T. A.

*Kuklos; an Experimental Investigation into the Relationship of Certain Lines.* By John Harris Part I. (Montreal, 1870)

IN a review of Prof. Bretschneider's History of Early Geometry we have mentioned some clever attempts to square a circle, made at a time when this problem engaged the attention of the first mathematicians. Then, however, as at present, there existed circle squarers of a different kind, who excel only in demonstrating their own ignorance. A fine specimen is preserved by Simplikios. Some persons had heard of square numbers which are at the same time cyclical, that is to say, the last figure in the square number is the same as that of the root, as 25 and 5. Nothing, of course, could be more evident to them than that a number which is both square and cyclos must be a measure for the circle. Mr. Harris ranks almost as high, only he does not give his conclusions in quite so short a form. His book is to consist of four parts in quarto, of which the first contains merely a preface, preliminary arguments, and on the last page an introduction. In the preface the author excuses the haste in which the publication has taken place, with the remark that if his researches are of value they cannot be brought early enough before the public,—if a failure " the communication itself would not be worth the additional labour bestowed on improving its form." This latter conclusion we willingly grant. It is only to be regretted that Mr. Harris has not had the same opinion of the time he spent in writing this communication and preparing the numerous and long figures which fill ten large plates.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Pangenesis

MR. GALTON—by acting upon the conclusion that the supposed gemmules supposed to be detached from the cells of the body at different periods of life in the case of the higher animals swarmed in the blood prior to their supposed collection and union to form the reproductive element—favoured the provisional hypothesis of Pangenesis, for he indicated a not improbable manner in which the very improbable phenomena involved in the hypothesis might actually occur.

But Mr. Darwin, in NATURE for April 27th, writes to explain that he maintains that the gemmules must be "thoroughly diffused"—I conclude, suspended in the fluids which circulate freely in every part of the very substance of all the tissues of the body. The supposed gemmules must be much more minute than the smallest particles that can be seen by the highest magnifying powers used in these days, and must be invisible to the eye when made to appear five thousand times larger than their real size. They must be capable of *diffusion*, and, as is suggested by Mr. Darwin, much as chemical substances are *diffused.** But the terms of the hypothesis would imply that the gemmules are actual particles *suspended* and not *dissolved* in the fluids.

It is not very encouraging to those who work, to discover after having performed numerous and well-devised series of difficult, laborious, and troublesome experiments honestly to test the value of a hypothesis, that they have been investigating a shadow, and to be then informed that the results they have obtained have little or no bearing on the question at issue. The "experiments are extremely curious," says Mr. Darwin, and the experimenter "deserves the highest credit for his ingenuity and perseverance."

It would, of course, be possible to remove from one animal portions of tissue which, according to the hypothesis, *must* contain the supposed gemmules, if they exist, and graft the pieces of tissue upon another. If the experiment was successful, and the offspring exhibited any of the characters of the variety from which the graft was taken, the opponents of Pangenesis would admit the doctrine at once, but if the results were again of a negative kind, would Mr. Darwin consider that his hypothesis had "received its death blow?" It would certainly be as easy to defend it as it is at this time. Nor do I believe it possible to obtain a series of experimental results which would lead the supporters of Pangenesis to abandon the hypothesis. A firm belief in hypothetical gemmules, which cannot be rendered evident to the senses, is not likely to be shaken.

Depend upon it, neither the well-devised experiments of Mr. Galton, nor any other experiments that may be devised, will overthrow this doctrine. The provisional hypothesis of pangenesis is perfectly safe, and will withstand every attack that may be made. It cannot be successfully assailed. Like many favoured hypotheses of these days, it can neither be proved to be true nor positively shown to be false, and it is open to anyone to ground his belief in the truth of this and other doctrines upon the fact that they have not been and cannot be disproved. For undoubtedly gemmules *may* be formed in the manner supposed; if formed, they *may* be detached; if detached, they *may* pass through the tissues; they *may* then collect together, and *may* form reproductive elements. Each one of the countless millions of sperm elements produced in such profusion during so many years of life *may*, indeed, be formed by the union of millions of gemmules which, after meandering through the various textures of the body, marshal themselves in order in one particular locality. From the vast company thus supposed to have collected, we *may* conceive, by the light of imagination, the formation of regiments composed of multitudes of individual gemmules of the same kind; and further, it is not difficult to imagine that each individual gemmule of every regiment *may* move away and unite with thousands and tens of thousands of others, to form at length that marvellous compound and complex speck of matter less than the $\frac{1}{50000}$ of an inch in diameter, which constitutes the active material of each small reproductive particle. This is one way in which the properties of the spermatozöon may be accounted for. Nor is it beyond the power of the imagination to picture the orderly arrangement and rearrangement of such vast hosts of potential molecules as is supposed. No confusion, no jostling of one another, no struggling would be seen, for each molecule takes its appointed place, in obedience to its own properties, knowing of course the position it is to occupy in the complex ranks at each different period of its life's progress, and, never ambitious of discharging a higher function than that which it is destined to fulfil, performs the important office of transmitting certain peculiarities, important or trivial, useful or useless, from the existing to a new being.

* In NATURE for May 1st, Mr Francis Galton very properly remarks that the term Mr. Darwin should have employed is "dispersion" not "diffusion," and there are other critical remarks which appear to me equally just.

We may be led from the consideration of the broad facts nature to conceptions of the most abstract kind, without being conscious of the slightest gap between the facts of Science and the creations of the Imagination. In these days the utmost skill is often displayed in hiding and ignoring or denying the hiatus by which the arguments deduced from the results of observation and experiment are separated from those which are based upon the fictions of the fancy. But, unhappily, the gulf cannot be filled up, or bridged over. It may be obscured by mists and clouds, but, though it be lost for a time, it is sure to be rediscovered and its limits studied by the curious and unphilosophical.

Nowadays analogical argument is employed very freely without any attempt to show, in the first place, that there is any real analogy between the facts upon which the reasoning is based. In order to convince people that a hypothetical gemmule may move long distances through all sorts of tissues, it is only necessary to show that actual matter, millions of times as large, does burrow a short distance through certain textures. Mr. Darwin remarks that it cannot be objected "that the gemmules could not pass through tissues or cell-walls, for the contents of each pollen grain have to pass through the coats both of the pollen tube and embryonic sack."

He might have advanced in his support the fact of fungi traversing tissues, of entozoa of various kinds burrowing long distances through the textures of the living body, and many well-known instances of a similar kind. But such facts do not strengthen the hypothesis of Pangenesis in the slightest degree. They were known before it was advanced, and the objection controverted has not been raised in the form indicated. We *know* that a thing infinitely larger than the hypothetical gemmule does pass through tissues, but do the gemmules really exist, and do they pass through? Certainly, if they exist, they *may* pass, but, as I have indicated, there are other matters invalidating the hypothesis besides the question of the gemmules traversing the tissues. Pangenetic gemmules might pass everywhere. They might leave the body, collect in the atmosphere and coalesce, and the compound particle formed might easily wriggle itself back again into the organism through the chinks between the cuticular cells. Such gemmules might move anywhere, up and down and in and out through any cell wall. They might pervade solids and fluids and gases. The pangenetic gemmule cannot be seen or tested, neither can its presence or absence be proved in any way. The phenomena adduced by Mr. Darwin in support of his hypothesis can be demonstrated; but the pangenetic gemmules are of the imagination alone, and the analogy between the actual facts and the supposed facts is surely but an analogy of the imagination. The facts alluded to no more support the pangenetic hypothesis than does the demonstration of living germs in the air support the hypothesis of life in the blue sky. It is possible to supply many arguments stronger than those adduced in support of the hypothesis, nay, perhaps, stronger than any Mr. Darwin himself has yet advanced in favour of Pangenesis; but yet other considerations appear to me greatly to preponderate against the acceptance of the doctrine. Mr. Darwin admits that "from presenting so many vulnerable points" the life of his hypothesis "is always in jeopardy;" but is it not this very jeopardy which lends interest and enchantment to many a hypothesis, and sustains it in the estimation of those who delight in conjectural information and scientific speculation?

LIONEL S. BEALE

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MR. DARWIN, in his letter to NATURE of April the 27th, says: "The fundamental laws of growth, reproduction, inheritance, &c., are so closely similar throughout the whole organic kingdom that the means by which the gemmules (assuming for the moment their existence) are diffused through the body, would probably be the same in all beings, therefore the means can hardly be diffusion through the blood." Now, if in the vegetable kingdom pangenetic gemmules are able freely to be "diffused" from cell to cell by endosmosis, we should expect that in the case of grafts, where certainly such diffusion goes on between the cells of the stock and the scion, a bud borne upon the graft would certainly be affected by the gemmules arising in the root and stem of the stock. Yet we all know that the pips from a pear grafted on a quince stock will not give rise to a hybrid between a pear and a quince, neither will the stone of a peach which has been grafted on a plum stock grow into a tree whose stock bears plums, while the extremities of its branches bear peaches.

A. C. RANYARD

### Noises at Sea off Greytown

IN NATURE, vol. ii. p. 25, Mr. Dennehy gave an interesting account of a peculiar vibration, accompanied by sound, which is perceivable at night on board *all* (?) *iron* steamers which anchor off Greytown, Central America; and in subsequent pages I have read with great interest various speculations as to its origin, which is ascribed (1, the probable solution) to troops of Sciænoids (with reservation) by Mr. Kingsley (p. 46); (2) to musical fish or shells, by Messrs. Evans and Lindsay (pp. 46 and 356); and (3) to gas-escape from vegetable mud and sand, by Mr. Malet (p. 47); whilst Mr. Dennehy himself suggests the possibility of some galvanic agency.

I remarked upon this vibratory phenomenon in a communication published in the *Field* newspaper of October 26th, 1867, signed "Ubique," after having heard it myself when on board the Royal Mail steamer *Danube* (Capt. Reeks) during the nights of the 12th, 13th, 14th, and 15th of May, 1867; the new moon occurring on the 4th of the same month. As my statement serves to confirm Mr. Dennehy's report, I may be forgiven for giving it in full.

After giving an account of the sudden appearance of a huge white shark in the deep sea when a man fell overboard, I proceeded to state as follows:—"On embarking on board the *Danube* steamer, lying at anchor in the roadstead off Greytown on the 12th May, 1867, I was informed that the ship was haunted by most curious noises at night since she had arrived, and that the superstitious black sailors were much frightened at what they thought must be a ghost. The captain and officers could make nothing of it, and it afforded a great matter for discussion. On inquiry I found out that other *iron* ships had been similarly affected. Curiously enough this noise was only heard at night, and at certain hours. Some attributed it to fish, suckers, turtle, &c., others to the change of tide or current; but no satisfactory conclusion could be arrived at. When night came on there was no mistake about the noise; it was quite loud enough to awaken me, and could be heard distinctly all over the ship. It was not dissimilar to the high monotone of an Æolian harp, and the noise was evidently caused by the vibration of the plates of the iron hull, which could be sensibly perceived to vibrate. What caused this peculiar vibration? Not the change of current and tide, because, if so, it would be heard by day. Like everything else that we cannot explain, I suppose we must put it down to electricity, magnetism, &c. If this should meet the eye of any of the officers of the above-mentioned steamer, or others who have noticed this phenomenon, I should be glad to hear whether this effect still continues, or if any satisfactory conclusion has yet been arrived at. I may add that from the hold of the vessel the grunts of the toad-fish could be distinctly heard. I hope that the above notice may lead to some answers from your various correspondents."

This brief notice drew forth a rejoinder from a correspondent (November 23, 1867) who had noticed a somewhat similar sound.

"The singular sound noticed by 'Ubique,' I have also heard without knowing its origin. One moonlight night in 1854, on board a steamer anchored near the Tavoy river (Tenasserim) we were struck by an extraordinary noise which appeared to proceed from the shore about a quarter of a mile off, or from the water in that direction. It was something like the sound of a stocking loom, but shriller, and lasted perhaps five or six seconds, producing a sensible concussion on the ear like the piercing scream of the cicada; and this gave an impression as if the vessel itself were trembling, or reverberating from the sound. One or two Burmans on board said simply, the noise was produced by 'fishes,' but of what kind they did not describe. It was repeated two or three times. I never heard it before or after the occasion referred to, nor have I ever met with any allusion to this singular phenomenon until I perused 'Ubique's' communication in the *Field* of the 26th ult. The steamer in my case, I should add, was a wooden one."

Mr. Evans, in his letter, speaks of the rapid silting up of Greytown harbour this still continues, and the passage over the bar, which is continually shifting, is often a matter of great difficulty, and indeed often so dangerous that the Royal Mail Company will not undertake to allow their own boats to land, and passengers have to land in the local canoes at their own risk. The Nicaraguan Government, however, propose to carry out Mr. Shepherd's plan of diverting the waters of the San Juan river from the Colorado mouth to the Greytown channel, hoping thereby to scour the harbour clear.

Mr. F. J. Evans also refers to the vast amount of animal life, and mentions the quantities of sharks and *alligators* which abound in and about Greytown Harbour. I can fully corroborate this, although I believe that what Mr. Evans terms *alligators* are really *crocodiles* (*Molinia Americana*), I should be glad to have certain information on this point: when not actually visible, their proximity is made evident by a powerful odour of musk. The most notable, however, of the denizens of these waters, besides the turtle, is the Atlantic manatee, which Columbus mistook for a mermaid, and which Agassiz terms the modern representative of the Dinotherium. The Mosquito Indians on the Indian, Rama, and Blewfields rivers are great adepts at harpooning this paradoxical mammal, and its flesh salted is a staple article of food all along these coasts, being not unlike to ship's pork.

Southsea, April 28 S. P. OLIVER

P.S.—When at anchor off Greytown, also in the *Danube* steamer, during the night of February 15, 1867, (moon eleven days old) there was no vibration or noise perceived, but then there was a tremendous swell breaking with high surf on the bar, and the vessel rolling heavily. It would be interesting to overhaul the logs of the Royal Mail Company's vessels which have been at Greytown, in order to discover the periods of these vibrations, but I am afraid that no observations have been recorded in their books.

## Mechanical Equivalence of Heat

YOU will see from the proceedings of the Literary and Philosophical Society at Manchester, that, since the discussion there, Dr. Joule has definitely abandoned the reasonings in his famous paper on the mechanical force of electro-magnetism, steam, and horses. I have now had time to test the facts and experiments of this new theory, and find it, as I hope soon to show in detail, as untenable as his former one. Indeed, I am sure that the mechanical equivalence of heat must soon be generally abandoned as inconsistent with facts. You will see that the April number of the "Review of Popular Science," has definitely pronounced a decision in my favour; and I am sure you will soon be convinced yourself that your own first reviewer of my article in the *Quarterly Journal of Science* was more reasonable than your second. H. HIGHTON

## Aurora by Daylight

AN additional well-authenticated instance of this very rare but indisputable phenomenon, may, perhaps, be thought worthy of insertion.

In the Transactions of the Royal Irish Academy for 1788 (embodied in "Memoirs of Science and the Arts," 1798), is "An Account of an Aurora Borealis seen in full Sunshine, by the Rev. Henry Ussher, D.D.," which opens in the ensuing manner:—

"The following phenomenon being very uncommon, if not entirely new, I think it worth communicating to the Academy, principally with a view to learn whether any other person has observed a similar one at any time:—

"'On Saturday night, May 24, 1788, there was a very bright aurora borealis, the coruscating rays of which united, as usual, in the pole of the dipping needle. I have always observed that an aurora borealis renders the stars remarkably unsteady in the telescope. The next morning, about eleven, finding the stars flutter much, I examined the state of the sky, and saw whitish rays ascending from every part of the horizon, all tending to the pole of the dipping needle, where at their union they formed a small thin and white canopy, similar to the luminous one exhibited by an aurora at night. These rays coruscated or shivered from the horizon to their point of union. These effects were distinctly seen by three different people, and their point of union marked separately by each of them.'"

T. W. WEBB

## The Coronal Rifts

THE enclosed extract of a letter from Captain Tupman, who observed the Eclipse of December last through the finder of Prof. Harkness's telescope at Syracuse, may interest some of your readers—

"It is a singluar feature in all the photographs that the 'rifts' are so wide and distinct. They are actinic rifts. As seen in the telescope simply the corona had no such rifts. I watched it during the whole 105 seconds; such a feature would, of course, have struck me instantly. I actually pointed Prof. Harkness's spectroscope *in* the rifts as being bright parts of the corona!"

A. C. RANYARD

## The Name "Britain"

As "C. L. N. F." has in your last well answered the letter of "A. R. H.," I have now only to reply to Mr. Hyde Clarke's letter, in which he says I should find it difficult in my derivation of "Britannia" and "tin" "to explain on the same basis the conformable names" of the countries and rivers which he mentions, inasmuch as "these names are not explainable in Phœnician, because they were given long before the Phœnicians entered on the stage of history."

His paper read before the Anthropological Institute I have not seen, but as "the learned" Bochart and other authors have considered the name "Britain" to have been derived from the tin which the Phœnicians exported from Cornwall more than 3,000 years ago (Num. xxxi. 22), and as no one will venture to say that "tin" was not then the name of this metal in the most ancient Cornish as well as in the Phœnician language, from which it proceeded, I do not think I can fairly be called upon to go into the "difficult" task suggested by Mr. Clarke.

The original name of our island I have imagined to be *Bretin* ("Tin Mount"), that being at first exclusively the name of the *mount* from which the Cornish *tin* was exported by the Phœnicians, and it is highly probable that the same name was afterwards given by these ancient traders to the entire island, of which the mount was only a part, for it was Britain that gave them nearly all their *tin*, and its most beautiful natural object known to them was St. Michael's *Mount*.

There being other islands close to Britain, the Romans gave the name *Britanniæ* indiscriminately to them all. When they spoke of Britain as dissociated from its contiguous islands, they called it either *Britannia* or *Insula Britannica*, which is synonymous with *νῆσος* Βρεττανικη. This word, Βρεττανικη, used at first adjectively by the Greeks, had in the time of Diodorus Siculus become a substantive, so that he uses it as such when describing the daily insulated port or mount called sometimes *Iktin* (Tin Port), and sometimes *Bretin* (Tin Mount), adjacent to Βρεττανικη, to which *port* or *mount* at low water the tin was carried from the mainland for sale and exportation. The following is the passage:—*εἰς την νησον προκειμενην μεν της* Βρεττανικης *ὀνομαζομενην δε* Ἰκτιν.

Plymouth, May 6 RICHARD EDMONDS

*** We cannot print any more letters on this subject.—ED.

## The Sensation of Colour

PROF. CLERK MAXWELL in his valuable paper on Colour in NATURE (vol. iv. p. 13) commits himself to the opinion that there must be three distinct sets of retinal nerves, one for each of the three primary sensations of colour. It is obvious that demonstrative proof or disproof of this is unattainable: we can only reason analogically. The analogy of the ear is in favour of such an opinion, so far as it goes; for there appears to be proof, or probability almost amounting to proof, that sounds of different pitch are conveyed to the brain by different nerves. But the ear resembles the other organs of sense less than they resemble each other; and there is surely no reason for thinking that there are distinct nerves of smell for every distinct kind of smell, or distinct nerves of taste for every distinct kind of taste. Nor I believe is there the slightest proof of nerves for the sensation of heat distinct from those of touch.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, May 8

P.S.—I am not now at home. I intend to write in reply to Mr. Laughton's important letter on the Prevalence of West Winds, when I am at home and have the file of NATURE and other authorities to refer to.

## The Cave-Lion in the Peat of Holderness

WHILST engaged in the task of rearranging this Museum, I have been impressed with the value of two specimens in the Palæontological collection.

One is labelled "No. 7, *Felis*—metatarsal inner (Right side)," the other one is "*Felis* — 14. Femur Right side." There is no record in the catalogue by whom they were presented, nor of any of the circumstances of their *gisement*. The specimens, in fact, have no history whatever, and I can only say that I found them in close juxtaposition with a large series of red-deer bones from Holderness, with which they perfectly agree in their mineralogical condition. I have no doubt that they are *bonâ fide* from the Holderness Peat.

Their identification as bones of *F. leo* (variety *spelæa*) is also certain.

Hull Royal Institution C. CARTER BLAKE

### Eozoön Canadense

SINCE reading some of the communications on the Eozoön, which have appeared from time to time in NATURE, I have felt constrained briefly to give the results of my examination of the "Eozoic" limestone in Eastern Massachusetts. I am the more disposed to do this, hoping that a new line of investigation will be suggested to observers in other localities.

Last autumn I visited for the first time the quarries of "Eozoön" limestone in Chelmsford, under the guidance of my friend Mr. Burbank, of Lowell, Massachusetts, who has furnished many microscopists with specimens for sections. Having been long engaged in the study of the foliated series of rocks, and having years ago discovered indubitable evidence that portions of the included limestone are of vaporous origin, I was prepared to recognise the same feature in the Chelmsford "Eozoic" rock. I was accordingly not surprised on examination to find, what the advocates of the organic nature of the Eozoön seem never to have suspected, that the limestone in question is not a "sedimentary rock;" that it occupies, or rather occupied, (for it has been for the most part removed) pockets or oven-shaped cavities, which were once plainly overarched by gneiss; that it is foliated, there being a regular succession of leaf-like layers from the walls toward the centres of the cavities, witness to which is borne by a like succession of different minerals; that in some places it ramifies the surrounding rock in a vein-like way, while in others it exactly conforms with the most abrupt irregularities of surface; that in one locality, which I have repeatedly examined, it conforms with the uneven portions of a mass of syenite, with which it is so associated as to reveal its more recent origin; and that, therefore, it is not of nummulitic derivation, but was deposited in a vein-like form, the materials having been probably forced up into the cavities from below while in a vaporous state.

Such, in few words, is the result of my examination—a result which tends to show that the "Eozoön" of Eastern Massachusets is not organic, and that thus it belongs to the department of Mineralogy, and not to that of Palæontology. Waving additional particulars for the present, I may simply add that I propose in due time to give a detailed exposition of the relations of this famous "Eozoic" rock.

Cambridge, Mass., April 15 JOHN B. PERRY

## *THICKNESS OF THE EARTH'S CRUST*

I SEE that at p. 296 of your journal for February last, which has recently reached Calcutta, you print a lecture by Mr. David Forbes "On the Nature of the Earth's Interior," in which reference is made to the Mr. W. Hopkins's method of determining whether the thickness of the earth's crust is great or small when compared with the whole radius, and to M. Delaunay's objection to it.

The lecturer refers to me as having approved of Mr. Hopkins's method, which I always have done and do still, and then makes the following apparently crushing remarks to annihilate Mr. Hopkins and all who approve of his method and of the result to which it leads, viz., that the crust is very thick. He says:—"M. Delaunay, an authority equally eminent as a mathematician and an astronomer, was induced to undertake the reconsideration of the problem; a labour (!) which has resulted in altogether reversing the above decision and demonstrating the complete fallacy of the premises upon which so much elaborate reasoning had been expended."

As the lecturer had condescended to mention my name in connection with the subject, I wonder why he has taken no notice of my letter in reply to M. Delaunay, which was printed in your journal for July 1870, six months before the lecture was delivered, and which also appeared about the same time in the *Philosophical Magazine* and the *Geological Magazine*. In this I showed that M. Delaunay had evidently misconceived the problem, and that Mr. Hopkins's method is altogether unaffected by his remarks.

So much has been said about profound mathematical calculations in connection with Mr. Hopkins's investigation, that I conceive many have shrunk from attempting to understand the question at issue, from a feeling that they would not be able to comprehend it were they to attempt to do so. But this is quite a mistake. Anyone with an ordinary degree of knowledge of popular astronomy and of mechanical action is quite competent to form a good opinion on the point in dispute. What Mr. Hopkins did may be divided into two parts. He first conceived an idea, which was to be the basis of his calculation; and then he made his calculation. It is the *calculation* that calls for the "profound mathematics." But it is not this that is the matter of dispute. It is the *idea*, on which the calculation is based, which M. Delaunay calls in question.

I think I can make the matter sufficiently plain to your readers to enable them to form their own opinion.

Everyone having a knowledge of popular astronomy is aware that the earth revolves round an axis, which is fixed in the earth's solid crust, but shifts very slowly in space, producing what has been known ever since the days of Hipparchus by the name Precession. On this fact as his ground-work Mr. Hopkins reasoned as follows; and so got to his *idea*, which formed the basis of his calculation. Suppose the earth has a solid crust, the interior being filled up with fluid. If the axis remained steady in space and the crust revolved round it uniformly, no doubt, although the crust and fluid may have moved differently at one time, yet in the lapse of ages friction and viscosity in the fluid would cause the fluid at last to revolve with the crust just as if the whole were one solid mass. This being the case, suppose a slight horizontal push is given to the two poles, in opposite directions, so as slightly to shift the axis in space; what would happen? The revolving crust, by this new and additional motion, would slip over the surface of the revolving fluid, through a small space proportionate to the push given to the poles. The fluid could not possibly acquire in an instant this new motion, however small it might be, because the fluid is not rigidly connected with the crust. Suppose a second, and a third, and a succession of slight horizontal pushes to be given to the poles in a continually altering direction, the effect will be that the revolving crust will be continually slipping over the revolving fluid which has not time to acquire these new motions given instantaneously to the solid crust. These successive slight pushes given to the poles, and so to the solid crust, represent the unceasing action upon the crust of the force which causes the motion of precession in the earth's axis, and arises from the attraction of the sun and moon on the protuberant parts of the earth about the equator.

Mr. Hopkins having reasoned thus far, went a step farther, and so came to his fundamental idea. He saw that the thinner the crust the smaller would be the mass which the disturbing force producing precession would have to move, and therefore the greater would be the motion caused, that is, the precession. Here, then, he discerned a connecting link between the amount of precession of the earth's axis and the thickness of the earth's crust. This was the *idea* I have alluded to.

Starting from this idea he entered upon a profound calculation and obtained a formula, which gives the thickness in terms of the amount of precession. This amount is a matter of observation; and the thickness can therefore be deduced by the formula from the observed precession. It is, as I have already said, not this calculation which is called in question by M. Delaunay, but the fundamental idea.

M. Delaunay says the fluid will have precisely the same motion as the crust; and that, because the new motion of the crust is so slow. But it is clear that its slowness has nothing to do with the matter. The fact is that the fluid and the crust not being connected together by any solid connection, no motion, whether small (*i.e.* slow) or not, can be suddenly communicated from the crust to the fluid mass. If the crust moved uniformly, as I have already said, and around a steady axis, the fluid might, after a lapse of ages, by friction and viscosity, acquire the motion of the crust. But if the crust is continually shifting from this steady position, however slowly, the fluid cannot suddenly acquire the new motion, and the crust slips over it; and the thicker or thinner the crust, the greater or less is the solid mass to be shifted, and the less or the greater the precession produced. If the internal mass obeys at once the shifting motions of the crust, that mass cannot be fluid, but must be solid, and have a solid connection with the crust; in which case the whole question is yielded.

Mr. David Forbes speaks of the "labour" M. Delaunay has gone through in giving vent to his opinion. If the thing *done* is to be measured at all by the thing *said*, his labour must have been infinite; for what he has said is an impossibility. He has evidently altogether mistaken the problem. Mr. Hopkins's method stands unimpaired by his criticisms. Indeed Mr. Hopkins was not a man to advance a theory which could be apparently set aside by such slender means. JOHN H. PRATT

## A THEORY OF A NERVOUS ATMOSPHERE

UNDER the above title, Dr. Richardson, in a lecture published in the *Medical Times and Gazette* of last week, suggests a new theory in respect to nervous function. We propose in a few sentences to state simply the meaning of this theory.

The earlier physiological writers on the functions of the nervous system were under the impression that the brain, spinal cord, and other nervous centres acted after the manner of glands, and produced or secreted, as they said, a liquid. They called this assumed secreted liquid the nervous fluid, and they considered that it charges the nervous system, some also supposing that it makes even a circulation through tubular nervous channels or canals. It was not an uncommon notion that the nervous fluid conveys nourishment to the organs of the body; but the most common, and indeed generally accepted, hypothesis was, that it acts as a means of communication between all parts of the nervous system, and is the communicating medium of the impressions and motions derived from the outer world. Attempts were made to measure the rate of motion through this fluid, how long it took to convey an impression by it from brain to muscle.

The discovery of frictional electricity, the special discovery of the electric shock by Cuneus, of Leyden, in 1746, and the after discovery by Galvani of the inductive action of the prime conductor of the electrical machine on the muscles of frogs, threw quickly into the shade the speculations of the earlier neuro-physiologists. It was assumed at once that there exists a true animal electricity, that there is production of electrical action within the bodies of all living animals, that there is conduction, and, in short, every mechanism and method for the carrying on, if we may so say, of electrical life. The discovery of the electrical organs of the torpedo, the dissection of the animal, the descriptions of its nerves by John Hunter, and the experiments made by a very earnest investigator, Mr. Walsh, aided greatly to establish the hypothesis which Galvani and his followers advanced, and which Volta, with the whole force of his experimental argument, failed to demolish.

Of late years the old hypothesis of the nervous fluid has been lost altogether, while the electrical hypothesis infinitely varied from its original and simple character, and infinitely varying with every new step of electrical discovery, has in a certain sense retained its popular hold. It is true the hypothesis has rested on so much laboured obscurity that nobody has succeeded in making out of it a demonstration like the demonstration of the circulation of the blood, and no one has made it so simple that every scholar can read it when it is written, and every medical practitioner practise by it and act upon it as a known principle. It is true that since the time when Volta gave his undeniable proofs against the truth of the first inferences of Galvani, the best and most thoughtful philosophers have felt doubts as to the electrical character of living action, and have looked on Galvani's construction of life as a beautiful crumbling ruin rather than as a temple befitting the worship of the gods of nature; and, lastly, it is true that whoever takes up to read the tomes or volumes of the most eminent writers on the subject of animal electricity is prone to lay them down again as he would the handles of a battery that master his will without appealing to his reason. All this is quite true; but still the electrical hypothesis has, as we before said, held its place; no attempt has been made to replace it; it has maintained around it a spell of fascination.

The theory that has been suggested by Dr. Richardson is in some sense a return to the old view respecting nervous action, and in some sense also is an extension to the nervous system of the physical idea of communication of motion by molecular disturbance. In a few words, the author of the theory supposes that the blood, as it circulates in the vessels on which the structures of the body are constructed, yields a diffusible vapour or atmosphere which charges the nervous system surrounding the molecules of nervous matter and pervading the whole nervous organism. He attempts to formulate the physical qualities of this vapour; it is probably an organic vapour containing carbon, hydrogen, and nitrogen; it is insoluble in blood, it is condensible by cold, diffusible by heat; it is retained after death longer in cold-blooded animals than in warm-blooded, and longer in warm-blooded animals that have died in cold than in those that have died in heat; it possesses conducting power, and as a physical substance is susceptible of variation of pressure; it connects the nervous system in all its parts together; it is the medium of communication during life between the outer and the inner existence; by the organs of the senses the impressions and motions derived from the outer world are vibrated into or through the nervous atmosphere to the brain; in the living and healthy animal the nervous ether, if we may so designate it, is in correct tension, in the feeble it is diminished, in the dead it is absent or inactive; in the waking times of the living it is most active; it may be used up faster than it is produced during exercise; it is renewed during sleep.

On the supposition of the existence of a nervous ether or atmosphere as thus suggested, the author of the theory accounts for various phenomena connected with the partial or complete destruction of conscious, and even of organic life. The action of narcotic vapours is an illustration in point. It is assumed that these vapours—vapours of chloroform or alcohol, for example—taken into the blood and carried to the nervous system, become diffused through the nervous atmosphere, and by their presence interfere with its physical qualities and thus obscure function. "The foreign vapour that has been introduced benumbs; in other words, it interferes with the physical conduction of impressions through what should be the cloudless atmosphere between the outer and the inner existence."

Carrying out in a different way the same line of thought, the author of the theory to which we have specially called attention, accounts for the diffusion of some poisons through the body and for that rapid action of certain poisonous substances which so many experimenters have endeavoured, but not successfully, to explain; further, he suggests that in some instances poisonous products of decomposition generated within the body itself, in disease, may be diffused through the nervous ether, and that the sudden collapse of nervous function, which is often seen in acute disease, may be due to this cause. Finally, there may be conditions of disease in which there is unnatural tension of the nervous atmosphere, followed by disturbance of muscular motion, convulsion, or cerebral pressure, leading to apoplectic insensibili y.

We have sketched out thus briefly the leading points of this theory of a nervous atmosphere or ether produced, during life, within and by the living organism, as a theory calculated to give rise to much discussion and device of new experiment.

## *ASTRONOMICAL OBSERVATION*

THE statistics of modern astronomical observation would, we suspect, be very curious, if it were possible to get at them. A report showing the gradual increase in the number of telescopes manufactured during the last fifty years would be very interesting; and so would be a table comprising at once the advance in their dimensions and the diminution in their cost. The result would, we believe, be such as at first sight to cause great surprise among those unacquainted with the subject, or those whose recollection does not go back to days when five nches was as extraordinary an aperture for an object-glass, as double that size is now. But the value of these, as of other tabular statistics, would suffer material abatement, if they were applied to establish any other conclusions than those to which they directly lead. For instance they would probably be fallacious, if considered as inferring a proportionate increase in the number of important observations. In order to bring out such a result, we require, so to speak, another factor, and a very essential one—a corresponding increase in the number of competent observers. This, we fear, may not have been commensurate with the advance of optical means: at least, except upon the supposition of some such deficiency, it is difficult to understand what becomes of the multitude of really good object-glasses which are annually produced, not only in England, but in Germany and America. A large proportion of these, we are led to think, must be purchased to be looked at, and not looked through: or handled as mere toys for the amusement of people who do not know what to do with themselves in an idle evening. This was not so much the case in the early days of telescope-manufacture. The greatest master of figuring specula in his own time was also the greatest proficient in using them: it is needless to add the name of Sir William Herschel. And so the finest reflectors in Germany were placed at the same period in the hands of the leader of all accurate selenographical investigation, J. H. Schröter. These were "the right men in the right place." Even then, it may be said, many noble reflectors went, no one knows where, the greater part of them long before this time useless from tarnish, or, still more mortifying to think upon, ruined by unskilful repolishing. Still, admitting this, the disappearance of powerful instruments does not seem to have been so remarkable in those days as it is now, and the quantity of really valuable observations appears to have been greater in the end of the last and the early part of the present century, in proportion to the means of observing.

This is not a very encouraging view of the present state of this branch of astronomy. But, if well founded, as we believe it to be, we might expect that there would be some assignable reasons for it; and, in fact, several are sufficiently obvious. One certainly is, that the process of discovery is not, generally speaking, renewable. What has been once detected is usually placed on record, in bar of all future claims. So it has been in the science of music; a man might arise among us with the fervid genius of Handel, but he could not write the Hallelujah Chorus over again; and doubtless the spirit of Mendelssohn must have been cramped by the impossibility of employing many of the noblest and most impressive subjects which had been anticipated by his predecessors. And so it has been in the researches of geography. The enterprising explorer has now to go much farther in pursuit of "fresh woods and pastures new," and every Alpine season is so rapidly narrowing the number of summits untrodden by the foot of man, that the excitement of a first ascent will soon have to be sought in remoter regions. Thus in astronomy, though it cannot be said that there are no worlds left to conquer, yet all the larger and more conspicuous features of the heavenly bodies have been long ago so fully noted and recorded, that what remains for exploration is chiefly of that delicate character which, without being the less interesting from its minuteness, is less accessible, for that reason, to the possessors of ordinary instruments. And on this account many a student who might well have risen from the ranks in the earlier days of scientific campaigning, is now compelled to remain in comparative obscurity—a mere spectator, when he might well have taken his place among the discoverers of fifty years ago.

Another reason why tools have multiplied without a corresponding increase of good work, may be this, that looking upon the observer and his instrument as a complex apparatus, the improvement of the intelligent has not kept pace with that of the material part. In fact, it is impossible that it should. The eye is but what it was when David learned humility from considering God's heavens, the work of His fingers, the moon and the stars which He hath ordained; the intellect, though more developed and cultivated, is not more strong and piercing than it was in the days of Hipparchus; man does much more with his brain, but he has no more brain to do it with, than his uncivilised ancestors; and observers may, and will be, collectively multiplied without being individually improved. Every man that has eyes does not know how to use them; or, not failing in this respect, he may lack other requisites: he may not know what to look for, or where to find it; or he may be deficient in his handling of the faithful pencil or the expressive pen. And so it comes to pass that the capacities of instruments may be much in advance of the abilities of those who use them.

Besides all this, there is a physical obstacle of an entirely different character, which must not be forgotten; the unimprovable constitution of our own atmosphere. This will ever be a sore subject for the zealous observer, especially among ourselves. If even Secchi finds fault with the glorious Roman heavens, what have we not to regret in our own murky, and fuzzy, and restless skies? Who that has read the most graphic as well as instructive writings of Sir J. Herschel is likely to forget his complaints of "twitching, twirling, wrinkling, and horrible moulding?" and who that has had much actual experience of observatory work will not endorse all this with a very lively fellow-feeling? The nights may easily be numbered, during a long season, in which the defects of the atmosphere do not overlie those of the instrument, and when the observer has not rather to wish that he could see all that his telescope could show him, than to long for greater power or light, to be expended in making atmospheric disturbances yet more conspicuous and prejudicial. The only way to obviate this grievous hindrance is to get above it; and no man has yet done this except Professor Piazzi Smyth in his most successful "Experiment"; it was said, indeed, that the French observers were about to follow his example, and to plant their instruments on the Pic du Midi de Bigorre; but we have never heard whether the idea has been carried into execution. And, however striking may be the advantage of such a plan, it must ever be confined to a favoured few.

We have dwelt at some length on a view of the present state of astronomical observation, which, though rather unfavourable, we believe to be substantially true. But it is not to be inferred that this is its sole aspect. There are, as usual, two sides to the shield; and much is to be said that is of an opposite tendency. If, for instance, we have asserted that for some time past observers have not multiplied in proportion to the means of observation, this is but a relative statement; the absolute fact is that at no former period has there been so numerous, or so zealous, or on the whole so competent a band of astronomical students. And of this we have a very pleasing evidence in the recent formation of an astronomical society expressly devoted to physical observation, to which we cordially wish success. If again it is probable that not many of the great discoveries are left within the reach of ordinary instruments, it should not be forgotten that many telescopes of very superior character are now housed in private observatories; and that for them investigations are still reserved, whose delicacy is no bar to their importance, and which may be undertaken with a hope of success no longer chargeable with extravagance. Great cabinets may be unlocked by little keys. Minute researches may give the clue to discoveries of the broadest extent and deepest interest. The changes of the lunar surface, the internal motion of starry clusters; the parallax and fixity of nebulæ; the planetary attendants on the brighter stars, these are mere specimens of the magnificent arcana, whose solution may not be denied to human energy and perseverance. We may remember, too, that if the telescope and the micrometer should be found unequal to the task, we have yet a new and most powerful method of investigation, the results of which are equally important and surprising—spectrum-analysis. The revelations of this beautiful invention may be said to be only beginning, and no man can foresee their end. What has already been done would have appeared as improbable as the reveries of Kepler, had it been predicted fifty years ago; and who shall say what may be the result of fifty years more of patient and energetic application? And what might not Kepler have said and done, had such an instrument of research been placed in his hands? We may suppose how his fervid imagination would have exulted in the prospects, and with what confident joy he would have repeated the memorable words which characterise one of his lofty aspirations, "Plus ultra est."

## *NOTES*

IT is stated that the Astronomer Royal is to have the honour of a K.C.B. conferred upon him in recognition of his services in respect to the International Exhibition. We trust this rumour is not strictly correct; for unless it is to be generally understood that services are to be rewarded in the inverse ratio of their value, it is simply grotesque and unbecoming of the Government to ignore all the Astronomer Royal's services to Science, and all his unpaid services to the State in connection with subjects more important to the nation than all the exhibitions which ever have been or ever will be.

IN a Congregation to be held at Oxford on Tuesday, May 23, three forms of statute will be promulgated on the subject of the Second or Final Examination. It is proposed to have one Pass School of a mixed character and six Honour Schools. In the Pass School the examination is to be divided into three groups, as follows:—Group A.—1. One Latin and one Greek author, one at least of which shall be a philosopher or an historian. 2. The outline of Greek and Roman history, with a special period of one or the other, and English composition. Group B.—1. Either English History and a period or subject of English Literature, or a period of Modern European History with Political and Descriptive Geography, together (in each case) with English composition. 2. A Modern Language, either French or German, including composition in the language and a period of its literature. 3. The Elements of Political Economy. 4. A branch of Legal study. Group C.—1. The Elements of Geometry, including Geometrical Trigonometry. 2. The Elements of Mechanics, solid and fluid, treated mathematically. 3. The Elements of Chymistry, with an elementary practical examination. 4. The Elements of Physics, not necessarily treated mathematically. Every candidate is to select two subjects from one of these groups, and one of another of them, and must pass in all three; but may present himself for each of the three subjects in separate Terms. The six Honour Schools are to be:—1, Literæ Humaniores; 2, Mathematics; 3, Natural Science; 4, Jurisprudence; 5, Modern History; and 6, Theology. The examination in the Honour School of Literæ Humaniores is to include Philology, Ancient History, and Philosophy:—1, In Philology, the Greek and Latin languages; 2, in Ancient History, the histories of ancient Greece and Rome; 3, in Philosophy, Logic, the History of Philosophy, and the outlines of Moral and Political Philosophy, each candidate being required to offer at the least two treatises by ancient authors. Candidates shall be permitted to offer in addition, as special subjects, one or more authors or portions of authors, or departments, or periods falling within or usually studied in connection with any of the stated subjects of this school. For the purpose of this provision philology shall be taken to include textual criticism, the minute critical study of authors or portions of authors, the history of ancient literature, and comparative philology as illustrating the Greek and Latin languages, and ancient history shall be taken to include classical archæology and art, and the law of Greece and Rome.

IT is with very great pleasure that we print the following intelligence of the safety of Dr. Livingstone:—Despatches were received last week at the Foreign Office from Dr. Kirk, the Acting British Consul at Zanzibar, containing information of the safety of Dr. Livingstone in October last. The doctor was then at Manakoso, helpless, without means, and with few followers. Dr. Kirk had sent him supplies to meet his immediate necessities, which, it was hoped, would shortly reach him.

AT the annual meeting of Convocation of the University of London, held on Tuesday last, Dr. E. A. Parkes was chosen by a very large majority at the head of the list of three graduates, to be submited to Her Majesty for selection therefrom of a member of the Senate in the place of the late Dr. W. A. Miller. At the same meeting a resolution proposed by Dr. Francis T. Bond, that it is expedient to retain Greek in the Matriculation Examination only as an optional subject, was rejected by a small majority.

THE example set by Clifton College in the formation of a botanic garden in connection with the Natural History Society is, we understand, about to be followed at Marlborough, a plot of ground having been granted by the authorities for that purpose. Such a garden will be a valuable adjunct to the herbarium, if such plants are selected as are typical of the principal natural orders, especially of those which are sparingly represented in the British flora.

THE following appointments have been made in consequence of the death of Prof. Miquel:—Dr. N. W. P. Rauwenhoff to be Professor of Botany and Director of the Botanic Garden at Utrecht. Dr. W. F. R. Suringar to be Professor of Botany and Director of the Botanic Garden at Leyden.

THE *Botanische Zeitung* records the death on March 23 of Dr. Schultz-Schultzenstein, of Berlin, well known as a copious writer on vegetable morphology and physiology.

MR. F. M. BALFOUR, late of Harrow, and Mr. P. H. Carpenter, of the Royal School of Mines and University College, London, have been elected to foundation Scholarships at Trinity College, Cambridge, for proficiency in Natural Science.

WE regret to have to record the death of Mr. James Yates, at his residence at Highgate, on the 7th inst. He was a prominent member of the Royal and Geological Societies, and of late years had been best known as one of the most active advocates of the introduction of the Metric System of Weights and Measures.

THE following gentlemen have been placed in the first class of the annual examination in Natural Sciences in St. John's College, Cambridge (order alphabetical) : —Edmunds, Garrod, Read, Sollas, Yule.

THE annual *conversazione* of the members of the Society of Arts will be held on Thursday, June 1, at the South Kensington Museum.

M. ELISEE RECLUS, a very active contributor to the *Revue des Deux Mondes*, has been appointed director of the National Library in Paris, to fill the room of M. Taschereau, who has left for Versailles.

PROF. WYVILLE THOMSON delivered, on the 2nd inst., his inaugural lecture to the students of the Natural History class in the University of Edinburgh. In the course of his observations he paid a glowing tribute to the services of his predecessor, Dr. Allman, whose valuable researches in zoology will continue to be prosecuted in spite of his retirement from the chair.

WE regret to learn from *Harper's Weekly* that at the great fire which recently destroyed the printing office of Weed, Parsons, and Co. in Albany, the edition printed of the Twenty-fourth Report of the New York State Cabinet of Natural History was entirely destroyed. Fortunately a nearly complete copy of the revised proof was saved ; so that no serious difficulty will be experienced beyond considerable delay, although the loss to the State in the destruction of fifteen thousand impressions of plates, &c., will be considerable.

THE continuation of the exhaustive work of Bronn on the classes and orders of the animal kingdom contains an elaborate memoir upon the anatomy of birds, and several numbers are devoted to the peculiarities of the muscular structure alone.

AT the meeting of the Boston Society of Natural History for March 1, the principal communication was one by Mr. George Sceva, in which attention was called to the fact of the shortness of the upper jaws in the skulls of the Hindoos, and the frequent absence of the third molar. This generalisation was based upon the examination of a number of crania ; and it was found that about fifteen per cent. of the whole exhibit this peculiarity, while in an extensive series of skulls of European races only about one per cent. showed the same feature.

AT the annual meeting of the Chicago Academy of Sciences, held on April 11, various communications upon a variety of subjects of interest were presented. The most important paper read was one by Colonel Foster, upon the subject of Artesian Wells, in which an account was given of the principal borings that have been attempted in the West, with a statement of their geological relationships, and the depth to which they were carried.

THE Rugby School Natural History Society has just published its Fourth Report for 1870. Rugby having to so great an extent taken the lead among our public schools in its cultivation of Natural Science, we looked for this Report with special interest, and have not been disappointed in its value. There is an interesting paper by the President, the Rev. F. E. Kitchener, on the Times of Flowering of Plants, containing just that record of facts and minute observations which it is one of the special functions of local natural history societies to collect. It is illustrated by two plates, in which are delineated curves representing the average forwardness of flowering in the spring and early summer months of 1867, 68, 69, and 70, contrasted with other curves representing the rainfall and temperature. Other illustrated papers are by the Rev. T. N. Hutchinson on Sun-spots, and Mr. C. H. Hinton on the Mechanism of a Crane's Leg. We learn that the society now possesses a museum of its own, and has just acquired cases for its botanical and entomological collections. We are glad to see that the officers, in their report, lay great stress on the importance of completing the local collections.

IN the Proceedings of the Cotteswold Naturalists' Field Club for 1870, the President, Sir W. V. Guise, Bart., calls attention, in his Annual Address, to the unusual interest and importance of two papers which occupy nearly the whole of the volume—The Gravels of the Severn, Avon, and Evenlode, and their extension over the Cotteswold Hills, by Mr. W. C. Lucy ; and On the Correlation of the Jurassic Rocks in the department of the Côte-d'Or, France, with the Oolitic formations in the counties of Gloucester and Wilts, by Dr. Thomas Wright. The terms in which these papers are referred to by the president are thoroughly well deserved, and the Club is doing great service to science in their publication. Mr. Lucy's paper is the result of four years' labour, and is copiously illustrated by numerous sections, and a large coloured map showing the surface geology of the country between Evesham, Chipping Norton, Gloucester, and Cirencester. The work is most creditable to the club, and renders this volume of its Transactions indispensable to anyone studying the geology of the western counties.

THE Proceedings and Transactions of the Nova Scotian Institute of Natural Science for 1869-70 lies on our table, and we may take this opportunity of acknowledging the great service rendered by this society in the elucidation of the natural history o- our American colonies. Among the papers in the present number we may mention especially the continuance of Dr. Bernard Gilpin's series on the Mammalia of Nova Scotia, a monograph by Dr. Lawson of the Ranunculaceæ of Canada and adjacent parts of British America : a paper by the President, Mr. J. M. Jones, on the Laridæ of the Nova Scotian coast ; and a record of Meteorological Observations for 1869, by Mr. Henry Poole.

THE Botanical Exchange Club has just issued its Report for the current year, signed by its indefatigable curator Dr. J. Boswell-Syme. It is chiefly occupied by observations on certain critical sub-species or varieties gathered by the members, no absolutely distinct indigenous species having been added to the British flora during the year. We are glad to see a considerable increase in the number of members of this useful society.

WE have received the Report of Observations made by the members of the Observing Astronomical Society for 1869-70. A considerable proportion of these observations has already been reported in our columns. In addition, the Report includes Hints and Suggestions on the Observation of Lunar Objects by Mr. W. R. Birt, and three drawings of the Bands of Jupiter on Oct. 6, and Nov. 1 and 24, 1869. We are glad to see that the number of members has increased to fifty, and congratulate the society on the good work it is doing.

IN a recent number of the "Proceedings of the Asiatic Society of Bengal," we have a report of the Address of the President, the Hon. Mr. Justice Phear, a considerable portion of which is devoted to a statement of the importance of a systematic series of barometrical observations in India. Mr. Phear points out that in India we possess almost unrivalled opportunities for examining and analysing the atmospheric column in all its parts, and that India proper, the Bay of Bengal and Burma, constitute a region which, for the purposes of one branch at least of meteorological science, demands to be taken and treated as a whole. The actual state of the case, on the other hand, is that for administrative purposes, British India is divided into eight principal districts or provinces, viz., Bengal, Madras, Bombay, N.W. Provinces, Oude, Panjab, Central Provinces, and Burma; and in each of these, except Burma, is a separate local system of observation, with its own independent head, and very little communication with one another.

ON the 2nd May, two days before the full moon, a complete lunar halo was observed at Clifton by Mr. George F. Burder, and described fully in the *Times* of the 4th. Mr. Burder saw the two halos, the large one and the small one, the larger being very difficult to be seen. A paraselenic circle, having the zenith for its centre, was also observed, and mock moons, or *paraselenes*, four in number, were seen at the intersection of the halo with the paraselenic circle. This appearance, as usual, was followed by very bad weather. It is produced, as demonstrated by the French natural philosophers of the eighteenth century, by floating particles of ice; and the light from the moon being considerable, the phenomenon was observed in all its glory.

MR. T. LOGIN's reports on the experimental cultivation of cotton at Camp Bahalgurh in the Valley of the Jumna, are exceedingly satisfactory. Although the crops were damaged by floods and by late frosts, the yield of clean cotton has been at the rate of 307½ lbs. per acre, or from four to five times the average yield in India. Mr. Login attributes this result in great measure to his practice of irrigating the fields in the afternoon or night, rather than in the mid-day, believing that the combined action of light and heat on stagnant water makes it under almost all circumstances injurious to plants.

OF the many fresh-water fish characteristic of the continent of North America, comparatively few, with the exception of members of the salmon and trout family, are of sufficient economical value to make it expedient to introduce them into regions where, they do not naturally occur. This transfer has been made to a very disastrous extent in the case of the pike (*Esox*), which although multiplying rapidly, is at the same time the determined foe of all other kinds of fish, and soon almost exterminates them from the waters which it inhabits. For this reason, some States have passed laws prohibiting, under severe penalties, except by direct permission of the Commissioners of the Fisheries, any transfer of the species in question to new localities. There is, however, one fish that is of great value, and which can be introduced without as much doubt of the propriety of the act as exists in regard to the pike. We refer to the black bass (*Grystes salmoides*). This inhabits, in one variety or another, the basin of the great lakes of the Mississippi Valley, and the upper waters of the streams of the south Atlantic coast as far north as the James River. Within a few years it has been transferred with success to streams previously uninhabited by it—to the Potomac, for one, where it is now extremely abundant. During the past summer some public-spirited gentlemen of Philadelphia collected among themselves a fund to stock the Delaware with this noble fish, and obtained about seven hundred, principally in the vicinity of Harper's Ferry. These were carried alive in large tanks to the Delaware, and deposited in that stream at Easton, about two hundred of the number dying by the way. The same party of gentlemen propose to use a surplus fund in their hands in experimenting upon the restocking of the river with shad and salmon.

THE white sugarcane of Cuba has been tried in Columbia and found more productive than the local variety called Cinta.

## REPORT ON THE DESERT OF THE TIH*

THE following report has been sent to the Vice-Chancellor of Cambridge by Mr. C. F. Tyrwhitt Drake, who received a grant from the University for the purpose of investigating the natural history of the Tîh. He spent several months in the district, accompanied by Mr. E. H. Palmer (late of the Sinai Survey), who was travelling on behalf of the Palestine Exploration Fund:—

I have now the honour to lay before you a report of my work during last winter in the "Badiet et Tîh," or Wilderness of the Wandering. As this desert had been only partially, and even then superficially examined, I shall give, firstly, a short account of the route we took and of the general physical features of the country; and, secondly, the various traditions of beasts and birds which are current amongst the Arabs. Many of these are curious, from their similarity to Western tales; and others, though seemingly foolish in themselves, are not without interest, as illustrating the beliefs and folk-lore of the Bedawín. These stories are not so numerous as I found them to be in former journeys amongst Arabs inhabiting more fertile tracts, for the Desert of the Tîh is in truth "a great and terrible wilderness." The last winter, too, was one of unusual drought even in those parched regions, and the scattered tribes of Arabs who live there experienced great difficulty in finding pasture for the herds of camels and goats which exist in considerable numbers in some districts.

The supply of water is very scanty and variable, as springs are extremely rare, and most of the water is obtained from "Themail," or pits dug in the gravelly beds of wadies, and similar situations into which the water filtrates. The water thus obtained is very bad, being impregnated either with mineral salts or lime, to say nothing of the quantity of earthy and animal matter held in suspension by its being constantly stirred up for the daily use of the Arabs and their flocks, who naturally collect in the neighbourhood of any place where water is to be had. This want of water was the greatest drawback to the satisfactory exploration of the country: want of food may be contended with, obstructive Bedawín may be quieted, and trackless mountains crossed, but the absence of water renders a country impracticable, especially to those who travel as lightly laden as we did, dispensing with the usual suite of dragoman and servants. Picturesque and desirable as a large retinue and guard of wild Arabs may appear to some persons, had we indulged in these impedimenta, I feel convinced that we should never have got through the country by any but the ordinary route. In these districts fertility is slowly but steadily being driven northwards, for various traces of cultivation and dwellings show that the rainfall must formerly have been plentiful and regular, for surely as tillage and the consequent vegetation decreases, so will the rain-supply diminish till the land has become an irreclaimable waste.

The manner in which gardens may be made and will afterwards sustain themselves, is well shown in those which still flourish at Sinai, notwithstanding the neglect of the present degraded inmates of the convent.

Even in those parts of the Tîh near El Aujeh and Wady el Abyadh which, from internal evidence, must at one time, and that within our era, have supported a large settled population, so desolate is the general aspect, that, to a casual observer, the country would seem to be and always to have been an utter waste. That they were so always is, however, at once negatived by the existence of several ruined cities surrounded by the remains of extensive gardens and vineyards; of these, the walls alone remain to tell their tale. The vineyards are clearly to be traced on the low hills and rising grounds by the regular heaps and "swathes" of black flints, with which the chief part of the district is covered, and which still retain the name of "Teleilat el 'Aneb" or grape-mounds. These facts are of great importance as showing that the objections to fixing certain localities—mentioned in Scripture as abounding in pasturage—in what is now completely desert, may be set aside as worthless. I consider too, that the southern limit of the Promised Land, at the time of the Israelitish invasion, must be placed as far south as Wady El Abyadh. This would remove many difficulties hitherto met with in the satisfactory identification of Kadesh. Though I have not space to enter fully into the question here, I may say that there is strong evidence in favour of fixing that much-disputed locality at Ain Gadis (first dis-

* A map to illustrate this paper is printed in the "Quarterly Journal o the Palestine Exploration Fund" for January.

covered by Mr. Rowlands, though he seems to confuse it with Ain El Gudeirat). Many facts support this supposition, for instance, the suitability as a strategic position for a camp of long duration. There is abundance of water there even at the present day, and springs are found at Ain Muweilah to the north and Biyar Maayin to the south. The probability is great that a large host like the Israelites, encumbered with their families and herds, would take the easy route by the open country to the west of the Azazimeh mountains in preference to the barren and rugged passes south-west of the Dead Sea.

The desert of the Tíh consists of a succession of limestone plateaux intersected by several wadies, of which the most important are W. El Arísh, which is joined near Nakhl by W. Rowag, W. Garaiyeh, with its tributaries Mayin, Jerur, Muweileh, W. El Ain, which runs into W. El Abyadh, W. Rehaibeh and W. Seba, which drain into the Mediterranean. W. Ghamr and W. Jeráfeh—the names of which have been interchanged by former travellers—fall into the northern slope of the Arabeh, and so run into the Dead Sea, as also do Wadies Murreh, Maderah, and Figreh, which debouch into the Ghor es Sáfi.

The southernmost limit is Jebel el Ráhah and Jebel el Tih on he S.W., and Jebel el 'Ejmeh on the S. and S.E., which together form a cliff running from Suez to Akabah, and projecting into the peninsula of Sinai much in the same way as that peninsula projects into the Red Sea. The height of this cliff at its most elevated point—on Jebel el 'Ejmeh—is about 4,200 feet above the sea, and from its summit the ground descends north-westwards.

To the N.E. of the Tih rises a third steppe or promontory, its northern portion corresponding to the "Negeb" or south-country of Scripture, its southern part bearing the name of Jebel Magráh, sometimes also called "the mountains of the Azázimeh," from the tribe of Arabs which inhabits it. To the S.E. of this mountainous region we came upon the only bed of sandstone which occurs throughout the whole country. It belongs to the same formation (New Red sandstone) as that at Petra and the lower strata of the Dead Sea basin.

Having carefully considered the best means of thoroughly examining the Tíh plateau, Mr. Palmer and myself determined to proceed along the base of Jebel el Tíh, and leaving to the west the Nagbs Emreikheh and er Rákineh—the passes on the ordinary routes for travellers proceeding northwards from Mount Sinai—to cross Jebel el 'Ejmeh wherever it might prove practicable, and thus proceed through a hitherto untraversed district to Nakhl, where we had established a depôt of provisions, and where we should have to make arrangements with a different tribe of Arabs for carrying our baggage northwards.

This plan was carried out, and we entered the Tíh by the Nagb el Mirád on January 12, 1870. From the summit of the cliff—for Jebel el 'Ejmeh has no pretensions to be called a mountain—a magnificent view is obtained of the Sinaitic peninsula. The range itself is composed of mountain limestone, so worn and broken by the action of frost and weather that the hills are covered with fine detritus, which, after rain, would produce some herbage, but when we were there only a few dried-up, stunted bushes were to be seen, which here as elsewhere in the desert supply good and abundant fuel.

From Jebel el 'Ejmeh the steep, bleak, waterworn hills gradually slope down and fall away into the great plains, or rather, low plateaux, which stretch across to the Mediterranean. The sameness of outline and dreariness of this country is something terrible: the few shrubs that exist are grey or brown, and seemingly withered and dead; no animal life enlivens the scene—at times perhaps a stray vulture or raven may be seen sailing far away in the blue sky, a frightened lizard will start from beneath one's feet, or a small flight of locusts be disturbed from their scanty meal on some "retem bush." Water on the road there was absolutely none; a supply for four days had to be carried from El Biyár, a well strongly impregnated with Epsom salts, and lying a few miles to the south of Nagb el Mirád.

Under these conditions we can scarcely expect to meet with many signs of life. Judging from the numerous cairns and other primeval remains, this district must at one time have been populous. Wearily did I tramp day after day, gun in hand, but I was seldom rewarded with any thing more than a stray beetle or lizard, and now and then some small desert bird, and on very rare occasions a hare or snake.

As from former experience we had found that it was impossible to work a country thoroughly when mounted, we only employed enough camels to carry our baggage. The camel-drivers acted as guides, and, to a certain extent, as attendants, for we took no servants whatever. This added to our already heavy work, yet it enabled us to get on much more satisfactorily with the various Arab tribes than we could otherwise have done.

From the Nagb el Mirád our course lay down Wady Rouág, which takes its rise in the highest part of Jebel el 'Ejmeh, about eighteen miles east of the head of Wady el Arish, with which it holds a nearly parallel course till it joins it at a short distance to the north-east of Nakhl. The district between Wady el Arísh and Wady Rowág is drained by W. Ghabiyeh, which falls into the latter about twenty-five miles from the Nagb el Mirád; after this junction the country becomes open and comparatively level. Here the ground is almost as hard as a macadamised road, and is covered with a layer of small, black, polished flints, which glisten in the sun as though they were wet. This polish must be attributed to the dust and grit kept in motion by the almost incessant winds, which are frequently very violent. Many of the monuments in Egypt bear witness to the destructive action of the grit. In this desert sand is almost unknown. There are only two or three sandy tracts, and these may be traversed in a few hours at most. The largest sandy district we had to cross was the Rumeilet Hámed, to the north of Khalasah (the ancient Elusa) where the prevailing north-west winds have formed extensive dunes. This sand, however, seems to have been entirely brought from the coast.

On arriving at Nakhl we found a small fort with wells and cisterns. In this dreary spot, encompassed by glaring white hills, a few miserable soldiers are maintained by the Egyptian Government for the protection of the Hajj caravan, the place being halfway between Suez and Akabah. Here we were obliged to dismiss the Towarah Arabs, and taking up our provisions which we had sent on from Suez, we entered into an agreement with the Teyáhah, who, after considerable discussion and futile attempts to extort a large "ghafr" or black mail, engaged to take us anywhere we wished through their country.

Of the various tribes which inhabit the Desert of the Tíh, the most numerous and powerful are the Teyáhah, of whom there are two divisions, the Sagairát and the Benaiyat, and truly they were, as their name implies, "birds of prey." They possess large herds of camels whose numbers are frequently increased by the product of the raids which they make on their hereditary foes the 'Anazeh, whose territory lies around Palmyra and to the east of the Haurán, and is about twenty days' journey from the Tíh. These forays are sometimes carried out on a large scale; on the last occasion the Teyáhah numbered 1,000 guns. At times the plunder amounts to many hundred camels, but at others the owners come down in force and the aggressors are compelled to retire. Bloodshed in these freebooting expeditions and even actual warfare is avoided as much as possible, for it results in a blood feud which is always much dreaded by a Bedawi, since it binds the relatives of anyone who has perished either by murder or manslaughter—the Arabs do not distinguish between them—to avenge his death. The blood feud or vendetta thus exercises a most salutary influence, for without it the value of human life would be totally disregarded in these wild regions which lie beyond the pale of the law.

The Terabín, the tribe next in importance, occupy the country east of the Teyáhah, their territory extending from Jebel Bisher and Bir Abu Suweirah on the Sinai road some forty miles south-east of Suez, as far as Gaza to the north.

The Haiwátt live in the mountains to the west and north-west of Akabah, and are not numerous.

The Azázimeh occupy the mountainous region which I have before mentioned as bearing their name: this tribe is not large, and they are exceedingly poor; their only food consists of the milk and cheese obtained from their camels and goats and such roots as they can dig up. On very rare occasions they may have the luck to shoot some wild animal which, whether it be ibex or hyæna, is equally acceptable to their not over squeamish stomachs. They are obliged to live in very small and scattered communities, from the fact that—with the exception of one or two brackish and unpalateable springs, their only water supply is derived from the rains collected in hollows of rocks in the ravines and wady beds, and even these are few and far between. This water was usually putrid and full of most uninviting animalculæ: however, as no other was to be had, we were obliged to drink it.

From Nakhl we went in a north-easterly direction to Wády Garaiyeh, thence to Jebel 'Araif, which we ascended; though it is little more than 2,000 ft. high; the view is very extensive. We then proceeded to cross Wady Mayin, W. Lussán, and W. Jerúr, and afterwards reached Ain Muweileh (the supposed site of Hagar's well). Here are very numerous primeval stone remains, the most remarkable being piles of stones placed in rows at the edges of the cliffs which face the East. Cannot they be the remains of the old Baal worship followed by the Amorites, whose name is still preserved in the country to the north of W. Muweileh, at Dheigat el 'Amerin (the ravine of the Amorites), Ras 'Amir, and Sheikh el 'Amiri? At various places on our route, especially at 'Uggábeh—between Nakhl and W. Garayíeh—on S. el 'Ejmell, S. 'Araif in Wady Lussán, we found very large numbers of cairns, stone circles with graves, and open spaces, which, to judge from the burnt earth within them, seem to have been designed for sacrificial purposes; also enclosures, girt by rude stone walls; and, in W. el Biyàr, circular dwellings, some of which are still standing, quite perfect. In W. Rowág nearly every hill is topped by a cairn; there are three on the summit of Jebel Araif, and we noticed that they frequently occurred as far north as Bir Seba and El Milh (Molada).

At Muweileh and near a neighbouring spring, Ain Guseimeh, are several caves. At the former place there is one cut in the face of the cliff, and entered by a staircase, ascending from a smaller cave below; this has been at one time the dwelling of a Christian hermit, as we noticed crosses rudely painted in red and traces of frescoes. At this place, too, we found, with the exception of one place in W. Lussán, the first signs of regular cultivation in former times. Stones are laid in lines across the wady-beds to check and, at the same time, distribute the drainage, and to prevent the soil being washed down by a sudden *seil* or flood.

Our next point was *El Birein*, so called from the *two wells* in the wady; here are traces of considerable ruins, a *fiskiyeh*, or reservoir, and aqueduct, the latter ruined, and the former nearly so. In the wady are some old *butmeh* or terebinth trees, remarkable as being the first trees, with the exception of two "seyáls" or acacias, that we had seen since leaving Sinai. About six miles N.W. of El Birein lie the ruins of El 'Aujeh, confounded by Dr. Robinson with 'Abdeh, which I shall presently mention, situated on a low spur running into W. Hanein. This valley, however, on account of a superstition attaching to its real name, has always been called by the Arabs, when speaking to travellers, W. Hafír. Some five or six square miles of the wady are covered with ruined walls of gardens and fields; the sides of the watercourse are built up with large stones, and dams still exist across it, though all the valley is now barren and neglected. Ten miles to the east of El 'Aujeh we discovered the ruins of a fortress called "El Meshrifeh," perched on a projecting spur, and defended on two sides by steep cliffs, which overlook a broad plain formed by the sweep of Wady el Abyadh as it debouches from Jebel Magrah; the south face of the cliff is fortified by escarpments and towers of massive masonry, and on the summit are ruins of several houses, and of a small church; on the third side a thick wall runs across the level crest of the spur. Beneath the towers and in connection with them are numerous rock-hewn chambers; also traces of a more ancient and, indeed, primeval wall, and pieces of masonry of a date far anterior to the rest of the buildings.

On the plain above mentioned and three miles and a half to the S.E. of El Meshrifeh we found the ruins of a considerable town called S'baita. This name seems to have been heard of by former travellers, who confounded the site with Rehaibeh; but I believe we were the first Europeans to visit the ruins. Here, as in many other cases, we experienced considerable difficulty, owing to the apprehensions of our Bedawin, who did their best to dissuade us from going there. I succeeded, however, in taking sketches and photographs of the chief points of interest. The town contains three churches, which, like those at El Aujeh el Meshrifeh and S'adi, must, I think, be referred to the 5th century. There are also two reservoirs, and a tower with a rudely ornamented gateway. With the exception of a fragment or two at El Aujeh, this was the only instance of sculpture we saw, and not a single inscription was anywhere to be found.

The structure of the buildings at S'baita is worth noticing: the upper stories of the houses are supported on wide, low-spanned arches two feet wide with intervals of three feet between them, and upon these is placed the flooring of the upper rooms, which consists of narrow slabs of stone. Numerous ruined towers and walled gardens and enclosures, extending to a distance of several miles from the town, attest its former importance. The vineyards, too, marked by the "Teleilát el 'Aneb," which I mentioned before, extend over large tracts in this neighbourhood.

From S'baita we went to Rehaibeh, examining *en route* the ruins of S'adi,* which do not seem to have been visited or even heard of by former travellers. At Rehaibeh the ruins are of much greater extent than at S'adi, but so confused that it is impossible to trace the plan of any single building. There are numerous wells, cisterns, and other remains of cultivation in the neighbourhood. From Rehaibeh we went to Khalasah and Bir Seba: the ruins at the former place have nearly disappeared, as the inhabitants of Gaza find it cheaper to send camels for the already squared stones than to quarry them near their town. Owing to the drought we found Bir Seba barren and deserted, though our Arabs assured us that in good seasons the grass is knee-deep, and furnishes ample pasturage for countless flocks and herds. Our unlooked-for appearance in out-of-the-way districts was usually considered by the natives to be in some manner connected with the exceptional drought, and on several occasions we were either implored to bring rain or cursed for the want of it, since the Arabs firmly believe that every *Nasráni* holds the weather under his control.

From Bir Seba we went to Jerusalem, and, after a short stay there, returned to Hebron, where we engaged three of the Jehalin Arabs, with their camels, to convey our baggage to Petra. Taking a new route, we passed Tell Arad and El Milh, and struck into the unexplored mountains of the 'Azázimeh, where we discovered the ruins of the El 'Abdeh (Eboda), which are of considerable extent, and similarly placed to those of El Meshrifeh, most of the dwellings here, as there, being half excavated and half built. Of the buildings now standing, the greater part are of Christian times. The natives are perfect savages, and detained us for two hours from visiting the ruins by collecting in a gang to the number of thirteen on the top of a pass, singing their war-song, throwing down stones, and occasionally firing off one of their old match-locks in bravado, and swearing by God and the Prophet that no one should come up. As the pass was very narrow, almost precipitous, we judged it best to propitiate them, a task accomplished, after much discussion, at the cost of eight shillings. They then escorted us to the ruins, where we took such measurements and photographs as we required. From 'Abdeh we went through the 'Azázimeh mountain, a region so awfully desolate as to defy description, struck the 'Arabah at the junction of W. Jerafeh with W. Ghamz, and crossed thence to Petra. Here the Liyathineh fully maintained their character for brutality and insolence. Infidels in all but the name of Moslims, they are descended from the tribe of Khaiberi Jews, who bear such a bad character in Arabia. To add to our discomfort, we were snowed up for two days in a tent only just large enough for us both to lie down in. During a stay of six days, however, Petra was thoroughly examined by us and accurately mapped. We then bent our steps northwards, and at El Barid, about seven miles from Petra, discovered a colony of dwellings and temples cut in the rock, and some rudely chipped Nabathæan inscriptions. The walls and ceilings of the rock-chambers were decorated with frescoes, some coarse others well executed. We next travelled down the 'Arabah to the Dead Sea, and having examined the Lisan, went up into Moab. Here we stopped about three weeks and wandered over the country in search of inscriptions, as Mr. Palmer had specially come to ascertain if another Moabite stone was in existence. At last, however, we both came to the conclusion that *above ground* there are none. From Moab we crossed the Jordan, near Jericho, and returned to Jerusalem.

(*To be continued.*)

* S'adi is two-and-a-half miles E.S.E. of Rehaibeh.

## SCIENTIFIC SERIALS

THE fifth part of the nineteenth volume of the *Palæontographica* recently published, is devoted to the description by Prof. Schenk, of fossil plants from the north German Wealden formation. The plants here described and figured upon 8 plates are all cryptogamous, and with the exception of a single *Chara*, and four *Equiseta* belong to the group of ferns, of which 21 species are noticed; but it must be remarked that Prof. Schenk has considerably lessened the apparent number of species by reducing a great many of the names given by former authors to the rank of synonyms. At the same time he describes and figures seven forms as new species, one as the type of a new genus, *Marsilidium*, belonging to the Rhizocarpeæ, and he also establishes the new genus *Matonidium* for *Laccopteris Göpperti*, Schimper. The other new species belong to the genera *Sphenopteris*, *Alethopteris*, *Laccopteris*, *Olean-*

*dridium*, *Dictyophyllum* and *Protopteris*, the last being doubtfully represented by a portion of a tree-like stem.

THE second part of Tome xx. of the *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève* (1870) contains an exceedingly important zoological paper, namely, a supplement to Prof. Claparède's descriptive account of the Chætopod Annelides of the Gulf of Naples. This not only includes descriptions of many new forms discovered by M. Claparède during the winter of 1868-69, but furnishes him with an opportunity of effecting a combination between his own observations and those of Prof. Ehlers, whose valuable work on the Chætophorous Annelides appeared almost simultaneously with Prof. Claparède's former publication. The memoir is illustrated with fourteen beautiful plates. This part also contains descriptions by Dr. J. E. Duby of some minor little-known exotic mosses, accompanied by four plates.

THE first and second numbers of the *Bollettino del R. Comitato Geologico d' Italia*, published together for the months of January and February of the present year, contain some interesting papers, among which, perhaps, the most important is that on the temperature of the rocks in the Mont Cenis tunnel, communicated by the engineer, M. F. Giordano. The highest temperature observed was 29·50° C. (=85·10° F.) at a distance of 6,450 metres (about 21,000 feet) from the southern opening, at the same time that the temperature of the rock at 400 metres (about 1,300 feet) from the opening was only 11° C. (= 38·2° F.). M. Giordano also publishes notes on the geological constitution of the Roman Campagna, illustrated with three long sections. These numbers also contain a translation into Italian of G. von Rath's memoir on the environs of the lake of Bolsena, an extract from a paper by Prof. T. Taramelli on the Eocene formation of Feiuli, and some short bibliographical notices.

THE editor of the *Geological Magazine*, in his April number (No. 82), has resumed his series of notices of eminent living geologists with a sketch of the scientific life of Mr. Thomas Davidson, illustrated with a good portrait. That Mr. Davidson's labours on the Brachiopoda fully entitle him to such an honour no one will be inclined to deny, but one is somewhat startled at learning what is the real result of his activity, chiefly in this field of research, and being told that his published writings occupy about 2,220 pages, and are illustrated with 244 plates, all or nearly of them drawn by his own hand! Mr. H. B. Woodward describes a curious example of the inversion of strata belonging to the carboniferous series at Vobster, in Somersetshire, to the north of the Mendip Hills, where coal is worked beneath mountain limestone. This phenomenon has been ascribed to a folding over of the main ridge of the Mendips, but the author adduces what seem to be good reasons in opposition to this view, and endeavours to account for it by local disturbance associated with faults. He illustrates his views by means of a diagram section. —Mr. G. H. Kinahan communicates a paper on Æolian drift or blowing sands in Ireland, in which he explains these peculiar deposits as being the products of the action of glaciers during the glacial period.—M. De Rance describes the pre-glacial geography of northern Cheshire. The number also contains a reprint of Mr. David Forbes' lecture on the nature of the earth's interior, and the usual reviews and short communications.

THE *Transactions of the Linnean Society*, vol. xxvii. part 3, has just been issued, containing three papers, each illustrated with 4to. plates:—Observations on the Lichens collected by Dr. Robert Brown in West Greenland in 1867, by Dr. W. Lauder Lindsay; On the Vertebrate Skeleton, by Mr. St. George Mivart; and Descriptions of some British Spiders new to science, by the Rev. O. P. Cambridge. Mr. Mivart's article is devoted to a discussion of the following questions:—1. What is the best way to seek *a priori* a general view of the axial skeleton? 2. What is the essential nature of ribs, transverse processes, and sternum? 3. What is the essential nature of branchial arches, and in what relation do they stand to the ribs? 4. What is the essential nature, as compared with branchial arches, of the hyoidean arch, mandible, and more anterior structures? 5. What relations exist between the "chevron" bones and other parts of the vertebrate skeleton? The appendicular skeleton, as distinct from the axial skeleton, consisting of the anterior and posterior limbs, is also discussed.

THE *Proceedings of the Natural History Society of Dublin* for the sessions 1867-68, 1868-69, vol. v. parts iii. and iv., was published on May 3, 1871. Among the more important papers we notice:—Prof. W. King "On some Palliobranchiate Shells from the Irish Atlantic;" Prof. Macalister "On the Myology of the Otter," "On the pyloric appendages of the common Trout," "On the Flora of Kinross-shire," and "On the arrangement of Pronator Muscles in the limbs of Vertebrate Animals.—Dr. D. Moore "On the Botanical Congress of Paris of 1867," and "On Addenda to British and Irish Muscology." Dr. A. W Foot "On some points observed in the dissection of an Aylesbury Duck." Rev. E. O'Meara "On some new Arran Diatomaceæ" (Plate 13). W. Archer "On a peculiar cyst-like structure enclosing examples of *Staurastrum cuspidatum*," &c., and "On some Freshwater Rhizopoda" (Plates 8, 9, 10). Prof. E. P. Wright "On *Tubipora musica*" (Plate 11). Notes of a tour in the spring and summer of 1868 to Sicily and Portugal (Plate 12). These Parts conclude vol. v., and have title page, index, and appendices.

## SOCIETIES AND ACADEMIES

### LONDON

**Royal Institution of Great Britain**, May 8.—Sir Henry Holland, Bart., M.D., president, in the chair.—The following Vice-presidents were nominated for the ensuing year:—Duke of Northumberland, Lord Lindsay, W. Spottiswoode, the Treasurer, Sir Frederick Pollock. William S. Burton, Arthur Samuel Hobson, Richard Liebreich, Abraham De Mattos Mocatta, and Edward Stanhope Pearson, were elected members. John Tyndall, F.R.S., was re-elected Professor of Natural Philosophy.

**Zoological Society**, April 29 (Anniversary Meeting).—Viscount Walden, president, in the chair. After some preliminary business, the report of the Council was read by Mr. P. L. Sclater, F.R.S., the secretary. It stated that the number of Fellows of the Society on the 1st of January last was 3,021, showing a net addition of fifty-five ordinary members to the roll during the year 1870. Twelve new corresponding members had likewise been elected during the year 1870. The total income of the society during the year 1870 was stated to have been 23,257*l*., being 488*l*. more than that of the preceding year. The total ordinary expenditure had been 21,364*l*., in which sum had been included every item necessary to keep the society's establishment in its present state of efficiency. Besides this the sum of 3,043*l*. had been devoted to extraordinary expenditure, in the shape of new buildings and works in the gardens. Of these works the most important was the new elephant-house, on completing which the sum of 2,324*l*. had been expended. This, when added to the sums spent upon the same building in former years, had raised the total cost of that building to 6,356*l*., in which, however, the yards, ponds, fences, terrace walk in front, and the necessary arrangement of the adjoining grounds were included. Other works carried on in the society's gardens during the past year had been the completion of the new first-class refreshment-room, and the extension of the system of heating the buildings by hot-water apparatus. The total number of visitors to the society's gardens during the year 1870 had been 573,004, showing an increase of 156 over the corresponding number in 1869. The greatest daily number of admissions in 1870 (28,457) was on Whit Monday, the 5th of June; the least number (28) on the 3rd of March; the average daily number of admissions throughout the year had been 1,570. The number of animals contained in the society's menagerie on the 31st of December, 1870, was stated to have been 2,118, showing an increase of 105 when compared with the corresponding number at the same date in the previous year. Among the additions made to the collection during the year 1870 had been a considerable number of special interest, either on account of their scientific novelty or from not having been previously brought to England in a living state. Full particulars concerning these were given, as also a list of the species that had bred in the society's gardens during the year. The report then proceeded to give a long list of donors and their several donations to the menagerie, after which, in conclusion, the council contrasted the present state of the society's affairs with that which had existed ten years ago. In 1860, they observed, the total number of Fellows was 1,716; it was now 3,021; in 1860 the number of visitors to the society's gardens had been 394,906; in 1870 it had been 573,004. The total income of the society in 1860 was 16,864*l*.; in 1870 it had amounted to 23,257*l*. In 1860 the reserve fund was 3,000*l*. Reduced Three per Cents.; it had now been augmented to 7,000*l*. of the same stock. Moreover, during the past ten years, sums amounting altogether to upwards of 46,000*l.* had been devoted to the permanent improvement of the society's garden establishment, the expenditure of which had enabled the council to renew nearly the whole of the more important buildings on an improved and enlarged scale. These facts, it was believed, could not be otherwise than gratifying to the Fellows of the society. The society then proceeded to ballot for the council and officers for the ensuing year, when Lord Calthorpe, Mr. Francis Galton, F.R.S., Captain the Count Gleichen, R.N., Mr. John Gould, F.R.S., and Dr. Hamilton were elected into the council, in the place of Professor Huxley, F.R.S., Mr. J. Travers Smith, Lord Walsingham, Mr. G. R. Waterhouse, and the Bishop of Winchester, who retired therefrom, and Viscount Walden was re-elected president; Mr. Robert Drummond, treasurer; and Mr. P. L. Sclater, F.R.S., secretary.

Zoological Society, May 2.—Viscount Walden, President, in the chair. A letter was read from W. H. Hudson, addressed to the secretary, containing observations on the habits of the various swallows met with in and around Buenos Ayres.—Mr. P. L. Sclater exhibited and made remarks on the shell of a river-tortoise of the genus *Pelomedusa*, obtained by Mr. Chapman on the Upper Zambesi.—Prof. Flower exhibited and made remarks on the mounted skeleton of the young hippoptoamus, recently born in the Society's Gardens.—The Viscount Walden read a paper on the Birds of the island of Celebes, in which the materials hitherto available for the elaboration of its avi-fauna were brought together and discussed. Out of the generic forms met with in Celebes, thirty-eight appeared to be Indian, and twenty-three Australian in character. To these were added a strong element of individuality, shown by the presence of sixty-five species, and nine genera unknown elsewhere. The avi-fauna of Celebes, so far as was certainly known, was composed of 193 species; but the author observed that a considerable portion of the centre of the island remained unexplored, which gave a prospect of future discoveries.—A communication was read from Mr. W. Harper Pease, of Honolulu, Sandwich Islands, containing a catalogue of all the known land-shells inhabiting Polynesia, together with remarks on their synonomy, distribution, and variation, and descriptions of some new genera and species.—A communication was read from Dr. John Anderson, Curator of the Indian Museum, Calcutta, containing the description of a new generic form of newt from Western Yunan, proposed to be called *Tulototriton verrucosus*.—A second communication from Dr. J. Anderson contained some drawings of and notes on the original specimens of *Testudo phayrei*, Blyth, in the Indian Museum. Having examined the skull in the British Museum upon which *Scaphia falconeri*, Gray, had been based, and re-examined the smaller example of *Testudo phayrei* at Calcutta, Dr. Anderson had come to the conclusion that Mr. Theobald's account of its history was strictly accurate.—A communication was read from Dr. J. E. Gray, F.R.S., entitled Notes on the species of *Bradypidæ* in the British Museum.

Geological Society, April 26.—Prof. Morris, vice-president, in the chair.—Mr. Robert Russell, of the Geological Survey of England and Wales, was elected a Fellow of the Society. The following communications were read:—1. "On a new species of Coral from the Red Crag of Waldringfield," by Prof. P. Martin Duncan, F.R.S. Prof. Duncan described, under the name of *Solenastræa Prestwichi*, a small compound coral obtained by Mr. A. Bell from Waldringfield, and stated that it was particularly interesting as belonging to a reef-forming type of corals which has persisted at least from the Eocene period to the present day. The single specimen consisted of several small crowded corallites, having calices from $\frac{1}{20}$ to $\frac{3}{20}$ inch in diameter, united by a cellular epithecal cœnenchyma. It was much rolled and worn before its deposition in the Red Crag, and hence the author regarded it as a derivative fossil in that formation, and he stated that it probably belonged to the rich reef-building coral-fauna which succeeded that of the Nummulitic period. Mr. Etheridge remarked that the origin of this interesting fossil seemed uncertain. It appeared, however, to be derived from some other source, and not to have originally belonged to the Red Crag. In England the genus was hitherto unknown in beds newer than those of Brockenhurst. The presence of this single specimen showed how much we had still to learn with regard to the crag formation. It was to be hoped that the coral might eventually be found attached to some organism from which its age might be determined. Prof. T. Rupert Jones remarked that he would be glad to hear of more corals being discovered in the so-called Coralline Crag. He inquired whether cœnenchymatous corals were necessarily reef-corals, observing that this coral was referred to the Miocene on account of its presumed reef-forming character. He added that some of the Foraminifera of the White Crag had the aspect of existing Western Mediterranean forms, and thus supported some of Prof. Duncan's remarks. Mr. Gwyn Jeffreys observed that the distinction between the Coralline and Red Crag seemed to be every day diminishing. The appearance of the fossil seemed to betoken its derivative character. Like other speakers, he complimented Mr. Alfred Bell on his great intelligence in the collection of crag fossils. Prof. Duncan, in reply, maintained that the differences between deep-sea and reef-building corals were well established, and around modern reefs in the deeper sea the forms were quite distinct, and the deep-sea corals never presented the cœnenchyma distinctive of the reef-building form. This, he suggested, might be connected with the difference in the amount of sea-water with which it was brought in contact, which in the surf was much greater than in the almost motionless depths of the sea.—2. "Notes on the Minerals of Strontian, Argyleshire," by Robert H. Scott, M.A., F.R.S. The paper stated that the existing lists of minerals to be found at Strontian were incorrect. The discovery of apophyllite, talc, and zircon seemed to be hardly sufficiently confirmed. On the other hand, Mr. Scott named several species which he had himself observed *in situ*, and which are not noticed in any of the books, viz., two felspars, orthoclase, and an anorthic felspar in the granite; two varieties of pyroxenic minerals in the granite and syenites, neither of which have as yet been analysed; natrolite in the trap-dykes; muscovite or margarodite in very large plates, lepidomelane and schorl. Specimens of these minerals and of the others found at the mines were exhibited; but it was stated that, owing to the fact that the old workings at the mines in Glen Strontian had been allowed to fall in, it was now no longer possible to ascertain much about the association of the species. The one is galena, containing very little silver. The gangue is remarkable for the absence of fluor and the comparative rarity of blende and heavy spar. Harmotome is found principally at a mine called Bell's Grove, both in the opaque variety and in the clear one called morvenite. Brewsterite occurs at the mine called Middle Shap, and at the mine Whitesmith strontianite is found with brewsterite, but without harmotome. Calcite is alse very common. Within the last few years a new mine has been opened called Corrantee, which is in the gneiss, whereas the other mines lie on the junction of the granite and gneiss. At this mine several fine specimens of calcite have occurred, many of them coated with twin crystals of harmotome, similar to those from Andreasberg, whereas the crystals found at the old mine are not so clearly macled. Associated with these were found a number of small hexagonal prisms, perfectly clear, and exhibiting a very obtuse dihedral termination. They gave the blowpipe reaction of harmotome; and on analysis by Dr. J. E. Reynolds, proved to be that mineral. Descloiseaux has already described a quadrifacial termination to harmotome, with an angle of 178° 20′. Mr. Scott submitted that possibly the crystals which he exhibited might bear faces which had a close relation to those described by Descloiseaux. He concluded by stating that Strontian promised as rich a harvest to the mineralogist as any locality is these islands. Mr. W. W. Smyth mentioned the wonderful collection of minerals from Strontian which had been brought to the Great Exhibition of 1861, which gave a most striking idea of the mineral riches of the locality. The occurrence of such a series of different substances in one locality in the granite was almost unparalleled, though in the Andreasberg mines, in clay state, they were to some extent rivalled. The features, however, differed in the two places, more silver and a greater number of zeolites being present in the Hartz mines. Mr. D. Forbes observed that harmotome occurred also at the Kongsberg silver-mines in Norway, at a distance from granite. He thought it remarkable that these crystals of peculiar form occurred in the same spot and in connection with crystals of the same substance, but of the ordinary form. Mr. Davis remarked that celestine was also to be placed on the list of the minerals from Strontian. Harmotome had been found in the same form of double crystals at Bodenwies in Bavaria. Mr. Scott stated, in reply to a question from the chairman, that the mineral had not been as yet optically examined, but that if he could procure more of it he should be happy to place it at the disposal of any gentleman who would examine it As regarded the idea that harmotome usually occurred near the surface, he could give no information about the old mines, as they had been allowed to fall in; but most certainly the new specimens from Corrantee came from surface-workings. He was very glad to learn from Mr. Davis that celestine had been found at the locality; and he felt sure that careful search would double or treble the number of species known to occur there. With reference to what had fallen from Prof. Smyth, he could fully corroborate his observations as to the difference between the forms of calcite associated with harmotome at Andreasberg, in the Hartz, and at Strontian. It was remarkable that the general facies of the crystals of calcite occurring at Corrantee, where the lode was entirely in the gneiss, differed from that usually observed in the old mines in Glen Strontian, which were partly in the granite and partly in the gneiss. 3. "On the probable origin of Deposits of 'Loess' in North China and Eastern Asia." By Mr. T. W. Kingsmill, of Shanghai. Communicated by Prof. Huxley, F.R.S., V.P.G.S. The author stated that the Baron von Richthofen had lately applied the term "Loess" to a light clay deposit covering immense tracts in the north of China. The author regarded this formation as in great measure corresponding to the Kunkur of India, and thought that it probably extended far into the elevated plains of Central Asia. Richthofen considered that this deposit had been produced by subaërial action upon a surface of dry land; the author argued that it is of marine origin, having been deposited when the region which it covers was depressed at least 6,000 feet, a depression the occurrence of which since the commencement of the Tertiary period he considered to be proved by the mode of deposition of the Upper Nanking sandstones and conglomerates, the bold escarpments of the hills on either side of the Yangtsze, and other peculiarities of the country. Prof. Ramsay remarked that the author had not proved that the loess he described was really stratified. He could not agree with his views of the inland escarpments he mentioned having been old coast lines. It was only accidentally that sea cliffs had any connection with the line of strike of the strata, whereas inland cliffs always followed the strike. He thought the phenomena were rather in accordance with a long exposure of the land to subaërial influences than with the loess, having been of marine origin. Even in England, in those parts which had long been free from marine action, beds of brick earth had been formed. He also instanced the plains of Picardy as exhibiting a vast extent of such subaërial beds. Prof. T. Rupert Jones said that though the area treated of by Mr. Kingsmill was too large to have its geology explained merely by reference to rain-wash and valley deposits, whatever his low-level loess might be, the higher accumulations of loamy deposits, stated to be 1,000 feet thick at an elevation of 3,000 feet, and regarded by Mr. Kingsmill as the quiet water sediments of a great gulf with the miocene conglomerates and sandstones of Nanking and elsewhere for its marginal equivalents, appeared to require different explanation. All loess need not be of sea origin; in oscillations of land marine deposits must be carried up to great heights: and, referring to Mr. H. M. Jenkins's determination of the marine origin of the loess of Belgium, Prof. Jones thought it highly probable that some at least of that in China may have been similarly formed. Mr. Hughes said that the author appeared to have grouped together all the superficial deposits of a vast area without explaining very clearly the grounds upon which he identified those deposits at distant points. He did not prove that what he called the shore deposit was marine, or that it was of the same age as the loam which he described, and which Mr. Hughes thought, from the description, was far more likely to be subaërial. Mr. Evans and Mr. Etheridge suggested the probability of much of the so-called loess might be derived from higher loamy beds, possibly derived from the decomposition of limestone rocks containing sand and clay, and redeposited by the action of rain. The following specimens were exhibited:—Minerals from Strontian; exhibited by Mr. Scott, in illustration of his paper. Corals; exhibited by Prof. Duncan, in illustration of his paper.

Royal Geographical Society, April 25.—Major-General Sir Henry C. Rawlinson, K.C.B., vice-president, in the chair. The following new Fellows were elected:—Mr. G. E. Bell; Staff-Commander Charles Burney, R.N.; Messrs. Walter J. Ellis; J. C. W. P. Graham; Simon Little; Henry Syme. A letter was read from Mr. R. B. Shaw to Sir Roderick Murchison, on that portion of his recent journey to Yarkand (with Mr. Forsyth) in which, detached from the rest of the party, he explored the rugged country between the western extremity of the Thibetan Plateau and the Valley of the Upper Shayok. He described the plateau (17,000 feet high) as ending abruptly on the west in a great limestone range, which, like the masonry *revêtement* of an embankment, has protected the level table-land from the wearing influence of the rains from clouds sweeping up the Shayok Valley. Standing on the edge of the plateau, the whole country westward appeared as an irregular mass of snowy peaks and narrow precipitous valleys. In attempting to descend one of the valleys towards the Karakoram road, the party suffered fearfully in struggling for three days through the broken ice of a torrent at the bottom of a stupendous chasm, from which, in some places, the light of day was nearly excluded.—A second communication was read "On the Journey of the Mirza across the Pamir Steppe to Yarkand and Kashgar," by Major Montgomerie. This was a detailed report of the journey of an Afghan gentleman, instructed by the officers of the Trigonometrical Survey to traverse the Mahomedan countries across the Hindoo Koosh and Pamir Steppe, eastward to the plains of Eastern Turkestan. The journey was successful in its main object; and we have now, for the first time, a scientific account of those little-known regions, with the means of fixing the geographical position of all the important places. The Mirza proceeded from Fyzabad eastward, along one of the head-waters of the Oxus, arising in Lake Pamir-Kul (13,300 feet), and thence to Tash Kurgan, Yanghissar and Kashgar. Crossing the elevated region of the Pamir, he suffered fearfully from the cold, although well clad, even to the lining of his boots, in warm woollen clothing. Sir Henry Rawlinson explained to the meeting that the Mirza's route was the same as that followed by Marco Polo and Benedict Goez, and in later times by Mahomed Amin. He also stated that the vexed problem of the longitude of Yarkand (placed by the Schlagintweits about 200 miles too far to the west) had been solved by the recent lunar observations of Mr. Shaw, the computation of which had been completed that day, at the Geographical Society, by Mr. W. Ellis of the Greenwich Observatory. These observations placed Yarkand in E. long. 77° 14′ 45″. Colonel Walker, of the Great Trigonometrical Survey of India, and Sir A. Scott Waugh also addressed the meeting, chiefly on the subject of the employment of native observers in the geographical exploration of the regions beyond the British boundaties.

Chemical Society, May 4.—Dr. Warren De La Rue, F.R.S., vice-president, in the chair. The following gentlemen were elected Fellows:—Messrs. R. S. Best, C. S. Cross, W. H. Darling, G. H. Ogston, I. Schweitzer, and. W. A. Smith. Dr. Völcker delivered a lecture "On the Productive Powers of Soils in Relation to the Loss of Plant Food by Drainage." The lecturer began by showing the futility of the belief that a soil-analysis could reveal whether a land was productive or not. To those who only imperfectly know the teachings of modern agricultural science, it appears veiy simple to remedy a deficient soil by finding out through analysis the wanting constituents, and then to supply them. But this is not so. Not only is it difficult exactly to analyse a soil, but many other conditions besides the composition of a land have to be observed. The state of combination in which the mineral constituents of a land are found, the physical condition of the soil, the presence or absence of some matter injurious to the growth of plants, all these are so many important points upon which soil-analysis throws no light whatever. The lecturer equally opposes the views of those who advocate that in a system of rational farming there should be kept up a debtor and creditor account as regards the constituents which are removed from the soil in the crops grown upon it, and the quantity of fertilising matter restored to it in the shape of manure. The fertility of the soil cannot be maintained, much less increased, if only as much fertilising constituents would be applied to the land as one removes from it in the crops. Dr. Völcker then discussed the relative values of various mineral salts as manures, quoting in support of his views the results of the classical field experiments of Lawes and Gilbert; and this then led the lecturer to speak of the examination of land-drainage waters. Lawes and Gilbert, throughout a long series of experiments on the growth of wheat, have experienced a great loss of nitrogen; the amount of nitrogen supplied in the manures was greater than that recovered in the increased produce. It appeared to Dr. Völcker that the nitrogen lost might have passed into the drains. Careful collection of such drainage waters, and their analysis, proved Dr. Völcker's supposition to be correct. It became clear that, in whatever form the nitrogen is applied to the soil, a large proportion of it is carried off chiefly in the form of nitrates. At all times of the year, but especially during the active period of growth of the crops, nitrates are found in the watery liquid which circulates in the land, whereas ammonia salts are never met with in any appreciably large quantities. It may therefore be assumed tha it is chiefly, if not solely, from the nitrates that the crops build up their nitrogenous organic constituents. Dr. Völcker's analyses of drainage waters further showed that potash and phosphoric acid, which certainly are the most important mineral constituents for the plant, are almost entirely retained in the soil, whilst the less important, as lime or magnesia, or sulphuric acid, pass with greater readiness out of the land.

Entomological Society, May 1.—Prof. Westwood, M.A., F.L.S., in the chair. Mr. Higgins exhibited fine collections of exotic Lepidoptera, Coleoptera, &c., from Natal and Borneo, and a number of photographic coloured figures of larvæ from Natal.—Mr. Meek exhibited *Nyssia lapponaria*, Duponchel, captured in Perthshire by Mr. Warrington, and new to Britain.—Mr. Champion exhibited *Scydmænus rufus*, captured by him in Richmond Park, a beetle new to Britain.—The Rev. R. P. Murray exhibited a collection of Swiss insects, including a singular variety of *Lycæna Eurydice*.—Mr. Bicknell exhibited an extraordinary specimen of *Conepteryx rhamni*, captured by Mr. Cowan at Beckenham in March 1870; this individual had the central margin of both fore wings, and of the right hind wing, broadly suffused with deep crimson; it was considered that the colour was accidental, and probably owing to the wings having come in contact with some chemical substance. Mr. Bicknell exhibited varieties of other British Lepidoptera.—Mr. Stainton exhibited drawings of Micro-Lepidoptera from New Grenada collected by Baron von Noloken.—Mr. M'Lachlan exhibited the tusk of a female Indian elephant lent to him by Dr. Sclater. The root of this tusk was much eroded and blackened, and on the diseased part were long rows of eggs, apparently those of some insect. The elephant had been shot in Malabar by Mr. G. S. Roden, of the 1st Royal, and both its tusks were in the same condition. Furthermore, it appeared from the notes of a writer in the *Field* that this circumstance was not uncommon, but always occurred in the female elephant. None of the members could give any information respecting the parasite, but it was generally considered that the parasite had not caused the decay, but rather that it had taken advantage of a previously morbid condition.—Mr. Lewis exhibited an earthern jar, like an ordinary tobacco jar, of Chinese manufacture. It had an enormously thick porous bottom, and it was stated that the inhabitants of Pekin use these jars for the purpose of confining large beetles, which they keep for fighting. The beetles are allowed no food but water, and become extremely ferocious. Prof. Westwood reminded the meeting that the Chinese were already known to keep mantides for fighting purposes.—M. Müller read notes on a gall on the common brake (*Pteris aquilina*) found by Mr. Rothway, and he remarked that Schenck had noticed the same gall in Germany, and referred it to *Diastrophus rubi*.—Prof. Westwood read descriptions of new species of *Lucanidæ*.—Mr. Bates read a description of a remarkable longicorn beetle from Matabili land, in the interior of South Africa, sent to him by Mr. T. Baines. This insect he proposed to call *Bolbotritus Bainesii*. It was especially remarkable for the enormously swollen third joint of the antennæ, the other joints being much shortened. Mr. Bates also read a description of a new species of *Mallaspis* from Chiriqui, which he named *M. præcellens*.—Mr. Kirby communicated synonymic notes on European *Lepidoptera*.—Attention was called to paragraphs going the round of the London daily papers respecting a so-called storm of insects said to have occurred on two occasions recently at Bath. These records were characrised by the usual newspaper inaccuracy and vagueness on scientific matters. Prof. Westwood thought they probably referred to *Branchypus stagnalis*, a large fresh-water entromostracon.

Linnean Society, May 4.—Mr. G. Bentham, president, in the chair. Dr. Oswald Heer, of Zurich, was elected a foreign member in the place of the late Prof. Unger. The following papers were read:—"The phenomena of Protective Mimicry, and its bearing on the theory of Natural Selection, as illustrated by the Lepidoptera of the British Islands," by Raphael Meldola, F.C.S. "On the Ascalaphidæ," by R. McLachlan.

Society of Biblical Archæology, May 2.—Dr. S. Birch, F.S.A., president, in the chair. The following new members were balloted for, and duly elected:—Louis Blacker, Rev. D. S. Heath, M.A., F.R.S.L., and Mrs. L. Blacker. The President read a paper "On a Hieroglyphic Tablet of Alexander II. (Ægus) son of Alexander the Great, recently discovered at Cairo." This tablet was dedicated to the goddess Buto, and is dated in the seventh year of Alexander (311 B.C.). It records the restoration to the priests of Buto of the district formerly given to them by Khabash, an Egyptian monarch, contemporaneous with the later years of Darius and Xerxes, which last monarch is mentioned in disparaging terms, probably to flatter Ptolemy, the Macedonian ruler of Egypt, who is styled on it, "The Satrap of Alexander." Dr. Birch also contributed a second paper, based upon communications received from Lieut. Prideaux, containing the interpretation by himself and the Baron de Moltzan, of three bronze tablets, with inscriptions in the Himyaritic character, recording adorations by Hanbaz, an Himyaritic monarch, to the deities Ath-tor and Wud on the conquest of the town of Kuderamelek.—A third paper was further read by Prof. Goldschmidt (of Copenhagen) on the derivation of the name Αἴγυπτος, from Ukh-hap-t, *i.e.*, "the land of the good stream-sending spirit." Some discussion followed the reading of these papers, Messrs. W. R. A. Boyle, S. M. Drach, Rev. T. Gorman, Rev. I. Mills, Sir Charles Nicholson, &c., took part.

CAMBRIDGE

Philosophical Society, May 1.—Mr. G. Hale, M.A., and Mr. C. Smith, B.A., Sidney College, and Mr. A. G. Greenhill, B.A., St. John's College, were elected Fellows. The following paper was read:—"On the Measurement of an arc of the Meridian in Lapland," by Mr. I. Todhunter, F.R.S. The object of this memoir was to draw attention to the numerous errors which have been made, even by distinguished astronomers, in their accounts of the two measurements of an arc of the meridian in Lapland. A comparison of the original authorities on the subject at once detects these errors, and supplies the necessary corrections.

EDINBURGH

Royal Physical Society, April 26.—Mr. C. W. Peach president, in the chair. After the appointment of committees for carrying on special investigations during the summer, Dr. M'Bain communicated a paper by Dr. John Kennedy Elis, "Remarks on a Japanese Skull."—Dr. Robert Brown read "Notes on the Breeding Places and Food of some Scottish Sea Birds," by Captain M'Donald, Fishery Cruiser *Vigilant*.—Mr. Peach exhibited a fine mass of gulf weed covered with small cirripedes, which he received on Monday last from Captain N. Leslie, of the ship *Lady Milton*, now lying at West Hartlepool, picked up on the homeward voyage; and then read the following extract from the captain's letter:—"I picked up a lot of gulf weed in 32 N. and 7 70 W., on the 9th of March. I send a sample; it looked very beautiful when fresh, so many little barnacles, and all full of life when in a bucket of water. I am now sorry that none of it was bottled, if only for curiosity; it might lead you to something of a knowledge of seasons, as I never saw so many barnacles on a voyage as I have this time, either on seaweed or wreck, and, strange to say, there are none on the ship's bottom. Last year we saw none on the seaweeds, &c., when the quarters of the ship were nearly covered with them, and this although we had not so much fine weather as this."—Mr. Peach stated that the cirripede most abundant in the parcel thus sent was covered with hars and spines, much like *Oxynaselis celata* of Darwin's monograph, but it differed in so many respects that it might prove to be a new species.—Mr. Andrew Taylor read "Notes on the Geology of Inchkeith."

NEW ZEALAND

Wellington Philosophical Society, January 28.—Hon. W. B. D. Mantell, F.G.S., president, in the chair. From the report of the Council it appears that out of fifty-nine communications made to the Society during the past year forty-four will appear in the forthcoming volume of the Transactions. The number of members has increased from 85 to 103, and the accounts show a balance in hand of 60*l*. 10*s*. 7*d*. The chief item of expenditure has been a grant of 50*l*. in aid of the Botanic Gardens, for the purpose of having the collection of native plants completed by the addition of those found in other parts of the colony, and also in providing labels for the principal trees and shrubs along the paths, giving the scientific and native names. The office bearers chosen for the ensuing year are W, T. L. Travers, F.L.S., President; J. C. Crawford, F.G.S., and W. L. Buller, F.L.S., Vice-Presidents, with J. Hector, M.D., F.R.S., and Messrs, J. Kebbell, W. Lyon, F.G.S., R. Hart, and W. Skey, as members of the Council. F. M. Ollivier, Esq., Hon. Secretary and Treasurer. Messrs. J. Prendergast, G. Allan, W. Colenso, F.Z.S., and Dr. Knox, were elected new members.—Dr. Hector called attention to a live katipo or poisonous native spider, with nest and young ones, on the table, and read a short notice by Mr. Duigan, of Wanganui, of an extraordinary flight of beetles that passed over that district in December last.—A paper was then read by Mr. Travers from Mr. Shand, of the Chatham Islands, describing the different kinds of Mokihi or flax stalk canoes that the natives used in former times, a model of one of which is in the Museum.—Dr. Hector gave an interesting account of the reports he had received from more than thirty stations respecting the magnificent meteor that passed over New Zealand on the 1st instant, at 8.30 P.M., which, he stated, had a general course from about a point west of north through the zenith of Picton, over which place it passed at less than thirty miles altitude above the surface of the earth, travelling with an apparent velocity of 12 miles per second. Its form was that of a ball intensely luminous, of a reddish hue, with a long very brilliant tapering tail, the light of which resembled burning magnesium wire, but giving off red sparks. It completely eclipsed the light of the moon which was shining brightly. The area over which it had been seen has a length of 700 miles, and width of 300, from lat. 36° S., long. 122° E., to lat. 46° S., long. 175° E. The apparent diameter of the head was 10′, and the length of the tail tapering about 1° Some of the observations appear to indicate that its course must have descended towards the earth's surface, but this depends on mere estimates of angular altitude, which cannot be depended on. The prolonged detonation which followed the passage of the meteor does not appear to have been heard at all the stations, but chiefly at those in the vicinity of Cook Strait, where the path of the meteor intersected New Zealand, all the observers in the North Island having seen it to the west, and those in the South Island to the east. When nearest to Wellington it must have been at a distance in a direct line of fifty-five miles, which agrees with the time, five minutes, which elapsed before the report was heard. This shows that the report did not proceed from the final bursting of the meteor, but proceeded from it at the time it was nearest to the observer. Indeed, from the length of the path in which the meteor was seen, its sudden disappearance, as if by bursting, must have been an optical illusion in the case of all the northerly observers. Mr. Marchant stated that he had witnessed another meteor, almost equal in brilliancy to the above, on the previous evening (27th inst.), passing from east to west. Mr. Floyd of the Telegraph Department, stated that this meteor was reported at several stations in the North Island, and appeared to have passed over Napier on the east, to Patea on the west coast. Its colour was blue.—After some further discussion two important papers on the electromotive and conductive power of mineral sulphides, were read by Mr. Skey, in which he claims to have made some discoveries.

### Paris

Academie des Sciences, April 21.—Eighteen members present. The sitting was not devoid of interest, although the communications were far from numerous. M. Egger, professor of Greek at the Sorbonne and member of the Academie des Belles Lettres, availed himself of the privilege granted to the members of different academies. He read a very long dissertation on a papyrus found in 1866, which gave a great deal of information on the state of ancient Egyptian civilisation. It related chiefly to the prices of different articles used in those times. The bursting of the shells and the thunder of French artillery was distinctly heard. It was an impressive scene to see these learned men discussing a civilisation which was swept from the earth so many centuries ago at a time when their own country was threatened by ruin not less awful and perhaps more disgraceful. The *Comptes Rendus* of the 7th April had gone through the press as usual. Its most important article was a communication from Prof. Simon Newcomb on the new method invented by him for discussing the inequalities of the moon's motion. The extract, four pages in length, is an abstract from the original communication, which was left by the American astronomer in the hands of the Committee instructed to report upon it. These *Comptes Rendus* are printed by Gauthier-Villars, printer to the Academy, at a great expense, and with the greatest difficulty. The continuation of the publication is highly creditable to that firm, of which the head, M. Gauthier-Villars, is a former pupil of the Polytechnic School. To show how difficult the business must be to manage, we must say, moreover, that the publisher of the *Connaissance des Temps* for 1872 is stopped merely because it is impossible to find working men for the printing of the last four sheets, which are ready to go through the press. If things continue for some time, French navigators sailing for distant Pacific Ocean expeditions will be obliged to resort to the Nautical Almanack.

## DIARY

*THURSDAY*, May 11.

Royal Society, at 8.30.—An Experimental Inquiry into the Constitution of Blood, and the Nutrition of Muscular Tissue: Dr. Marcet, F.R.S.—On Non-Spontaneous Generation. On the Influence of Heat on Protoplasmic Life. On the Preparation of Nitrogen: Prof. Crace-Calvert, F.R.S.
Society of Antiquaries, at 8.30.—Sepulchral Remains at Rouen: The Abbé Cochet, Hon. F.S.A.—Letter to Mr. John Stanhope, from Sir Geo. Buck: Earl Stanhope, President S.A.—Sir James Tyrrell cleared (A.D. 1483): Rev. W. H. Sewell.
Mathematical Society, at 8.—On the Singularities of the Envelope of a non-Unicursal Series of Curves: Prof. Henrici.—On the Resultant of a large number of Vibrations of Irregular Phase, as applied to the Explanation of the Coronas: Hon. J. W. Strutt.—A Question in the Mathematical Theory of Vibrating Strings: W. Spottiswoode, F.R.S.—On the Problem of Finding the Circle which cuts Three given Circles at given angles (communicated by Prof. Cayley, F.R.S.): J. Griffiths, M.A.
Royal Institution, at 3.—On Sound: Prof. Tyndall.
London Institution, at 7.30.—On Economic Botany: Prof. Bentley.

*FRIDAY*, May 12.

Astronomical Society, at 8.
Quekett Microscopical Club, at 8.
Royal Institution, at 9.—On the Defence of the United Kingdom: Col. Jervois, R.E.

*SATURDAY*, May 13.

Royal School of Mines, at 8.—Geology: Dr. Cobbold.
Royal Institution, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

*MONDAY*, May 15.

London Institution, at 4.—On Astronomy: R. A. Proctor, F.R.A.S.
Anthropological Institute, at 8.—On Dreams, Sympathy, Presentiment, and on Divination and Analogous Phenomena among the Natives of Natal: Dr. H. Callaway.—Notes on a Cairn at Khaogaum, and on a Kist in Argyleshire: Dr. A. Campbell.

*TUESDAY*, May 16.

Statistical Society, at 7.45.—On the Influence of a High Bank Rate of Discount on Monetary Crises: R. H. Patterson.
Zoological Society, at 9.—A Description of the Madreporaria dredged up during the Expedition of H.M.S. *Porcupine* in 1869-70: Dr. P. Martin Duncan.—On Speke's Antelope and the allied species of the genus *Tragelaphus*: Sir V. Brooke, Bart.—On a new Humming-bird, discovered by Mr. Whiteley, in Peru: Mr. J. Gould.
Royal Institution, at 3.—On Force and Energy: Charles Brooke, F.R.S.

*WEDNESDAY*, May 17.

Society of Arts, at 8.—On the Utilisation of Prison Labour: Captain E. F. Du Cane, R.E.
Royal Society of Literature, at 8.30.—On Shakespeare's Birthday: C. M. Ingleby, LL.D.

*THURSDAY*, May 18.

Royal Society, at 8.30.
Society of Antiquaries, at 8.30.
Chemical Society, at 8.
Royal Institution, at 3.—On Sound: Prof. Tyndall.

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Erratum.—In vol. IV. p. 20, 2nd column, line 7, for "N. Hartog" read "M. Hartog."

THURSDAY, MAY 18, 1871

## *THE PEOPLE'S UNIVERSITY*

A GIGANTIC and imposing educational scheme is about to be launched, which, whether it proves feasible or not, must attract the attention and enlist the sympathy of all well-wishers to the intellectual development and material welfare of the country. This is no less an idea than the establishment of a National Working Men's University, which is to be founded with special reference to instruction in those subjects which have a direct bearing on the arts and manufactures. That our workmen are, as a rule, altogether ignorant of the scientific principles upon which the processes they ought to guide and govern are dependent, and that England in this respect stands in a much inferior position to continental nations, is now a well-recognised fact. The result of this lamentable ignorance is stated by certain authorities to be severely felt in those of our trades and manufactures in which we have to compete with other nations; and although this conclusion has been denied by many, yet concerning the necessity for scientific education amongst our artisans there has never been a difference of opinion. The question then arises, How are we to bring to our rising artisans on an extended and national scale the knowledge of scientific principles which they so much need, and for which the best of their class show so much desire and even aptitude? One solution to this problem is being attempted by the scheme of a National University for Industrial and Technical Training. The proposal is to establish a metropolitan institution in which complete and thorough instruction in all those branches of knowledge which are of importance to our manufacturing industry shall be given. It is proposed (1) to build ample lecture-rooms, laboratories, art (as well as scientific) museums on the most extended scale; (2) to create professorships both of the pure sciences and of such more technical subjects as can be systematically treated, and we will also hope chairs of at least such literary subjects as the modern languages; and (3) to found scholarships by which artisans may be enabled to live during the years of their studentship. This central university is to be connected with other similar institutions scattered over the country in the foci of the industrial pursuits, each carrying out in its locality the same function which the central one is to perform perhaps on a somewhat higher scale for the metropolis and the country in general.

The idea is a noble and grand one, but the difficulties of carrying it out are immense, whilst the dangers of the scheme proving abortive are scarcely less so. The first requisite in such a scheme for artisan education is money, the second condition of success is good management. If the wealthy city guilds come forward to the good work with subscriptions of tens and hundreds of thousands, and if men of ability and of high views and of sound practical knowledge on educational questions undertake to work the scheme, the University of the People may possibly become a reality.

That the best of the English artisans value a scientific training when it is placed within their reach, is a matter which has now been satisfactorily proved, and if any system of high science instruction can be inaugurated by which the force of thousands of powerful brains, now lying dormant, can be made productive, an increase of energy will be gained to the country of which we cannot form the slightest conception.

When, however, we come to count the cost, we may well doubt the accomplishment of the design, for we must set it down as a first principle that every artisan must not only be gratuitously taught, but also kept during the period of his studentship. This would necessitate a scholarship of at least 40*l.* per annum for each student; or 40,000*l.* for every thousand students; add to this a like sum (a moderate estimate) for the payment of professors, expenses of working the science departments, museums, &c. we see that each student will cost probably nearly 100*l.* There is, of course, plenty of money, even in the metropolis itself, which might with propriety be applied to this most laudable object, but whether such a sum can be raised as shall yield an annual income say of 80,000*l.* to 100,000*l.* large enough to support a People's University on a truly national scale (and anything less than this would be a practical failure) appears more than doubtful. For although the importance of this movement, in a national point of view, cannot in reality be overestimated, it is but too evident that this opinion is not held by the world at large, and certainly not (unless they are much belied) by rich corporations or city companies; and without aid from some such old and wealthy foundations, a scheme of this kind can scarcely be permanently supported.

The financial are, however, by no means the only or the most important difficulties which will beset the new University. These will only begin to be felt when the scheme has been started—such as dangers of giving an instruction too purely theoretic, or of running into the worse evil of teaching details without scientific *aperçu*.

In face of such difficulties it all the more behoves those who really believe the movement to be a wise and beneficial one, to exert themselves to support it. It is simply a duty to draw attention to a proposal which, if properly carried out, may improve to a very important extent the condition of Science in England.

H. E. ROSCOE

## *THE SUN*

*Le Soleil. Exposé des principales découvertes modernes sur la structure de cet astre, son influence dans l'univers et ses relations avec les autres corps celestes.* Par le P A. Secchi, S.J. Pp. 422, 8vo. (Paris: Gauthier Villars, 1870. London: Williams and Norgate.)

*The Sun: Ruler, Fire, Light, and Life of the Planetary System.* By Richard A. Proctor, B.A., F.R.A S. Pp. 480, crown 8vo. (London: Longmans, Green, and Co., 1871.)

DURING the past few years the number of workers in the domain of solar physics has been so great, their progress so rapid, and the results of their labours have been published in so many forms, that it has been difficult to keep pace with them. Under these circumstances, a summary of these labours, which shall extract what is most valuable from all, and refer the reader to the original publications for the remainder, is a great desideratum.

The work of the Père Secchi seems designed not so much to supply this particular want as to give a general popular *resumé*, of what is known of the physical constitution of the Sun. It would therefore scarcely be just to measure it by the standard in question, and all the less just because the very branch of research in which the author is most eminent is that of which he speaks the least. Only one chapter and part of another are given to spectrum analysis of the solar light and its results, and as the operation of spectrum analysis itself is described at some length, there is little space left for the discussion of the results. Roughly speaking, one half the book is devoted to a description of purely optical phenomena as observed with a telescope. The appearances and movements of the solar spots are in particular treated at great length. The other half is devoted to radiation, temperature, gravitation, spectrum analysis, and the relation of the sun to the stars. The chapter on Radiation seems designed to save the reader the trouble of referring to elementary works on natural philosophy or chemistry, and the next has nearly the same object with respect to astronomy.

The chapter on Temperature is that which has most piqued our curiosity. The author calculates that the temperature of the sun must be at least ten millions degrees centigrade. On examining the process by which he reaches this conclusion, we find that he sets aside the law of radiation of Dulong and Petit, and substitutes that of radiation proportional to the simple temperature. He gives no reason for the adoption of this new law, and we were not aware that the other had been disproved. The question is of importance, for, if the law of Dulong and Petit were true, a sun at a temperature of ten million degrees would speedily reduce our earth to vapour. It would be interesting to measure the temperature of a furnace by the effect of its radiation upon a thermometer, in the same way that Père Secchi has measured that of the sun.

The paper and typography of the work are excellent, and among the illustrations are six finely-executed charts of stellar spectra, illustrating the author's classification of such spectra.

Mr. Proctor's work, while covering nearly the same general ground with the former, is much more complete in its account of recent observations and theories, especially of the phenomena of solar eclipses. It is, on the whole, better than might have been supposed from its stilted title.

The first chapter is designed to give an historical discussion of the solar parallax. A history of this subject at the same time popular, accurate, and complete, is indeed much to be desired; but Mr. Proctor's is imperfect and inaccurate in a remarkable degree. He begins very well, but grows worse and worse as he approaches his conclusion. He suddenly stops his history with the year 1868, and ignores all that has been said or done since. Confining ourselves to two or three paragraphs and a note near the close of the chapter, we find the "ligaments," "black drops," and distortions sometimes seen in interior contacts of the limbs of Mercury or Venus with that of the Sun, described as if they were regular phenomena of a transit, without a mention of the facts and experiments which indicate that these phenomena are simple products of insufficient optical power and bad definition, which disappear in a fair atmosphere, with a good telescope well adjusted to focus; and this is followed up with a grave proposal to measure this product of bad definition during the next Transit of Venus. One might suppose, from his closing statements, that Mr. Stone was the first to "infer from the account given by the different observers, whether real or apparent contact was noticed," and to allow for the difference between the two. The subsequent examinations of the observations used by Mr. Stone are, with a single insignificant exception, entirely ignored. We cannot, therefore, but wish that the author, before printing this chapter, had submitted it for revisal to some one acquainted with the subject.

In the second chapter we find the author more at home. We rarely see the accuracy of the mathematician united with that vigour and clearness of style so desirable in the popular presentation of truth. Mr. Proctor, however, here seems to unite both qualifications in a high degree.

The third chapter gives a very clear and satisfactory account of the first principles of spectrum analysis. The historical and the logical development of this subject coincide remarkably with each other, and it is therefore that very properly adopted in its presentation. We find one statement which we must ask leave to doubt, until a more satisfactory proof is given than we have yet seen. It is that the intensity of the D light (if we may use the expression) of incandescent sodium vapour is not only apparently but actually diminished by passing sun-light through it. If this were so, it would follow that the sodium flame not only absorbed the light in question, but that, in doing so, it lost the power of emitting it. This would, indeed, be a remarkable result. We understand Kirchhoff, in the experiment alluded to, to speak only of relative light and darkness, and to assert that the D part of the combined spectrum is less bright than the surrounding and intermediate parts. But we cannot conclude from this that there was really less light there than when the sodium flame shone by itself, as Mr. Proctor does.

The succeeding chapters give a very full, classified summary of recent observations upon the sun, the protuberances, the corona, and the zodiacal light. The accounts of the phenomena observed during total eclipses are carried up to that of August 1869. From the preface it would seem that the work was passing through the press in December 1870, and it is a pity it could not have been completed by adding the observations of the eclipse during that month. The discussion of theories of the corona and protuberances is evidently honest, and perhaps intended to be complete. He tries to disprove the "atmospheric glare theory" by showing that no part of our atmosphere in the direction of the corona is illuminated by direct sunlight, a proposition which we apprehend no one ever maintained. But we know that every bright celestial object is surrounded by a certain amount of stray light, due to atmospheric reflection, which increases rapidly in intensity as we approach the object; and such a light must therefore surround the real corona and protuberances. We also know that every bright object of this kind appears larger than it really is, and of a different form, from mere optical illusion. Until these two effects are eliminated, we can gain no positive knowledge either of the exact form or the exact extent of the real objective corona. The "meteoric theory" of the corona and zodiacal light, sustained by the author, is subject to objections as grave as those he brings against other theories;

but we have no room to explain them at length in the present article. It is the less necessary to do so, as the final conclusion of the subject is very well embodied in two lines of the table of contents :—"The origin of the prominences still a mystery," "The corona's true nature also unknown."

Respecting the general spirit of the work, it may be remarked that while the author doubtless intends to do justice to all the investigators whose labours he describes, there is one feature of the work which may lead the reader to doubt whether he has really done so. We refer to the indications of personal feeling scattered here and there, and the depreciating tone adopted in treating of the labours of those he does not personally like. However this may be, there are few or no popular expositions of a scientific subject in which the observations, opinions, and labours of so many men of science have been collected and referred to their authors.

S. NEWCOMB

## *FOREIGN SCIENTIFIC ASSOCIATIONS*

*Proceedings of the Scientific Association of Trinidad*, 1866—69, Port of Spain. (London : Trübner and Co.)

*Proceedings of the Essex Institute.* Vols 4 to 6. 1864—70. Salem, U.S. (London : Trübner and Co.)

*Journal of the North China Branch of the Royal Asiatic Society.* 1864—68. New Series. (Shanghai : A. de Carvalho. London : Trübner and Co.)

THE Scientific Association of Trinidad has now been in existence for some years. Its object is "the cultivation of scientific knowledge in the West Indies ;" and if we may judge by the character of most of the memoirs contained in the parts of the Proceedings hitherto published, it must be a very useful society.

Dr. Mitchell has communicated more papers than any other member. He has contributed articles "On the Use of Sulphites in Medicine," with an "Additional Note on the Use of Sulphites and Bisulphites, whether Medicinally or otherwise," "On Earth Closets," "Hints on the Breeding and Rearing of Horses," "On the Manufacture of Sugar by the Process of Drying the Cane," and "On the Manufacture of Sugar by Evaporation." Mr. Guppy contributes three papers, "On the Mollusca of Trinidad," "On Petroleum and Naphtha," "Remarks on the Cultivation of Scientific Knowledge in Trinidad," "On the Tertiary Fossils of the West Indies," and "On the Marine Shells found on the Shores of Trinidad." Amongst other articles of permanent interest we may especially mention Dr. Goding "On the Petroleum or 'Green Tar,' and the 'Manjack' of Barbadoes," the Hon. Richard Hill, "On Poisonous Fishes," and "On Fish Poisons ;" and Mr. Prestoe's "Catalogue of Plants in the Royal Botanic Gardens." Many of the subjects treated of in these Proceedings serve to illustrate various points described by the Rev. Canon Kingsley in his charming "Letters from the Tropics."

The Essex Institute seems to have commenced its existence as the Essex County Natural History Society, and it published a "Journal" as early as 1836. This Journal subsequently merged in the "Proceedings" and "Historical Collections" of the Institute, the former commencing in 1848, and the latter in 1859. It is only with the "Proceedings" that we have to deal at present, and the volumes now before us contain "The Records of the Meetings, the Written Communications on Natural History and Horticulture, and the Naturalist's Directory." Amongst the most important memoirs we may especially notice Morse "On a Classification of Mollusca based on the Principles of Cephalization ;" Verrill's "Synopsis of the Polyps and Corals of the North Pacific Exploring Expedition from 1853 to 1856, collected by Dr. Stimpson ;" Hyatt's "Observations on Polyzoa ;" Dr. Wilder's "Revision of researches and experiments upon Silk from Spiders, and upon their Reproduction, by Raymond Mariâ de Termeyen, a Spaniard, translated from the Italian ;" Horace Mann, "On the Flora of the Hawaiian Islands ;" Cowes's "Catalogue of the Birds of North America in the Museum of the Essex Institute ;" Wood, "On the Phalangeæ of the United States ;" and Packard "On Insects inhabiting Salt Water."

These quarterly "Proceedings" came to a close at the end of the year 1868, when the "Bulletin of the Essex Institute," which appears in monthly parts, took its place. The "Bulletin," which we shall take an early opportunity of noticing, contains "All the short Communications of General Interest, both of an Historical and Scientific character, made at the Meetings of the Institute, and the Records of the Meetings and Business of the Institute."

Turning from the West to the uttermost parts of the East, we take up the "Journal of the North China Branch of the Royal Asiatic Society," of which the new series commenced in December 1864, when the Society which had been formed in 1861 was reorganised.

The papers contained in this Journal are for the most part very interesting, in consequence of their treating of subjects on which comparatively little is known in this country. The geographer will find articles "On the City of Yeddo," "On the Overland journey from St. Petersburg to Pekin, "On an Overland trip through Hunan from Canton to Hankow," "On the Sea-board of Russian Manchuria," "On a Journey from Pekin to Chefoo *viâ* the Grand Canal," "On a Journey from Pekin to Shanghai," and "On a Journey from Canton to Hankow through the Provinces of Kwangtung, Kwangsi, and Hunan." The naturalist (using the term in the widest sense) will find articles "On the Geology of the Great Plain, and of a portion of Quangtung Province," "On the Coal-fields in the South Eastern Province of China," and "On the Bituminous Coal Mines west of Pekin," "On the Birds and Beasts of Formosa," "On Chinese Notions regarding Pigeons and Doves," "On some Wild Silk Worms of China," "On the Entomology of Shanghai," "On the Sorgo or Northern Chinese Sugar Cane," and "On the mineral and other productions of North China and Shantung." Amongst other valuable papers may be mentioned those by the late Dr. Henderson "On the Medicine and Medical Practice of the Chinese," by Dr. Bastian "On the Remains of Ancient Kanbodies," Dr. Keer "On the Great Examination Hall at Canton," the Rev. A. Wylie "On the Opinions of the Chinese with regard to Eclipses, and on the Eclipses recorded in Chinese works," Mr. Hollingworth "On the Chinese Game of Chess," Mr. Forrest (acting Consul at Ningpo) "On the Christianity of Hung Tsiu Tsuen, being a Review of Taeping Books," and the Rev. S. R. Brown's translation of a curious old Japanese manuscript entitled "Annals of the Western Ocean." The last-named article is one of singular interest in many respects. It is divided into three parts, the first of which contains an account of the arrest of a Roman Catholic priest upon an island called Yaku-Shirna in the year 1708, his removal to Nagaski and examination there, and his subsequent arrival at Yeddo, imprisonment, trial, and death. The name of the person as given in Japanese syllables was Jean Baptista Shirotte, and he is supposed to have been the last Roman Catholic missionary who landed in Japan previous to the year 1859. The second part contains the report of the prisoner's examination, and the information obtained from him respecting the military and naval power, and the wars and conquests of the Western nations; while the third comprises the missionary's answers to the questions put to him about himself and his family connections, his reason for coming to Japan, and his religious creed.

From the very curious paper on the "Birds and Beasts of Formosa" which is translated by Mr. Swinhoe, H.B.M. Council at Taiwan, from the 18th chapter of the "*Tai-wan-foo-che*, or Statistics of Taiwan," we learn that "as soon as the doe that has finished suckling observes her roe getting to maturity, she deserts it and repairs to other hills, fearing that her young might entertain an improper affection for herself. Animals do not confuse the laws of consanguinity, the horse excepted. The doe deprives her offspring of any such opportunity by setting a distance between herself and her young." We have quoted this passage because it contains almost the identical views expressed by Aristotle,* but we suspect that this idea is not based on any sound foundation.

Several of the articles, and especially those of Dr. Henderson "On the Medicine and Medical Practice of the Chinese," and of Mr. Walters "On Chinese Notions about Pigeons and Doves," throw considerable light on the absurd mode of practice adopted by the native doctors. From the latter paper we learn that the eggs of pigeons are an antidote to the injurious effects of boils and smallpox. Some persons may think the remedy worse than the disease, as the following course has to be followed:—Two eggs must be hermetically sealed in a bamboo tube and placed in the middle of a cesspool for half a moon. The whites are then to be mixed with three ounces of *shen-sha*, a very fine red sand-like substance, and the compound is to be divided into pills of the size of a green pea. If thirty of these pills are taken three times a day, the patient will soon find relief, for the poisonous matter will be rapidly discharged by the bowels and kidneys. The excrement of the same bird, when roast to cinder and soaked in wine, forms a cure for cold on the chest, and there are several other affections in which it is very useful. Let us conclude with a pleasanter remedy. "Of the *shi-chin* or wood-pigeon it is written that its flesh is sweet, delicate, and without poison. It also gives one a composed mind, and enables him to do with little sleep. Its foot and leg bones have the very delightful quality of exciting affection between husband and wife. If on the fifth day of the fifth moon the husband takes one of these bones and the wife takes one, each putting the bone in a basin of water, one from the left and the other from the right side, the two bones will come together and float together, thus indicating a long and happy union to the parties trying the experiment."

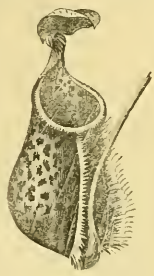
G. E. D.

* See his "History of Animals," Book ix., chap. 34 (Creswell's Translation in Bohn's *Scientific Series*), in which he tells two very remarkable stories regarding a camel and a stallion in relation to this subject.

## *OUR BOOK SHELF*

*A Manual of Structural Botany for the Use of Classes, Schools, and Private Students.* By M. C. Cooke. New Edition. (London: R. Hardwicke.)

WE have so often felt it our duty to expose the incompetence of those who attempt to write elementary text-books of science, that it is a real pleasure to come upon one like Mr. Cooke's "Manual of Botany," where a man of really accurate scientific knowledge applies himself to writing an elementary work on the rudiments of his science. The special object of the publication, as stated in the preface, is to supply a cheap manual to place in the hands of students in the Botanical Classes established for operatives in connection with the Department of Science and Art; but it may well be used as a first book to prepare for other objects, as, for instance, for the first B.Sc. examination, or that for Women, at the University of London, though it would then have to be supplemented by others on the systematic branch of the subject. The descriptions are clear and accurate, and expressed in commendably terse language. It is illustrated by over two hundred woodcuts, some of them of decided merit; and we have reserved our crowning sentence of commendation till the last—the price is one shilling! A. W. B.



Pitcher of Nepenthes

*Geographisches Jahrbuch.* III. Band, 1870. Unter Mitwirkung von A. Anvers, J. Baeyer, A. Fabricius, A. Griesbach, Fr. Müller, Fr. Neumann, L. K. Schmarda, F. R. Seligmann, J. Spörer, H. Wagner:—Herausgegeben von E. Behm, Mitredakteur von Petermann's Geogr. Mittheilungen, 1870. (Gotha: Perthes. London: Williams and Norgate.)

WE lately had occasion to speak in terms of high commendation of Vivien de St. Martin's *Année Géographique*, and we can award equal praise to Behm's corresponding work, which is the more elaborate of the two, and consequently the less agreeable to the ordinary reader. It is divided into four parts, devoted respectively to Geographical Chronology, Geographical Statistics, Essays on the Progress of Geographical Knowledge, and Tables of use in Mathematical Geography. The first part consists of a geographical calendar, stating the date of the discoveries of various countries, of the birthdays and deaths of great geographers, &c. (for example, on the day on which we are now writing, April 22nd, J. Richer arrived at Cayenne, 1672; the island of Rea or Wallis was discovered by Maurelle, 1781; Reao was discovered by Duperry, 1822; Denham arrived at India (Mandara) 1823; and the *Novara* sailed from Singapore, 1858); and it treats of the manner in which time is calculated in certain countries. The second part is extremely valuable, but is very dull; any information that may be required as to the state of the population of any country, of the number of houses and inhabitants in a square mile, &c., may be readily found here. The third part consists of extremely valuable memoirs by Baeyer, on the progress lately made in the measurement of the degree; by Griesbach, on the Geography of Plants; by Schmarda, on the progress of our Knowledge of the Distribution of Animals; by Seligmann, on the Progress of Ethnology; by Müller, on Linguistic Ethnography in Relation to Anthropology; by Fabricius, on the Progress of our Knowledge of National Statistics; by Spörer, on the History of Geography; by Neumann, on the Products, Merchandise, and Currency of Different Nations; and by Behm, on the most important Geographical Travels during the years 1868-69. Behm's memoir, which extends over more than a hundred pages, is unquestionably the most valuable portion of the book, and next in order of interest, at all events to the naturalist, we should place the essays of Schmarda and Griesbach. The last part of the volume is purely numerical, and requires no comment. Everyone desirous of keeping himself up to the existing level of geographical knowledge should purchase both the German and French annuals. For those who must content themselves with a single volume, we should say the French one was the better.

G. E. D.

*The Romance of Motion.* By Alec Lee. (Longmans: 1871.)

THIS is another of those books in which the author does not understand the first principles of the science with which he deals. The laws of motion seem to be affording more than usual trouble to certain people just now, and most unfortunately they write books about it couched in the longest scientific terms and the most formidably accurate-looking phraseology. The author alleges, as one of the extraordinary paradoxes among the opinions of the nineteenth century, "how all bodies are supposed to persevere in their state of rest or of motion, in a straight line, unless compelled to change that state of rest or motion by the impression of some force on them; and how, in opposition to this law, the planets become accelerated and retarded in their orbits without such adequate impression of force; also how bodies initially projected at the surface of the earth, fall by the force of gravitation with velocities uniformly accelerated, and how the planets similarly projected descend towards the sun with velocities comparatively equal throughout the entire duration of their revolutions." We need hardly remind the reader that these conclusions, so far from being in any way in *opposition to* the law of motion stated by the author, are in complete harmony with that law, and, as was demonstrated by Newton, follow from it on the hypothesis (to give it no higher name) of gravitation. The author at least might have observed, in comparing the case of the stone and of the planet, that the direction of the force on the former is unaltered, while that of the force on the latter is continually changing.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Thickness of the Earth's Crust

ARCHDEACON PRATT's explanation in NATURE of May 11 seems to assume that a rigid body moving in contact with a fluid body can never communicate its own rate of motion to the latter as quickly as it would do if this were also a rigid body attached to itself. Supposing the earth to consist of a rigid crust inclosing a fluid interior, and the crust to be moved by the forces producing precession, it would, he says, "slip over the surface of the revolving fluid through a small space proportionate to the push given to the poles. The fluid could not possibly acquire in an instant this new motion, however small it might be, because the fluid is not rigidly connected with the crust."

I venture to suggest that if in the last sentence Mr. Pratt would substitute the word "slow" for "small," the question would have a different aspect, notwithstanding his subsequent statement to the contrary.

Strictly speaking, when a body, however rigid, is moved, the whole of it never moves instantaneously. The particles on which the moving force immediately acts move first, and the rest move in succession afterwards. The smallness of the interval is the measure of the rigidity, but some interval must always be assumed. Bodies move as a whole through the attractive or repulsive forces of their particles; and every such force resolves itself into a power of moving something through a certain space in a certain time. The reason why a moving solid will "slip over" the surface of a fluid instead of carrying it with it, is that the rate at which it can carry it with it by reason of its attractive force is commonly less than the rate at which the solid is moving. But if the motion is slow enough to exceed that which the attractive force will cause in the fluid, it will slip over no longer; and if it be so slow that not only the power of the solid over the fluid, but of the fluid particles over each other, is able to produce an equal rate of motion, the whole mass will move together as if it were a rigid body. This rate will depend chiefly on the nature of the fluid. If a metal plate four inches in diameter is filled with lamp oil, and made to rotate at about one revolution in three minutes, the oil will move with the plate without appreciable retardation, though if the speed be doubled, the oil is seen to be "slipped over." If water is used, or the size increased, the rotation must of course be very much slower. It would appear on these grounds, I think, that the extremely slow movement of precession might practically affect the whole body of the earth as if it were rigid, notwithstanding the granting of a fluid interior.

May 14 A. J. M.

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IT requires no little courage to attack so eminent a mathematician as Archdeacon Pratt on his own ground, and it is, therefore, with the utmost diffidence that I venture to suggest that in his defence of Hopkins against Delaunay in your last number, he has mistaken a mathematical fiction for a fact.

In calculations involving quantities which vary in magnitude, the imperfection of our methods oblige us to have recourse to an artifice, and for the benefit of the non-mathematical reader, I will try to explain what this artifice is, taking the case of nutation as an example:—

The motion of the earth's axis which is known by this name, is caused mainly by the attraction of the moon on that part of the earth which lies outside a sphere, whose centre is the earth's centre, and its radius the polar radius of the earth. Now this force of the moon's attraction is never the same in magnitude; and, however small be the interval of time we consider, it is not the same at the end as the beginning of that interval; it is incessantly changing. Everyone will realise the difficulty of estimating the effect of such a force. This difficulty is got over by the artifice I mentioned, which is as follows:—The time is divided into a number of small intervals, and the attraction is supposed to keep during anyone of these intervals the magnitude which it has at the beginning of that interval, and at the end of that interval *suddenly* to assume the magnitude which it has at the beginning of the interval next following, and so on; the force, in short, instead of varying by insensible changes, is supposed to act by a series of fits and starts. This must be what Archdeacon Pratt means when he talks of "a succession of slight horizontal pushes being given to the poles." The amount of motion produced during each interval on the above supposition is then determined, and these amounts are added together to obtain the displacement produced.

It is clear enough that such a method can only be approximately correct; but it is also clear that the smaller each interval is, the nearer will the hypothetical be to the real state of the case, and the nearer will the calculated be to the actual result. As long as the intervals are finite there must be some error, but the smaller the intervals are made the less will this error be. I need not go into the methods of mathematical analysis which enable us to get rid of this error, and which, when we have found out what will be the effects of a force acting with variable intensity by fits and starts separated by small finite intervals, enables us to deduce the effect of the same force when it comes to vary incessantly; for I hope I have made clear the nature of the mathematical artifice on which this analysis is founded.

Now it seems to me that Archdeacon Pratt all along reasons on the supposition that the moon's attraction acts after the manner the mathematical artifice I have described supposes it for the mere necessities of calculation to act. All his argument, if I understand it aright, depends upon the displacement being by fits and starts. Thus he says (NATURE, July 28, 1870), "The precessional force has its full effect in producing the precession of the solid crust, the fluid *not having time** to diminish that effect before the axis has assumed a new position;" and "The friction of the fluid within, which *has not time* to influence the nutation before the nutation is actually produced;" and (NATURE, May 11, 1871), "Suppose a succession of slight horizontal pushes to be given to the poles in a continually altering direction, the effect will be that the revolving crust will be continually slipping over the revolving fluid, which *has not time* to acquire the new motions given *instantaneously* to the solid crust."

The leading idea in all these passages seems to me to be that the attractions of the sun and moon, to which precession and nutation are owing, act by impulses, by a succession of sharp pulls quickly repeated. This is truly enough the supposition with which mathematical calculation starts; but the real action, I need not say, is a steady, continuous, though ever varying, pull, and it is the result of such an action which our calculations in the end lead us to, by a method which enables us to get rid of the error necessarily involved in the approximate result which would follow from our first supposition.

I cannot then help thinking that even Archdeacon Pratt has for once been carried away by the beauty of mathematical analysis, and has for the moment forgotten that the conditions which it is obliged to employ for its ends do not in their initial form represent the actual conditions of nature. The explanation occurred to me on first reading his paper in the *Philosophical Magazine*, but seemed to me so unlikely that I shrank from putting it forward. I can, however, in no other way imagine how he can have come to the startling conclusion, that, if a solid shall be moved by a steady, continuous pull over an interior ball of fluid, it can make no difference in the result, whether there is or is not friction between the interior of the shell and the surface of the fluid. Archdeacon Pratt, will, I know, if I am wrong, pardon my presumption and put me right.

Barnsley, May 12 A. H. GREEN

### Pangenesis: Graft-Hybrids

EACH person who assails this unfortunate "provisional hypothesis" makes the attack from his own particular point of view. Thus, in NATURE of last week Prof. L. S. Beale, as a microscopist, objects to it because the gemmules cannot be made evident to the senses. From this somewhat narrow view of the case the atomic theory of chemistry, the undulatory theory of light, or the mechanical theory of heat, must all break down, for no one has as yet seen an ultimate atom, or an ethereal undulation. Mr. A. C. Ranyard, in the same paper, publishes a letter which is quite at variance with fact, for if he will turn to pp. 390, 391, 394, 397 in vol. i. and pp. 364 and 365 vol. ii. of Mr. Darwin's work on "The Variation of Animals and Plants under Domestication," he will there find many cases given of the scion affecting the stock and producing intermediate forms known as "graft-hybrids." Pangenesis has not yet "received its death blow."

R. MELDOLA

May 13

IN your last number Mr. Ranyard brings forward an objection to Mr. Darwin's theory of Pangenesis on the ground that the grafting of a bud on a stock of a different species does not produce a hybrid offspring. I am not about to defend the doctrine of Pangenesis, which appears to me incapable alike of proof and of disproof. It is, however, a well-known fact that the stock does affect the scion, and *vice versâ*. In Prof. Henfrey's "Elementary Course of Botany" (Dr. Masters's edition) he says, "A certain amount of physiological influence of the stock over the scion is shown to exist by such facts of horticultural experience as that the fruit of the pear is smaller and more highly coloured when 'worked on' the quince and medlar than when grafted on pear-stocks, and is earlier when worked on the mountain-ash." The well-known instances of the communication of variegation from the scion to the stock in *Abutilon*, recorded by Prof. Morren and others, are considered cases of contagious disease; but what is the theory of contagion but that the blood or other

* I have taken the liberty of italicising those expressions which seem to me of vital importance to the argument in these quotations.

"fluid" of an animal or plant is affected by emanations, call them "gemmules" or what you will, from another individual? The same writer records an instance which he considers well authenticated of the production of the hybrid *Cytisus Adami* by the grafting of *C. purpureus* on *C. laburnum*.

ALFRED W. BENNETT

### The Rev. Mr. Highton and Thermodynamics

YOU are cruelly kind to Mr. Highton in giving him space to develop his absurdities.

His new remarks on Joule, like his earlier ones on a paper by Sir W. Thomson, simply show that *he does not understand* what he ventures to criticise. Of course, what Joule now says is precisely what he said a quarter of a century ago, with the simple difference that it is put in a somewhat more popular form.

No one who has taken the trouble to understand the experimental *facts* and the elementary reasoning of which the Laws of Thermodynamics are the condensed expression, has any more doubt of their truth than of the truth of Newton's Laws of Motion. They are, perhaps, a little harder to understand; but the proof is of the same nature, and already almost of the same extent, in the newer science as in the older one.

I have not seen the *Review of Popular Science* referred to by Mr. Highton, but I hope (for the credit of that journal) that he misconceives its statements as he does those of Joule.

Your "first reviewer" (or rather précis-writer) of his article, whoever he may be, certainly gives him no encouragement in the number for Jan. 19, whatever may have been the effect of my treatment of his not singular case.

YOUR REVIEWER

### On the Radial Appearance of the Corona

WOULD an indefinitely extending solar atmosphere, if its existence could be proved, be in itself sufficient to explain the appearance of the solar corona? Should we not still have to explain the apparent radiation which is so distinctly part of the phenomenon?—If the light or heat of the sun which radiates symmetrically outwards as from a point at its centre be the cause of the illumination, surely the figure of the corona would bear some relation to the figure of the atmosphere or medium on which the light or heat acts? Yet I think I may say that it is quite impossible to conceive a medium so distributed and arranged as to form rays such as those seen in the corona. If the recent photographs had not shown beyond a doubt that this irregular radiating appearance belongs to the corona and the neighbourhood of the sun,* it would have gone a long way to prove that the corona is at least partly due to the earth's atmosphere or mere optical effect. But, as it is, I think this radiation clearly proves that the corona cannot be due to the direct action of the light and heat of the sun on any surrounding matter. In fact, I cannot conceive an atmosphere the character of which varies in a radial manner, however rapidly either its nature or density may vary with the distance from the surface of the sun. If, instead of an atmosphere, we try to conceive a ring of meteors, still the radial gaps so clearly marked on the photographs present insurmountable difficulty. This, moreover, is impossible on other grounds. It is impossible that there can be an almost homogeneous mass of meteoric matter circulating round the sun in the form of an outer sphere, and if it circulated in the ecliptic or any other plane, it would present the appearance of Saturn's belt, whereas the corona appears altogether different from this, and cannot possibly be a film of light in any plane but that of the sun's limb.

Nor can these radial rifts be of the nature of shadow. For the shadow which anything like a sun spot would produce in a misty atmosphere must be conical, the vertex of the cone being outwards, so that the edges of the shadow would approach each other instead of receding as they do. Moreover, such a shadow would still be seen through a great extent of illuminated solar atmosphere, and therefore be only partial or faint, whereas the rifts are so dark and definite as to imply a total absence of coronal light; this must be the case unless the rifts or gaps in the spherical envelope extended right across the sphere from front to back, and we know that there is no obstruction on the surface of the sun that could cast such an extensive shadow.

What, then, does this radiate appearance show the corona to be? I think that it proves that the corona is an emission either of illuminated matter or of an action illuminating matter,

* Has this yet been established?—ED.

such as electricity, driven off unequally from parts of the sun's surface, and in directions radiating from points either on or beneath the surface.* If only a small portion of the sun's surface, such as that covered by sun spots, sent out such streamers, the appearance might exactly coincide with that of the corona; for these streamers, when seen projected on to the plane of the sun's limb, might in some places appear to overlap so as to form a continuous corona, whereas in others they might appear to be separated by gaps.

So far as the appearance is concerned, it would be the same whether the emission consisted of matter or was electricity; there are, however, other indications of its being of the latter kind.

The action which the sun exerts on terrestrial magnetism shows it to be in an electric state, and the observations of Stewart and others have established a connection between the variations in its electric condition and the changes in the sun-spots and red flames, and the observations on the recent eclipse have connected the red flames with the brighter parts of the corona. Here then we have a distinct and independent reason for assuming that the electric condition of the sun's surface is partial and unequal, and for connecting the corona with this electricity.

As I have already ventured to explain the solar corona, as well as comet's tails and the aurora, to be a kind of electric brush, I now offer these remarks on the radial appearance of the corona in confirmation of my views.

Owens College, May 8 OSBORNE REYNOLDS

### A few more Words on Daylight Auroras

IN NATURE for December 29, 1870, there is a letter from Dr. G. F. Burder in reply to a previous correspondent,† who had sent a description, with an illustration, of a Daylight Aurora observed by him, wherein he made the following statement:—

But auroral arcs, as far as I know, never appear in the east, and the conclusion, therefore, is unavoidable, that the object observed was nothing more than a remarkably symmetrical form of cirrus cloud."

He then states his convictions that all records of so-called daylight auroras are "errors of observation."

As assertions like these might have an undue influence on the minds of those who read my letter on "Aurora by Daylight" (NATURE, May 4, 1871), I am induced to say a few more words on this subject, especially to prove the fallacy of such reasoning.

It is well known that the aurora borealis assumes innumerable shapes; some of the most remarkable were given by me some time ago in these pages;‡ and that they appear, at times, actually in the *east*, but more often in the north, north-west, and stretching to the south. A writer of some excellence in the last century § says:—

"Sometimes the aurora appears like arches, nearly in the form of a rainbow, reaching from one point of the horizon to another. The arches always cross the meridian at right angles, *tending to the east and west point of the compass.*"

The correspondent whom Dr. Burder is so hard upon, most probably saw the arc in a position nearer to this, than directly *facing* him, with his back to the *west*, and as the illustration sent by him shows only a segment of the arc, I am inclined to think that the extremities were nearly in the east and west.‖ The "cirrus cloud" hypothesis is simply untenable, when it is known positively that on several occasions the aurora was seen against an azure background, with no form of cloud in the field of view, as for instance that mentioned by me, where a faint arc was seen before sunset in the east (possibly N.E.) against a cloudless sky.¶ His other assertion, from which I must dissent, was: "A comparison of the auroral light with the light of other objects whose visibility can be more easily measured, tends strongly to confirm the view I have advanced." He then instances the invisibility of Donati's comet by daylight. He might have instanced the invisibility of the stars, although they can be seen in broad daylight when the observer is placed at the bottom of a deep pit; but this need not be done, for daylight does not always mean bright sunshine; and with diffused light, Venus is often seen before the sun has actually gone below the horizon. This, so far, may appear mere assertion, but the following will, I hope, be sufficient to show that the view he holds requires some kind of modification.

"A.D. 678.—This year the star (called a comet) appeared in August and shone like a sunbeam every morning for three months" (Anglo-Sax. Chron.).* By every morning I take to mean daylight, because in these months the mornings would invariably be very light, especially the few moments before the comet actually disappeared.

With regard to all the record of daylight auroras being mere "errors of observation," I am sure no one will continue to entertain such an opinion after carefully examining all that has been said upon the subject in these pages. As it may be useful to those who are interested in this question, I have made a summary of all the daylight auroras recorded in this and other publications, which do not admit of doubt.

A.D. 1122. A phenomenon appeared like a great and broad fire, and lasted till it was quite light. (Anglo-Sax. Chron.)

A.D. 1467. A *most probable day aurora*, described as "horsemen and men in armour rushing through the air."† (Ingulf. Second Cont.)

A.D. 1788. May 5 at 11 A.M. an auroral display seen, consisting of "whitish rays ascending from every part of horizon." Observed by "three different people." (Trans. Royal I. Academy for 1788, quoted by Rev. T. W. Webb in NATURE, May 11, 1871.

A.D. 1827. "Aurora Borealis seen in the day-time at Canonmills" at 4.30 P.M. Described in "Jameson's Journal" and NATURE for May 4, 1871. (Arcana of Science and Art for 1828.)

A.D. 1849.—In September an aurora seen, consisting of "three slightly diverging beams of light on the eastern horizon. One might have taken them for beams from a setting sun . . . had it not been that they did not emanate exactly from the spot where the sun had set; that they had an evident motion to the southard, and that two of them extended to the zenith, and finally down to the eastern horizon." ‡ (Mr. J. Langton, in NATURE, April 27, 1871.)

A.D. 1870.—September 4, about 4.30 P.M., an aurora observed "in the form of thin reddish streaks." ("S. B." in NATURE, October 13, 1870.)

A.D. 1870.—October 25, at 4.30 P.M., a brilliant aurora seen in the *east*, and fully described with illustration of it. (NATURE, December 8, 1870.)

A.D. 1870.—December. A *probable* auroral display, which was observed a "little before sunset," and developed as the evening advanced into a brilliant aurora borealis. The day phenomenon, however, not sufficiently described to make the record trustworthy. (J. Langton, in NATURE, April 27.)

A.D. 1871.—April 10, about 4.30 P.M., a whitish arc seen, almost east, against a cloudless azure sky. On the previous night there was a magnificent aurora borealis.§ (Mentioned by me in NATURE, May 4.)

These form the whole of the most reliable records, which are certainly few, for the period embraced between the earliest and present date; but I am inclined to believe that the occurrence of daylight auroras is not so rare as is here shown, but that they have been seen and actually recorded in the works which I have here and elsewhere quoted, but for the want of the statement of the time of day or night, one cannot tell to which the appearance belongs. Often a display which can be said to have been seen in the night, might as easily be said to belong to the day, so far as the actual wording of the record goes. It will follow from this that the scanty records we have of daylight auroras referred to phenomena of extraordinary magnitude and magnificence. JOHN JEREMIAH

### The Conservation of Force

I HAVE been endeavouring to understand what is meant by the Conservation of Force; and as it is one of the most interesting subjects I have studied, I send you the result of my labours.

* We are of opinion that there is still another explanation.—ED.

† NATURE, Dec. 8, 1871. ‡ Ibid, Dec. 29, 1871.

§ "Compendious System of Astronomy," by Margaret Bryan. London: 1797, p. 132.

‖ For evidences of night auroras being seen in the east, consult the letters in NATURE for 1870.

¶ NATURE, May 4, 1871, p. 8.

* This is confirmed in Beda, Flor. of Wor. (under A.D. 677) and Chronicum Scotorum (under A.D. 673 in error for 677).

† See also Pliny, Bk. II. c. lvii.

‡ This very singularly explains the following passage in Pliny:—"Round about the sun there was seen an arch when Lu. Opimius and Q. Fabius were consuls" (about B.C. 123). This was not an ordinary halo, for he says further:—"and a *circle* when L. Porcius and M. Acilius were consuls." (Bk. ii. c. xxix.)

§ It appears curious that the majority of the displays occurred at or about 4.30 P.M., in the autumn, winter, and spring months. Cases of magnetic disturbances during this hour are not rare, accompanying the aurora. It may prove of some value to note this.

The first law of motion laid down by Sir Isaac Newton (Princip. Math. Jes. Ed. tom. i. p. 15) is not a universal law, but is only capable of a restricted application. The incapacity of matter to alter its condition, whether of rest or motion, is a doctrine which becomes untenable when we examine matters which are always *proprio motu* altering their condition. Grave as such a statement may at first sight appear, we must begin with it if we wish to arrive at the truth.

Motion is the property which matter possesses of always changing its position relatively to other matter, and each little atom of the 64 elementary substances known to chemists contains a certain amount of tendency to move; this is a part of its nature, it would not be what it is without this; as that great mathematician, M. Poisson, says, the tendency to move resides in it. The five gaseous elements, for instance, have each their respective amounts of tendency to move residing in the atoms of which they are composed. Prof. Faraday says, in his "Researches in Chemistry," p. 454, "a particle of oxygen is ever a particle of oxygen—nothing can in the least wear it. If it enter into combination and disappear as oxygen—if it pass through a thousand combinations, animal, vegetable, and mineral—if it lie hid for a thousand years, and then be evolved, it is oxygen with its first qualities—neither more nor less. It has all its original force, and only that; the amount of force, which it disengaged when hiding itself, has again to be employed in a reverse direction when it is set at liberty."

Now what is the meaning of the word *force* which Prof. Faraday uses here? is it not the certain amount of tendency to move which I mentioned before?

A particle of oxygen contains a certain amount of tendency to move, without which it would not be a particle of oxygen at all; and this tendency it can never get rid of, "it has all its original force, and only that."

What, then, does the Conservation of Force doctrine amount to in plain English?

It amounts to the simple admission that the tendency to move is a property of matter inseparable from it and coexistent with it, and it is this tendency to move which is the cause of all the changes which we observe around us.

There is, however, nothing new under the sun, for the old doctrine of Argan in *Le Malade Imaginaire* is revived again; when Argan answers his examiner for a licence to practise in medicine, he says:—

Mihi a docto Doctore
Domandatur causam et rationem quare
Opium facit dormire
A quoi respondeo
Quia est in eo
Virtus dormitiva
Cujus est natura
Sensus assoupire.

Many a clever student has laughed at this answer who little thought that research and experience would confirm it so strongly as they now do.

The virtues of opium are chiefly dependent on the morphia which it contains, and morphia is one of the vegetable alkalis containing nitrogen in combination with carbon, oxygen, and hydrogen. The *virtus dormitiva* of morphia is the certain amount of tendency to move inherent in this combination; and this tendency, if the morphia is exhibited in the human subject, comes in contact with and retards the tendencies to move which certain component parts of the body possess, and produces that state which we call sleep. The salts of morphia are largely used to allay pain and produce sleep. Dr. Bence Jones says in his Croonian Lectures on Matter and Force, p. 84, "Stimulants, tonics, and evacuants may perhaps not only take part directly in the motions of any part of the body, but they may also promote or retard the conversion of one motion into other motions. Specifics and alteratives may directly as well as indirectly change the motions in the system. And sedatives and narcotics may have the same double action in retarding or stopping the motions that take place. This view will almo t lead us to consider all medicines as alteratives, and if so we may perhaps place stimulant and sedative medicines at the two extremes of the alterative actions; the stimulants giving rise to the greatest increase of motion, and the sedatives allowing the least motion or the nearest approach to rest."

The practical student of our day, when he speaks of terrestrial matter being at rest, means that it is then moving at the same rate of motion as the earth itself. Prof. Ansted treats of motion thus:—"The first and greatest lesson that the students of Geography and Geology must learn is that motion is not limited to masses of bodies, but is actually taking place always and under all circumstances within all masses, whether solid, liquid, or gaseous, and often without approaching the surface."—"Physical Geography," p. 2.

The Universe is one mighty system of changes, and these changes arise from the inseparable connection between matter and motion; and Dr. Bence Jones says truly, "The question between materialism and spiritualism is in fact only a question between ponderable and imponderable materialism."

Trinity College, Oxford N. A. NICHOLSON

## THE BIG GUN OF WOOLWICH

WHETHER considered as a weapon of terrible power or simply as a specimen of skilful and successful forging, the 35-ton Fraser cannon is without parallel. Of extraordinary strength and proportions, and withal so carefully, and one might almost say, elegantly finished, this magnificent gun is indeed a masterpiece well worthy of the greatest factory in England, from which it emanates. Cannon of larger dimensions have, it is true, been produced, capable actually of delivering a heavier projectile than that employed with the Woolwich weapon, but none of them are to be in any way compared with this, either in respect to battering power or length of range. That the gun is, moreover, not merely a show production, as was the case with the monster Krupp cannon, but a really serviceable and efficient fire-arm, is shown by its endurance of the severe test to which it was subjected at proof. On this occasion the 700lb. projectile was thrown from the gun by the enormous charge of 130lbs. of gunpowder—the largest, in fact, that has ever been safely consumed in any fire-arm—the explosion being without the slightest injurious effect upon the steel bore or surrounding wrought-iron castings. The solid cylinder of iron which constituted the shot issued forth at the terrible velocity of 1,370 feet per second, and, after travelling some fifty yards, buried itself in the butt of loose earth to a depth of thirty-three feet.

The pressure of the gas at the time of explosion was, as may be supposed, exceedingly great, and herein obviously lies the great difficulty to be overcome in the construction of large guns; this pressure or strain, we find, increases in a much greater ratio than the amount of powder that is burnt would appear at first sight to justify, and for this reason large guns require to be proportionately much stronger than little ones. Thus, in the present instance, when a charge of but 75lb. of powder was fired, the pressure of the gas upon the copper piston at the rear of the projectile was shown to be seventeen tons per square inch, while 130lbs. of powder (not double the former charge therefore) gave a pressure amounting to sixty-four tons on the square inch. It has, by the way, been questioned whether this method of estimating the pressure, by means, namely, of a copper piston which is pushed in upon itself, affords a strictly reliable test, but in any case there can be no doubt that the strain upon the gun is increased in a greatly increasing ratio to the quantity of powder consumed. When we state, therefore, that the weapon withstood in every part this excessive strain, and that, under ordinary circumstances, the cartridge will contain but 90lbs. of powder, there is every reason to believe in the solidity and perfection of the structure.

The data obtained by the firing of the gun at proof lead us to hope for very successful results from its employment. It is calculated that at a distance of fifty yards the heavy projectile would be thundered forth with such force as to penetrate fourteen and a half inches of solid iron, an armour plate such as no vessels of our present construction are enabled to carry. At two thousand yards—at upwards of a mile, therefore—the shot would possess enough penetrating force to pass clean through the side of the strongest ironclad afloat—those of the *Hercules* class—or, in other words, is endowed with impact sufficient to pierce twelve inches of iron; and it must be remembered that this last-named distance is one at which gunners can make very good practice, so that, under ordinary circumstances, every other shot would take effect against a target such as is presented by the keel of a large frigate. As regards extreme length of range, a quality of some importance, when, as in the recent instance of the Paris siege, great projecting power is of more importance than precision of aim, this Fraser gun may vie with almost any other, with the exception, perhaps, of Whitworth's cannon. The utmost distance to which "the Woolwich infant," as it has been nicknamed, will in all probability be capable of projecting a shell is about ten thousand yards, supposing the arm to be laid at an elevation of some thirty-three degrees.

So satisfactory, indeed, has this experimental structure turned out, that a further batch of sister guns have forthwith been commenced, and will serve to arm some of our heavy iron-clads which are now building. Only a small number of such weapons will be carried by these vessels—two, or at the most four, apiece—and thus our modern men-of-war will present a perfect contrast to those of a dozen years ago, when a ship, being regarded merely as a box of guns, sometimes received on board as many as a hundred and thirty cannon. Nevertheless, a broadside delivered from four guns of these giant dimensions (for the whole armament being carried in turrets may be brought to bear at one time), representing almost a ton and a half of metal, very far exceeds that which an old first-class three-decker could throw into her antagonist, and would indeed be sufficient to sink most vessels at a first discharge.

As regards the method of building up these large guns, we need say nothing, seeing that the subject was fully discussed recently in these columns. It may be of interest to know, however, that in the present instance as much as fifty tons of metal were employed in constructing the arm, and that at one time thirty tons of this was brought to a glowing white heat for the purpose of welding. The reverberatory furnace in which this massive coil was heated is an apartment in which a dozen persons could dine comfortably, and the length of the bars before coiling amounted to upwards of 1,200 feet. The length of the arm is sixteen feet and a quarter, and its extreme diameter fifty-six inches.

## *A NEW INEXTINGUISHABLE STORM AND DANGER SIGNAL LIGHT*

THIS new Signal, possessing most remarkable properties, has now been brought before the public. It was first exhibited at the President's meeting of the Royal Society on 22nd April, when it attracted great attention. The peculiarities of the Signal Light are, that it is self-igniting when placed in water or thrown on the sea. Contact with water being the only means of igniting the lamp, it is inextinguishable when once ignited; neither wind nor storm has any effect upon the flame. The light is of intense brilliancy, and of great duration, and can be seen for a great distance in the open-air. Photographs may be taken by the light of this new signal. Experiments were tried on the evening of 25th April, at ten o'clock, in the presence of some scientific gentlemen, to determine its brilliancy as a signal. A lamp was placed in a bucket of water on the top of Primrose Hill, and the light was so intense that after the signal had been burning for twenty minutes small newspaper-print could be distinctly read at a distance of seventy feet, notwithstanding that the night was thick and foggy. This new signal light will burn for over forty minutes. In construction the lamp is exceedingly simple, and so contrived that when once burnt the whole may be thrown away. The chemical preparation contained in the lamp is a solid, hard substance, free from danger; not affected by heat, and so non-explosive; and the signal is comparatively inexpensive. Its applications for marine signals are numerous. In case of shipwreck a few lamps thrown on the sea would illuminate the entire scene, and enable assistance to be promptly and efficiently rendered. For rocket-line apparatus it is equally valuable, as, bursting into a flame on falling into the sea, it would indicate the position of the rocket-line. In connection with life buoys it would be a mark to the drowning sailor. In life-boat services it would be a signal to the vessel in distress, and the brilliant light would greatly assist in the rescue. In cases of salvage, ships' signals, tide and harbour warnings, the duration of the light renders this new invention of great value. As a railway signal, to be used by the guards and station porters in cases of accident, it is equally available, and will be of great utility. The difficulties of preparing the chemical compound have been entirely overcome by Messrs. Albright and Wilson, of Oldbury, the contractors for the manufacture of the lamp for Mr. Nathaniel Holmes the patentee.

## *FRESHWATER BATHYBIUS*

AT a late meeting of the Natural History and Medical Society of the Lower Rhine, the well-known zoologist, Dr. R. Greeff, noticed an organism inhabiting freshwater and approaching very nearly, both in its structure and mode of occurrence, the celebrated deep-sea *Bathybius Haeckelii* of Professor Huxley.

Dr. Greeff, as much as three years ago, published a notice (in Max Schultze's "Archiv für mikrosk. Anat." Bd. iii., p. 396) of a new shell-less freshwater Rhizopod, which was remarkable for its gigantic stature in comparison with all previously-known organisms of that kind. He called attention at that time to its occasional occurrence in great quantity in the mud of standing waters, and indicated that, on account of its peculiar structure, it could be referred neither to the true *Amœbæ* nor to the *Actinophryes*. Since that time, the author has never lost sight of this extremely remarkable creature, and he thinks it desirable no longer to keep back his observations, especially considering the high degree of interest that has been excited by the *Bathybius*-mud which has been discovered in the depths and abysses of the ocean (to beyond 25,000 feet).

As regards the occurrence of this freshwater organism, to which the author provisionally gives the name of *Pelobius*,* and which he considers to be truly comparable with *Bathybius*, Dr. Greeff states that it is found in many standing waters with muddy bottom, which have apparently persisted for a long time, and seldom, if ever, have dried up. Thus, near Bonn the bottom of the Poppelsdorf fish-pond is found occasionally to be *almost entirely covered with masses of Pelobius;* to such an extent, indeed, that sometimes a glass vessel brought up from the bottom contains almost more *Pelobius* than true mud-particles, &c. The *Pelobius* never disappears in these waters, but remains throughout the year in great masses, sometimes in one place, sometimes in another. The cake-like lumps of mud which rise to the surface and float about there by the agency of enclosed gas and air-bubbles, especially during the warm season, also sometimes contain *Pelobius* in masses.

In their external form, in both the living and the contracted state, these organisms present the appearance of more or less spherical lumps, varying from one or two millim. in diameter down to the most minute points, scarcely perceptible by the naked eye. Middle-sized

* From πηλός, mud. [The name *Pelobius* has been long preoccupied.—ED.]

examples of about one millim. in diameter are the most abundant. They are generally so densely filled with mud-particles, Diatomaceæ, shells of *Difflugia* and *Arcella*, &c., that by transmitted light they can scarcely be distinguished from the actual mud without experience and careful examination; they may consequently be compared to a living mud. By direct light, on the other hand, they appear as grayish-white, yellowish, or brownish bodies. Their movements consist in an amœboid and often lively creeping by means of processes which are usually broad and lobate; during this process, the transparent body-substance often protrudes at the margins in elevations and undulations. This fundamental substance of the body consists of a *hyaline protoplasm* of irregularly *frothy or vesicular* consistency, containing, besides the above-mentioned ingested particles, a great number of very peculiar elementary particles. Among these there may be distinguished *round or roundish oval nucleiform bodies*, and *fine bacilliform structures*. Of the former by far the greater number consist of *shining pale bodies without any special structural characters*, but *of great firmness*, and presenting *considerable resistance to reagents* (acetic acid and caustic potash). These bodies may possibly be correlated with the coccoliths, &c., of *Bathybius*. Besides these, however, there are less numerous roundish nuclei of softer consistency, and with more or less finely granular contents, which, from their whole nature, must undoubtedly be regarded as equivalent to the ordinary cell-nuclei.

Hence in spite of its great simplicity in other respects, *Pelobius* represents a *pluricellular organism*, and is not to be referred to the so-called Monera, like *Bathybius Haeckelii*, according to the investigations of Huxley and Haeckel. Nevertheless, in connection with its possible relationship to *Bathybius*, it must be noticed that the cell-nuclei of *Pelobius* may occur in very variable quantity, often in so small a number as almost to disappear altogether; and further, that they can be detected only in the perfectly fresh state. This latter statement applies also to the frothy vesicular arrangement of the body-substance, which disappears immediately after death or the application of reagents.

The second kind of the chief elementary parts of *Pelobius* consists of *fine, clear, shining bacilli*, which are scattered through the whole body, and likewise present great resistance to the action of acetic acid and caustic potash. These were mentioned by Dr. Greeff in a former publication, when he expressed the opinion that they originate in certain nuclei, which, however, he has since seen reason to doubt.

The author has devoted much time and trouble to the investigation of the developmental history of this interesting organism, an exact knowledge of which would be in many respects of the greatest importance. He proposes to publish what has hitherto been observed upon this point (which in some respects recalls the Myxomycetes) in a detailed memoir upon *Pelobius* in Max Schultze's "Archiv für mikrosk. Anatomie," in which some other Rhizopods found under the same conditions as *Pelobius*, and resembling it, will also be described.

## *NOTES*

THE intelligence of the death of Sir John Herschel will fall on the whole scientific world with a sense of personal bereavement. Though he had attained above the ordinary span of life, his mind was still in the maturity of its powers; and few men have been so familiarly known by their writings and their discoveries beyond the narrow pale of the world of science. Next week we hope to give a biography of the great astronomer whose loss we deplore. It is fitting that Herschel II. should be buried in Westminster Abbey, and it is creditable to the authorities that his ashes will be permitted to rest there.

THE annual visitation of the Royal Observatory by the Board of Visitors is fixed to take place on the 3rd of June.

LETTERS have been received in this country from Dr. Adolf Bernhard Meyer, who left Europe last year for a journey through a part of the Malayan Archipelago and New Guinea. He reached Manado in Celebes in November last, just as the wet season commenced. He had chosen this place as his starting point, because he had been informed by a celebrated traveller in the East that the fine season commenced at Manado in the month of October. Nevertheless, he succeeded in making large collections of birds, reptiles, and fishes, which are on their way home.

THE Anniversary Meeting of the Geographical Society takes place on Monday next at 1 P.M., and that of the Linnean on Wednesday at 3. The Victoria Institute holds its annual meeting at eight o'clock on Monday, 22nd May, at the Society of Arts Rooms, John Street, Adelphi, when the Rev. W. J. Irons, D.D., will deliver the address.

A MANUAL OF ORGANIC CHEMISTRY, by Dr. Henry E. Armstrong, F.C.S., Professor of Chemistry in the London Institution, is advertised by Messrs. Longmans and Co., as being in preparation for their admirable series of Text-books of Science.

AT a session of Council on Saturday last, the Right Hon. Lord Belper, vice-president in the chair, Mr. E. J. Poynter, A.R.A., was appointed Slade Professor of Fine Art in the College. The buildings, forming part of the north wing, which have been designed for the Fine Art School, are nearly completed, and it is intended to open the classes for drawing, painting, and sculpture at the beginning of the College session in October next. The late Mr. Felix Slade has established at the college six scholarships for proficiency in those branches of Art, each of the value of 50*l*. per annum tenable for three years, and which may be held by ladies.

AN exhibition of Palæolithic Stone Implements will be on view at the rooms of the Society of Antiquaries, Somerset House, from May 19th to 25th inclusive, from 11 A.M. to 6 P.M.

AT the General Examination of Women at the University of London, just concluded, four passed in honours and nine in the first division. It is understood there were about double that number of candidates.

WE learn from the *Academy* that the Zoological Collection of the British Museum has been lately enriched by the purchase of a magnificent series of Sponges from South Africa, the majority of which are likely to prove new to science. It is to be hoped that the group of the *Spongiadæ*, now attracting so much attention in scientific circles, will receive a more liberal allotment of space in the new museum to be erected at Kensington. The utter unfitness of the present building to meet the daily increasing requirements of the national collection is evidenced from the fact that numerous groups of the *Invertebrata* are literally "crowded out," and entirely unrepresented in the series devoted to public exhibition, for want of the necessary space. This, and the inadequacy of the present slender staff of the Natural History department to effect the thorough and systematic arrangement of the extensive and valuable collection, and to elevate it to that high scientific status enjoyed in the leading continental museums, demand the most earnest and speedy attention.

MR. BOUCARD, the well known dealer in specimens of Natural History, and traveller, formerly living in Paris, but now resident in London, proposes the publication of a work on the Coleoptera of Mexico and Central America, including the adjacent portions of the United States, especially the Pacific region. He earnestly desires contributions of specimens, whether named or not, to be used in his investigations, and will return such as he is not permitted to keep, suitably identified, and will render an equivalent in other specimens, if desired, for such as are sent to him to be retained. Any specimens intended for him may be sent to his establishment, 55, Great Russell Street, Bloomsbury.

MR. C. H. BELFRAGE, of Waco, M'Lennan Co., Texas, announces that, at the request of several gentlemen in the United States and Europe, he intends making an extensive eight or nine months' entomological collecting tour in Western Texas and Southern New Mexico, if sufficient means can be raised, and invites every entomologist who wishes to enrich his collection to assist in the undertaking. Mr. Belfrage is recommended by Dr. A. S. Packard, jun., the editor of the *American Naturalist*, as a faithful and excellent collector, and the opportunity seems to be an unusual one of obtaining specimens of rare or little-known insects. Mr. Belfrage's address is at the above township, care of Messrs. Forsgard and Co.

THE Clifton College Scientific Society has just issued the first part of its "Transactions," which affords a happy illustration of its motto, *Viresque acquirit eundo*. Not yet two years old, and commencing with eighteen members, it has steadily increased in popularity and usefulness under the able presidency, first of Dr. Debus, and then of Mr. Barrington-Ward, till at one of its most recent meetings, nearly ten times that number of visitors and members were present. In this volume a number of interesting papers by the members, on various branches of natural and physical science, are printed; but we are most interested in the sketch of the constitution of the society. The School Museum has wisely been constituted especially a British one, and in order to facilitate the study of the natural history of the neighbourhood, and promote the other objects kept in view, the Society has been divided into sections of botany, geology, entomology, chemistry, physics, and archæology, the novel principle being introduced of limiting the number of members of each section to ten, in order to ensure a thoroughly working body. The Society has entered on its work in a spirit which entitles us to hope that it will be among the leaders in the spread of a real love of science among the generation now rising up.

MR. H. ROGERS read an important paper before the Edinburgh Botanical Society on March 9, a report "On the Effects of cutting down Forests on the Climate and Health of the Mauritius." The epidemic which broke out in 1865 in the colony, previously so remarkable for its salubrity, he traced to this cause, and stated that between 1854 and 1862 vast tracts of forests had disappeared, causing a diminished amount of rainfall, an increased amount of dryness, and a proportionate elevation of temperature. The difference in seasons is now much less marked, rains are scarce, droughts frequent and excessive, vast tracts of land, formerly productive, are now barren and desolate, and districts before noted for salubrity are now notoriously unhealthy. Although the amount of rainfall is much reduced, the violence of the rain is increased when it does fall, and heavy floods are the result. It was immediately after one of these inundations that the fever broke out in February 1865, which proved so terribly fatal in the colony.

FROM Ireland we have received the First Annual Report of the Natural History and Philosophical Society of Derry. Among papers of local history we find some on the antiquities, geology, entomology, and fucology of the neighbourhood of Derry, with drawings and photo-lithographs of cinerary urns found at Grange, Malin, and Buncrana, to illustrate a paper on that subject by the president, Mr. W. Harte. The society has made a good start, and we wish it all success.

THE Manchester Scientific Students' Association has issued its Tenth Annual Report for 1870. Although none of the papers read during the past year are printed in the report, the association, judging from the list of proceedings at the ordinary meetings, the Microscopical Club, and the Mechanical and Engineering Section, appears to have been doing some good and useful work. The number of members has slightly decreased during the year, but the committee hope soon to raise it again, and thus obtain funds for some desirable additions to the library.

IT is stated in *Land and Water* that the laudable effort of the Acclimatisation Society of Otago to introduce birds and animals into New Zealand has lately met with great success. The ships *City of Dunedin* and *Warrior Queen* have arrived in New Zealand with a living cargo of birds and animals which have thriven wonderfully well on board ship during their long voyage. Thus there are now in New Zealand five red-deer—we regret the stag died on the voyage—goldfinches, skylarks, blackbirds, sparrows, chaffinches, &c. The robins, numbering over a hundred, unfortunately died on the voyage. It is certain that the climate of New Zealand is admirably suited for the well-being and establishment of a British fauna. The colonists wisely recognised the importance of the study of "Practical Natural History," particularly as regards keeping insect life in check by means of small birds, the little feathered servants whose services are not sufficiently appreciated by agriculturists at home. Within the next fortnight thousands of young rooks will fall victims to the pea-rifle in England. How much better would it be if these unfortunate birds could have been sent across the ocean to our friends and relations in New Zealand! We understand that the cockchafer has been imported with English grasses, but the enemy to the cockchafer, the rook, has been left at home. We hope that next year the Otago Society will repeat their experiments with insect-eating birds, the unpaid "police of nature," which would keep in check the insect "pests of the farm," which have now pretty nearly their own way, to the injury of the farmer and horticulturists at the antipodes. The greatest credit is due to Mr. John A. Ewen for the pains he has taken in shipping the birds, and to Mr. Bills for the care and skill he has shown by his judicious management of the birds during the long voyage.

ON the 4th and 6th of March two shocks of earthquake were felt at Bogota, in Columbia, and these shocks were felt at the same dates at Cartago.

ON the 19th February the great earthquake in the Hawaian Islands took place. This was succeeded on the 2nd March by an earthquake at Eureka, in Humboldt County, California.

THE whole west coast of America, throughout its great mountain range, has now been seriously disturbed for some months. As far north as Washington Territory, Mount Rainer is reported as in commotion.

ON the 22nd February an earthquake was reported at Pano, in Peru, and a stronger one at two A.M. on the 23rd. These were slightly felt at Lima.

FROM recent advices we have to report the continuation of serious disturbances on land and by sea of the meteorological and physical conditions of the west coast of the Pacific. The phenomena appear to have been preceded long since, and are now accompanied, by volcanic disturbance, and some of them have passed from north to south. One remarkable feature is that of the inundations, particularly in the districts actually rainless. On the Isthmus of Central America rain is common, but this year the amount has been great, and the inundation greater. During the season immense quantities of vapour have been converted into rain along the western slopes of the Cordillera and the Andes. In northern Peru the effects have been particularly felt, and the more severely as the cities were unprovided to encounter rains or floods. Lambayegue, an interior town of 7,000 people, is destroyed, and the population have abandoned that of Supé. In some places bogs have been produced in which the cattle perish. Mud is washed far out to sea, and among the animals carried off was an alligator driven into Payta Bay. The circumstances are worthy of notice, as they illustrate some of the incidents of geological disturbance. At sea rain is met with a hundred miles out, to the surprise of captains, who report the winds and currents as changed.

It is stated in some of the papers that the system of storm-signal observations, now in progress under the direction of the Signal Corps of the army, was devised by Great Britain before it was made use of by the United States Government. This is perhaps correct, so far as it goes; but it is to Prof. Henry, Secretary of the Smithsonian Institution, that we owe the original idea of procuring despatches regularly in relation to the weather, and tabulating them, as also of placing them on a map so as to show, day by day, the general character of the weather throughout the United States. For several years prior to the beginning of the war this system was carried on regularly, and was of great interest to visitors to the Institution. The occupation of the telegraph lines for military purposes, and the fire in the Smithsonian building, broke up the arrangement; and it was about to be resumed when the Government undertook the work, thereby relieving the Institution from the necessity of its further prosecution.

The California vulture (*Cathartes Californianus*) is the largest species possessed by the fauna of Western America, where it ranges over an immense space of country in search of food. When any large game is brought down by the hunter these birds may be seen slowly sweeping towards it, intent upon their share of the prey. Nor in the absence of the hunter will his game be exempt from their ravenous appetite, though it be carefully hidden and covered with shrubs and heavy branches, as they will drag it forth from its concealment and speedily devour it. Any article of clothing, however, thrown over a carcase will shield it from the vulture. In some localities the nests are known to the Indians, who year by year take the young, and, having duly prepared them by long feeding, kill them at one of their great festivals. The California vulture joins to his rapacity an immense muscular power, as an instance of which it is stated that four of them jointly have been known to drag for over two hundred yards the body of a young grizzly bear weighing more than a hundred pounds.

Dr. Newberry, in his interesting report of the botany of the explorations for a railroad route from the Sacramento Valley to the Columbia River, speaks thus of the district lying east of the Sierra Nevada and the Cascade Mountains:—"The general aspect of the botany of this region is made up of three distinct elements. Of these the first is presented by the grassy plains which border the streams flowing down from the mountains. On these surfaces grows a considerable variety of animal vegetation, not unlike that of the Sacramento Valley in its general character. The second of these botanical phases is that of the 'sage' plains, surfaces upon which little or nothing else than clumps of artemisia will grow. The third is formed by forests of yellow pine (*Pinus ponderosa*), which apparently finds on these arid surfaces its most congenial habitat. It sometimes happened to us that, during a whole day's ride, we were passing through a continuous forest of these yellow pine trees in which scarcely a dozen distinct species of plants could be found."

The night heron of the United States (*Nyctiardea Gardeni*) is much dreaded by the Indians, who have many traditions and superstitions connected with it, and believe that it has the power of transforming human beings into inferior animals. Of the blue heron (*Ardea Herodias*), they say that he was formerly an Indian, and that perpetual quarrels raged between his wife and himself. On this account they were both transformed by a superior power, the man becoming a heron, the woman a dabchick (*Podiceps cornutus*), at the same time the brother of the woman was changed into the western grebe (*Podiceps occidentalis*), a native of the Pacific coast.

## REPORT ON THE DESERT OF TIH

(*Continued from page* 35)

The following are the various observations I have made and tales I have collected about some of the birds and mammals found in the desert of Tíh and adjoining regions. For convenience of reference I have arranged them alphabetically. In the cases of well-known animals, or of such as have been before scientifically described, I confine myself chiefly to the Arab stories or legends attaching to them:—

Bears (*Ursus syriacus*), Arabic *Dabb*, are still found on Mount Hermon and the Anti-Lebanon, and must formerly have existed in Palestine, but the destruction of the woods has now driven them northwards. They do much damage to the vineyards in the neighbourhood of Hermon, but seldom interfere with the herds of goats. The Arabs share in the widely-spread belief that bears sustain themselves during their hybernation by sucking their paws. They also say that when the female drops her cub it is quite shapeless, and that she carries it about in her mouth for fear lest it should be devoured by the ants, and then licks it into proper shape. Bear's grease is said to be useful in cases of leprosy.

Boar, wild, Ar. *Halhouf*, or usually in Palestine, *Khanzir*, which simply means pig. These animals are very abundant wherever there is cover near water, as on the banks of the Jordan and in the Ghor es Sáfi at the S. of the Dead Sea. I was much surprised to find traces of recent rooting by them in the W. Rákhamah, which lies between El Milh and 'Abdeh. This place is far from any water except what may have collected in hollow rocks, and can boast of no cover. The 'Azázimeh eat the wild boar, but the Ghawárineh, who will eat a hyæna, though it is known to frequent the grave-yards, will not touch them.

In this, as in the case of the other animals, I can insert but a few amongst the many medicinal uses to which they are put by the Arabs, as these are in general unsuited to the taste of European readers.

Bustard (*Otis hubara*) Ar. *Hubara*. I noticed a few of these birds in the Tíh; the Arabs say that the lesser bustard (*Otis tetrax*) which is also occasionally found there, is the young of the larger, but does not attain its full growth for two years. They also say that these birds, when attacked by a falcon, will cover it with their fæces, and so drive it off.

Camel, Ar. masc. *jemel*, fem. *nágah*. A stallion camel is called *fahl*. Collectively, *ibil* vulgo *bil* or *báir*, pl. *aarán*. *Hejjin* is usually applied to a dromedary, but is properly used of a man, horse, or camel having an Arab sire and foreign dam, which, in the case of the animals, is considered the best possible cross. Hence, a dromedary (or well-bred camel used for riding) is so called.

Camels are most peevish animals, docile only from stupidity; ill-tempered, they never forget an injury. I have but once seen a camel show the slightest sign of affection for its owner, although they are always well treated. All their feelings of like and dislike, pleasure and annoyance, are expressed by a hideous sound between a bellow and a roar, to which they give utterance whether they are being loaded or unloaded, whether they are being fed or urged over a difficult pass; in fact, they disapprove of whatever is done. Without them, however, it would be impossible to cross the deserts, for no other animal could endure the fatigue and want of water; I have myself seen a camel refuse water after having been without any for three days. For their food they always choose the most uninviting thorny shrubs; the seyal (acacia) which has thorns two or three inches long, is an especial favourite with them. Many of the Arabs subsist almost entirely upon the milk and cheese afforded by their herds of camels.

The Pelican is called *jemel el ma*, or water camel; and the Chameleon, *jemel el yehúd*, the Jew's camel.

Cat, or *Kutt*, also *Sinnaur* and *Hirr*. According to some lexicographers, the first name is not a pure Arabic word. Cats are held in great estimation in the east, and large prices are sometimes paid by native ladies for fine Persian specimens. In Cairo a sum of money was left in trust to feed poor cats, who daily receive their rations at the Mahkemah (law courts).

Though the Arabs in Sinai and the Tíh spoke of a wild cat, *gatt berri*, I found that this was always the lynx (*Felis caracal*), which is called in some parts of Arabia *'inak el ardh*, or earth-kid; in Sinai, it is also spoken of as *ânazeh* (from *ânz*, a she-goat). In Morocco, it is only known as *owdál*.

I may here remark that the word *Fahd*, translated by Lane and others as "lynx"—an animal that is never used for hunting—really means the *cheeta*, or hunting leopard of Persia and India.

The Arabs in the Tíh and in Morocco, as well as the Fellahin in Egypt, eat the lynx, and esteem it a delicacy, but, as some of them eat hyænas, jackals, foxes, vultures, and ravens, they can hardly be quoted as epicurean authorities.

Many animals have in Arabic a large number of names, more than 560, for instance, being applied to the lion. The following story current among them will illustrate this fact with reference to the cat. A Bedawi was out hunting one day, and caught a cat, but did not know what animal it could be. As he was carrying it along with him, he met a man, who said, "What are you going to do with that *Sinnaur?*" then another asked him, "What is that *Kutt* for?" A third called it *hirr*, and others styled it succesively *dhayûn*, *khaidà*, and *khaital*. So the Bedawi thought to himself, this must be a very valuable animal, and took it to the market, where he offered it for sale at 100 dirhems. At this the people laughed and said, "Knowest thou not, O Bedawi, that it would be dear at half a dirhem?" He was enraged at having his dream of wealth thus rudely dispelled, and flung it away, exclaiming, "May thy house be ruined, thou beast of many names, but little worth."

The Arabs say that the occasion of the cat's first appearance was as follows. The inhabitants of the ark were much troubled with mice: Noah, in his perplexity, stroked the lion's nose, and made him sneeze, whereupon a cat appeared and cleared off the mice.

In the East, as in Europe, a black cat is regarded as "uncanny," and various parts of it are used for magical and medicinal purposes; its claws, for instance, are said to be a charm against the nightmare.

Coney (*Hyrax Syriacus*) Ar. *'Waber* (lit. fur, from the thickness of their coats) *ghanem beni Israél*—sheep of the sons of Israel. Some Arabs say that this animal may be eaten, but others, as in Sinai, declare that it is unlawful, and call it Abu Salmàn, or else the brother of man, and say that it was originally a man who was metamorphosed for his sins, and they believe that any one who eats him will never see his house again. It is a common joke among the Hajjis and people of Mecca to say "A good digestion to you who have eaten Abu Salmàn."

Dog, Ar. *Kelb* (in Morocco *jero*, which properly signifies *puppy*, *whelp*), is the ordinary dog. A large kind of rough greyhound is called *Seluki*, from the town Seluk, in Yemen.* This dog much resembles the Scotch deerhound (cf. Gaelic name, *slogie*). In Syria and east of the Jordan there is a variety which is smooth, but has its ears, tail, and legs feathered like a setter; the females are said to be keener for hunting than the males, and black dogs are said to be the most patient. The dogs in Eastern towns live in communities, and have distinct bounds, usually ending at a street corner, and woe betide any dog who wanders beyond his own proper limits. I have often, when living at Cairo, amused myself by watching these animals. No sooner does a strange dog appear than all the rightful owners of the soil rush at him; the intruder takes to his heels, but the moment he has reached his own frontier, he turns round and snarls defiantly at his pursuers, and if they do not quickly retire his friends come to his assistance and drive them back in turn.

Dogs are said to have an intense hatred of hyænas, so much so that if a dog is smeared with the fat of a hyæna, he will go mad; and—which seems inconsequent—if a person carries a hyæna's tongue the dogs will not bark at him. This certainly would be most useful on entering an Arab encampment, for there a stranger is immediately surrounded by a pack of snarling brutes, who seem to sleep all day with one eye open, and at night to be continually awake and barking, either to frighten away some prowling jackal or lynx, or to repress some errant sheep or goat, who may wish to wander outside the circle of tents.

The Arabs believe that a dog can tell a dead person from one feigning death, and say that the Greeks (*Room*) never bury a person till they have exposed him to the dogs. It is, however, of only one breed that this is asserted, namely, the kind called *el Kalti*, and which is of small size, with very short legs. It is also called the Chinese dog. Of the origin of this story I am quite ignorant. The following is almost identical with a well-known Northern legend:—

A king had a favourite dog, whom he left at home one day while he went out hunting. Having ordered his cook to prepare a dish of *leben* (sour milk) for him on his return, the cook obeyed the order, but carelessly left the milk uncovered, and a snake came and drank of it and rendered it poisonous. On the king's return the dog tried to prevent him from touching it; at this moment the cook came in with some bread, which the king took and became to dip into the *leben*, when the dog immediately bit his hand. Upon this the king was very angry, and stretched out his hand again to the bowl; the dog, however, was before him, and began to lap the sop, whereupon it straightway fell down dead. The king then became aware of the sagacity and faithfulness of the beast, whose loss he mourned ever after, and erected a splendid tomb to his memory.

* The usual derivation, however, is "Seleucia."

Donkey, Ar. *Himár*. The donkey, much used by the Arabs, (for it will thrive in the desert where a horse could not exist) chiefly for carrying waterskins, as the Bedawin often encamp several miles from water, and the women bring up a supply every two or three days.* At Damascus there are three breeds of donkeys—(1) The white, which is most valuable, being sometimes worth 30*l.* or 40*l.*, and in Egypt I have heard of 60*l.* being given for a fine animal of this kind; (2) the ordinary donkey, which is used for riding, &c.; (3) A large donkey, standing from 13 to 14 hands, which is used for carrying burdens in the town; in the country, however, it is useless, as unlike the other breeds, it is far from sure-footed.

The Wild Donkey, Ar. *Air*, *fera*, or *himár wahshi*, is found to the east of Damascus; it is said to be very long-lived.

Dugong (*Halicore Hemprichii*), Ar. *otum* (called by Dr. Robinson *tûn*). This curious mammal is found in the Red Sea, and harpooned by the fishermen as it basks on the surface on the water. The skin is used by the Sinai Bedawin to make sandals of, for which purpose it is admirably adapted. In some parts of Arabia, it is said, that *khifaf*, or boots to protect the camels' feet from the rocks, are made of it. Some commentators take the Heb. *tachash*, which is translated "badger-skins," to mean the *otum*, and there is an Arabic word, *Tukkas*, applied to the dolphin species generally.

Fox, Ar. *Taáleb*, *Abou'l Husein*. In the East, as in Europe, this animal is looked upon as the type of cunning, and numberless stories are current concerning it. The following are examples:—

When a fox is over much troubled with fleas, he plucks out a mouthful of his hair, and then he takes to the water, holding the tuft in his mouth; all the fleas creep up on to this to escape drowning, and the fox then drops it into the stream and retires, freed from his enemies.

The celebrated Arabic author and theologian, Esh Shafiey, relates that when in Yemen, he and his fellow travellers prepared two fowls for dinner one day, but the hour of prayer coming on, they left them on the table and went to perform their devotions; meanwhile a fox came and stole one. After their prayers were finished, they saw the fox prowling about with their chicken in his mouth, so they pursued him and he dropped it; on coming up nearer to it, however, they found it only to be a piece of palm fibre, which the fox had dropped to attract our attention, and had, in the meantime, crept round and carried off the second chicken and left them dinnerless.

The fox is said to feign death, and to inflate his body, and when any animal, prompted by curiosity, comes to look at him, he springs up and seizes it.

The fable of the fox and stork is changed to the fox and raven; the former invites the latter to dinner, and gives him soup in a shallow wooden bowl; the raven returns the compliment, and pours out some wheat over a *silleh* bush. The *silleh* is one of the most thorny of the desert plants.

Another story told of the fox is, that one day he met five slaves, who were travelling with a large supply of food and other goods; he joined them, and after a time they reached a well, but had no rope wherewith to draw up the water. The fox suggested that they should throw down the meal and that one of their number should go down and knead it, which was accordingly done. After a while the fox said to the four who remained above, "Your comrade must have found a treasure, why don't you go down and share it?" This hint was enough, and they all hurried down, while the fox decamped with their goods and chattels.

A fox's gall is said to be a specific for epilepsy, and his fat for the gout.

Gazelle (*Gazella dorcas*), Ar. male *'ard.*, fem. *ghazaleh*, also (chiefly in poetry) *Dhabyeh* (cf. Tabitha, Acts ix. 36). This gazelle is found in the more open parts of the country between Sinai and the Lebanon; their haunts vary much with the different seasons. Though we never found any in the centre of the

* A tribe in the Desert, towards the Euphrates, is said to use donkeys only, and to possess neither horses nor camels.

Tíh, the Arabs said that, after a good rainy season, large numbers come there.

The Arabs speak of three kinds, viz. :—1. *El Rim* (*antelope addax*). 2. *El Edam* (*A. leucoryx*). 3. *El 'Afar*, which I cannot satisfactorily identify.

The tongue of an antelope must be an invaluable charm, for if it be dried and powdered, and then given to a woman who henpecks her husband, it will ensure her future good behaviour!

Goat, Ar. *ma'az* f. *ma'azeh* or *anz*. A he-goat (either wild or tame) is also called *tais*. In mountainous districts, large herds of goats are kept by the Arabs, chiefly for their milk and hair, which is used for making tents and sacking. The Arabs more usually eat a kid than a lamb on the occasion of a feast, and always a male. Full-grown animals are seldom killed. There are several varieties of goats from the upright eared kind to the Syrian goat with pendant ears, 12–14 inches long. That usually seen in the desert has ears slightly drooping and rather curling up at the top.

Horse: the generic term in Arabic, *Kheil*; a horse, *hisán* (in Morocco *'aud*); a mare, *fars*; a colt, *mohrah*.

*'Atík* is a thorough-bred Arab. Tradition says that the Devil will never enter a tent in which an *atík* is kept.

*Hejjin*: a crossed horse. (The term is explained under the head "Camel.")

*Berdhún* is a pack-horse with foreign sire and dam.

*Kadish* is a badly-bred *berdhún*.

The Bedawin reckon seven principal breeds of horses, which are as follows:—

1. *Musalsal*, which ought to be thin-crested, with short white stockings, red-eyed, short-coated, full in the barrel, and long-winded.
2. *Haikali*.
3. *Sharthar*.
4. *Hareifish*, a breed well known in Syria.
5. *Tubal*.
6. *Jij*.
7. *Kumeit*. These horses are usually bay, with black points, and ought, say the Arabs, to have a very fine muzzle; head thin, and well set on; upright, small ears; conspicuous white star on the forehead; round quarters, and to be well ribbed up; with a short or rat tail. They add, a well-bred horse is known by having the tail thick at the root, and carried well out.

The favourite colours are chestnut, gray, dun, black, and dark bay. The Prophet is related to have pronounced the following dicta:—"The best horses are black with white foreheads, and a white upper lip; next to these a black horse with a star, and three white stockings; next a bay with these marks." "Prosperity is with sorrel horses." The same authority judged *shikál*, *i.e.*, having the right-fore and left-hind feet white, to be the sign of a bad horse.

The first man who tamed and rode a horse is said to have been Ishmael. The first horse appeared when Adam sneezed on first awaking into life (cf. the story of the cat.)

Hyæna (*H. striata*) Ar. *Dhaba'*, also (in Sinai) *Arkudha*. This animal is found throughout the desert and Palestine. It is a cowardly beast, feeding chiefly on carrion, and is consequently little feared by the natives; as I have before mentioned, the Ghawárineh eat it. It is said to change its sex yearly; the same fable is told of hares.

Jackal, Ar. *Ibn 'Awi*, or in Syria *Wadwi*, in Morocco *Deeb* and *Taaleb Yusuf*. These animals are not found in the desert, but are common in the cultivated parts of Egypt and Palestine, where their weird cry is very frequently heard, beginning just after sunset. They are timid beasts, and do little damage, except in the vineyards, where they commit great ravages, being exceedingly fond of grapes.

Ibex (*Capra beden*), Ar. *Bedan* (from *bedn*, a body: probably so called as being the largest game in Sinai), the correct Arabic is *waal;* this is the name given to them north of Damascus. Some travellers have called them *Taytal*, but the word is not Arabic, and is only used by the Sinaitic Bedawin when speaking to Europeans, "poor simpletons," as they politely put it, "who don't understand Arabic." The derivation of this word I am quite unable to determine. Among themselves the Bedawin speak of the buck as *Bedan*, and the doe as *Anz* (she-goat), and the kids as *Dhalít*. A male in his first year is called *Fenaigill;* after this he is distinguished by the length of his horns; thus in his second year he is called *Abu Shibrain*, the father of two spans; in his third, *Thelathi;* in his fourth, *Rubai;* in his fifth, *Khammasi*; and they add that the horns never exceed five spans in length, which I believe to be true, for on measuring the largest pair that I have ever seen, I found them to be just 5 spans (about 41 inches) long. The term *garími* (red) is applied in a general way, much as we speak of red deer. These animals are found in Sinai and on both sides of the Dead Sea. I have reason to believe that those near Palmyra are a different variety.

Jerboa, Ar. *Yerbuah*, also *Dirs* or *Dars*, and sometimes *Zarumaih* (the lord of the little lance). There are several kinds of jerboas and desert rats; some of them are only found amongst the rock, others only burrow in the sand and gravel. Opinion is divided amongst the Arabs as to whether the jerboa is lawful for food or not; some eat it, but others reject it as being "a creeping thing." The Arabs say that they never drink, and believe that they live in communities, and appoint a sheikh, whom, however, they unhesitatingly kill should his rule not suit them. There is an Arabic proverb about a deceitful man: "He acts like a jerboa." This is said with reference to the ground outside a jerboa's hole, which, though seemingly solid, is really undermined, and gives way when trodden upon.

Leopard (*Felis leopardus*), Ar. *Nimr*, occasionally called in Sinai *Giblán** (corruption of the Turkish *Koplán*), the cubs are called *Weshek*. In the more secluded and inaccessible mountains of Sinai these animals are far from rare, and in a former visit to that country I was told that eleven camels had been killed by them during the preceding year in the district lying between Senned and W. Nasb. Like the hyrax the leopard is said to have been formerly a man changed into his present shape for performing his ablutions before prayer in milk, thus despising and diverting from their proper uses the good gifts of God.

Leopards are tolerably abundant on the shores of the Dead Sea; their tracks were here mistaken by M. de Saulcy for those of the lion, which animal is, however, quite extinct in Palestine and the Tih.

The Bedawin assert that young leopards are born with a snake round their necks, and that when a leopard is ill he cures himself by eating mice. Their fat is used medicinally, and their hair is burnt as a charm to drive away scorpions and centipedes.

Lizard. The larger lizards, especially the *Uromastix spinipes*, are called in Arabic *Dhabb*, and the smaller *Hardhun*. The Bedawin say that the former lays seventy eggs and even more, resembling pigeons' eggs, and that the young are at first quite blind. They are believed to be very long lived, indeed I have heard 700 years assigned as the term of their existence. By some tribes they are eaten, but are generally thought unclean. The Syrians curse them freely, for they say that they mock the devotions of the true believers. Certainly the way in which they jerk their bodies up and down is not unlike a caricature of the Muslim prostrations.

The dried bodies of some of the Skinks or Sand-lizards (Ar. Sakankúr) are much sought after as an aphrodisiac throughout the East. The particular kind in vogue is found in Nejed, and large quantities are brought by the Hajj caravans.

Owl, Ar. *Boomeh*. This bird is in some places regarded with veneration on account of a tradition which says that the souls of men appear on their tombs in the form of owls. I am told that they are sometimes used by fowlers as decoys.

Pigeon, Ar. *Hamám;* wild-pigeon, *Yemám*. In Egypt there are enormous numbers of pigeons who live in towers specially built for them. They are chiefly kept for their dung, which is very valuable as manure, and largely exported.

Most mosques are tenanted by pigeons, and not unfrequently a sum of money is left by some pious Moslem to buy corn for them. At Jerusalem they are especially numerous, whence the Arabic proverb, "Safer than the pigeons of the Harem." The mourning of doves is as frequently alluded to in Eastern as it is in Western poetry.

Quail, usually called in Arabia *Summana*, or *Selwa*. I only met with one specimen in the Tíh, and that was called by the natives *Firreh*. There is a tradition that the first instance of meat becoming corrupt and stinking was when the children of Israel stored up the flesh of the miraculous quails contrary to the commands of the Almighty.

Raven. There are three species of this bird scattered over the Desert, viz., *Corvus corax*, *C. umbrinus*, and *C. affinis;* all of these are called by the Arabs *Ghoráb*. They are generally found near a herd of camels, and may often be seen perched on the backs of these animals searching for ticks. Their chief food consists of reptiles and insects, but any dead or dying animal

* Giblán is the name of the chief of the Nimr (leopard) family of the Adwán Arabs in Moab.

will attract them. On one occasion I saw two ravens attack a horse which had fallen from exhaustion.

An Arabian proverb says, "Take a raven for your guide and he will lead you to a dead dog."

An Arab tradition evidently taken—as many others are—from the Old Testament, ascribes the first idea of burial to the raven. "While Adam was absent on a pilgrimage to Mecca, Cain and Abel each erected an altar for sacrifices. Cain, a husbandman, offered the refuse of his garden, but Abel chose the finest young ram of his flock and laid it upon the altar. His sacrifice was accepted, and the ram taken up to heaven, there to remain till it was required as a substitute for Ishmael when his father Abraham should offer him up on Mount Moriah. Cain seeing his offering refused, conceived so sudden a jealousy against his brother that he slew him, but being perplexed after the deed, and knowing not how to dispose of the body, he carried it about with him for many years. At last he saw two ravens engaged in deadly conflict, and one having killed the other scraped a hole in the ground and buried it, a hint which Cain took, and thus instituted the first burial rites as he had caused the first death. Adam returning mourned for his son and cursed the ground which had drunk up his blood, wherefore say the Muslims, the earth will never more absorb the blood of one who is slain, but it remains above ground, a lasting testimony to the murderer's guilt."

Sandgrouse (*Pterocles setarius*).—This species is most common in the Desert, but three other kinds are also found, viz. *P. exustus* and *P. senegalensis* (found by Tristram near the Dead Sea) and *P. arenarius*. All these are called *Kata*, or, in Bedawi dialect, *Gata* (in Morocco *Koudri*). The first and last mentioned species are called by some Bedawin *Koudriyeh* and *Sunifeh* respectively.

These birds require to drink morning and evening, and thus often prove of great service to the traveller by indicating the proximity of water. While staying at Damascus I was assured that these birds exist in such numbers in the territory of the 'Anazeh Bedawín that during the nesting season two men will go out with a camel's-hair bag between them and fill it with eggs in a very short space of time. The women then squeeze out the eggs and cook them, leaving the shells inside the bag. The Kata is said always to lay three eggs, neither more nor less. Its bones when properly prepared are said to be a cure for baldness, and the head may be used as a charm to extort secrets from a sleeping person. From its being so sure an indicator of the presence of water, the Arabs have the proverb "More truthful than the Gata."

Sheep. The proper Arabic name is *Dhán*; *Ghanem* is the general term for flocks of sheep and goats.

In the Tíh there are few sheep, but in Moab and Palestine they are numerous; these are generally the fat-tailed variety (*Ovis laticaudata*). A fine-woolled breed is found in some districts. I have always noticed that in the East sheep's milk is much better than that of either cows or goats.

Snake, Ar. *Haiyeh*, *Taabán 'Offi* (cf. ὄφις) *Dúdeh* (*lit. worm*) *Rakshah* (speckled one). Owing to its being winter when I passed through the Tíh. there were very few snakes to be found. The attitude taken by a horned snake (*Cerastes hasselquistii*) which I captured was remarkable. Immediately it saw me it began to hiss, and, tying itself as it were into a knot, created a curious grating sound by the friction of its scales. This snake is considered the most deadly of all by the Arabs, who hold it in great dread. They also affirm that if a snake has swallowed a bone which it cannot digest it will coil itself tightly round a tree or stone till the bone inside it is completely broken up.

Tortoise (*Testudo græca*), Ar. *Salahfát* (in Morocco *afkah*). The water-tortoise (*Emys caspica*) is called *Lejah*. The former is occasionally found in the Tíh. though common in Palestine. The latter abounds in the pools and streams of that country. Another species of land tortoise (*Testudo marginata*) is mentioned by Tristram, as being found on Mount Carmel. The water-tortoise is known to be carnivorous, and the Arabs declare that the land species also eat snakes, but this I believe to be quite false. Tortoises have a very strong odour, and I have frequently seen pointers in Morocco stand to them as they would to game.

Vulture, Egyptian (*Neophron percnopterus*), Ar. *Rakhamah* (Heb. *racham*) or *Onak* (in Morocco *Sew*). This is the only vulture at all frequently seen in the Desert. The Griffon (*Gyps fulvus*) and Lammer-geier (*Gypaetus barbatus*) seldom wander beyond the limits of cultivation. The Egyptian Vulture is commonly found near Arab encampments, where it shares the office of scavenger with the dogs. Many tribes, however, both in North Africa and the East, consider its flesh a delicacy.

Wolf (*Canis lupus*), Ar. *Deeb*. These animals are found in the mountains of Sinai and Palestine, but rarely in the Tíh. They do not pack like European wolves, but hunt by twos and threes.

The Bedawin say that "they sleep with one eye open," and have a similar proverb to our own, "A wolf in the stomach." Hunger is sometimes called *Da' ed deeb*, wolf's malady. Various parts of the animal are used for charms, *e.g.* a wolf's head in a pigeon cote, or a tail in a cattle stall, will keep off other wild beasts.

In addition to stories about real animals, the Bedawin have many fables of imaginary creatures, such as the Ginn, the Efreet, and the Ghoul. These hardly come within my province, and are well described by Lane ("Arabian Nights," vol. i.). I may however mention the *Nis-nás*, which is said to resemble a man bisected longitudinally, and to possess but one arm, one leg, and half a head. The story goes that it is found in Yemen, and that the people there hunt and eat it, notwithstanding that it can speak Arabic! The *Hud-hud* (so called from its cry) is a mysterious creature, not uncommon in Sinai. The Bedawin declare that it is never seen Though I often heard its plaintive cry close to my tent, and rushed out gun in hand, yet I never could obtain so much as a glimpse of it. At one moment the sound came from just over my head; the next instant it was far away up the hill side, and would either pass into the distance, or as suddenly return to me. From this I am convinced that the cry is made by some bird, probably of the owl tribe. The Arabs, of course, will accept no such materialistic solution of the mystery.

The Botany of the Tíh, especially in a season of drought such as we experienced, is very limited. The climate is so dry that mosses and even lichens are not found, except near Nakhl, where I gathered some much resembling the true Reindeer moss. This only grows on the northern side of the hillocks.

The passage in Job xxx. 4, "Who cut up mallows by the bushes," seems wrongly referred to the Sea Purslane (*Atriplex Halimus*.) In North Africa and the country east of Bir-Erba there is a small mallow which is eaten. This *invariably* grows either where an Arab encampment has stood or on the site of an ancient town. It has a small pinkish flower, and seldom exceeds seven or eight inches in height.

In the caves near Ain Muweileh a considerable quantity of salt crystallises on the surface of the limestone. Though disagreeable to the taste, it is eaten by the Arab.

At Petra the natives chip the interior of the caves. The fragments of sandstone are crushed and boiled, and a saltpetre sufficiently pure for the purpose of making gunpowder is thus obtained. The sulphur is found on the Lisan and coasts of the Dead Sea.

The above report necessarily contains but a sketch of our work. It will, however, I trust, give some idea of the country we had to examine, and of the difficulties which we encountered. In conclusion, I must here tender my best thanks to the University of Cambridge for having aided me in the investigation of this hitherto so little known but important district. It is the intention of Mr. Palmer and myself to publish together as soon as possible a full and systematic account of our explorations.

CHAS. F. TYRWHITT-DRAKE

(Note by Mr. C. R. Crotch on the Coleoptera brought from the Tíh.)

"In the small collection now before me are contained ninety species of Coleoptera, representing more or less all the larger families of the order, except the Water-beetles, an omission easily to be accounted for. The group most largely represented is, as throughout Syria, the Heteromera. These curious apterous, sluggish forms seem to thrive under the most arid conditions. The whole cast of the fauna is essentially Mediterranean; that one is on its southern side is shown by genera like Adesmia, Graphipterus, Pachyscelis, &c. The relations of this collection with an Egyptian one are very marked, many specimens being identical. None of them, however, extend to the Algerian deserts, though congeneric species occur there in their place. Nearly all are confined to the S. corner of Palestine and E. of Egypt, except the Dung-beetles (Histerida, Aphodiadæ, and Coprida), and these are more or less identical with those of S. Europe. The paucity of vegetation is very strongly indicated by the fact that the two great groups of Rhynchophera and Phytophaga number only seven species between them."

## *AMERICAN NOTES*

THE annual meeting of the National Academy of Sciences was held on the 18th of April last in the rooms of the Smithsonian Institution, in Washington, and continued in session four days. A number of interesting communications were presented. A report was presented by the treasurer of the Academy in regard to the Bache bequest, in which it was stated that its present value was about 41,000 dols., invested at 6 per cent., and bringing an income of about 2,400 dols. a year. It may be remembered by some of our readers that Prof. A. D. Bache, the late head of the Coast Survey, left his property in trust to the National Academy of Sciences, after the death of Mrs. Bache, for purposes connected with the advance of science, appointing as special trustees Prof. Agassiz, Prof. Peirce, and Prof Henry. The precise disposition of this fund has not yet been determined upon, the bequest having fallen too recently into the hands of the society to make it necessary to come at once to a conclusion.—The ship *Onward* arrived at New Bedford a few days ago, and her captain—Pulver—reports passing Sunday Island on the passage from Honolulu, and states that the volcano near by, referred to in a previous number of Scientific Intelligence, was at that time three miles long, and from 300ft. to 400ft. high. The sulphurous vapours extended around to a distance of three to four miles. He thinks that when the volcano becomes quiet there will be a good harbour between it and the main land, where before there has been only an open roadstead. The island is in latitude 29° south, and in longitude 178° west. The statement of Captain Pulver, according to the New Bedford *Standard*, is corroborated by other witnesses.—An examination has recently been made by an officer of the United States army of an old pueblo situated about twenty-five miles from the town of Socorro, on the Rio Grande. The walls of the buildings of this pueblo are composed of thin sandstone, heaped one layer upon another without mortar, and without any traces of beams or timber of any kind. The edifices seem to have been but one story high, and to have consisted of four separate buildings, arranged so as to form a hollow square with a fifth a little outside of these. The longest range was over 200ft. in length. and the whole five contained about two hundred rooms. Near the pueblo extensive silver mines have recently been discovered, and a town is to be laid out during the present year, the material for the houses to be derived from the ruins. There are evidences of ancient workings of these mines in the form of shafts now entirely filled up with earth, although it is probable that these do not antedate the period of the occupation of the country by the Spaniards.—According to late advices from South America, an unusually brilliant electric phenomenon was visible from Tacna, on the coast of Peru, early in March of the present year, around the snowy peak of Tacora, lasting for over two hours. The lightnings were of extraordinary shapes, and the thunders were of such intensity, and were heard over so wide an extent of country, as to completely terrify the population, unused to such exhibitions. This unwonted display was preceded by a slight shock of earthquake on the previous night.—According to the *Comercio*, of Lima, on the 12th of February, at Pitchican, an extraordinary meteor, of an oblong shape, and of a red colour, was seen to descend suddenly from the sky toward the earth; and, as soon as it touched, an explosion occurred, leaving a dense cloud over the place, and knocking down a fence for about five hundred yards. Among the stones heaped around by this meteoric body were found recently dead fishes of different species, which were supposed to have been lifted out of the river and dashed against the stones.—The cattle disease continues to spread throughout South America, all efforts to resist its progress having been unavailing. At the present time it is very prevalent in the Southern provinces of Chili and in the adjacent country.—The details of later advices from the Isthmus of Panama indicate the discovery of a rather low water-shed between the Atlantic and Pacific, on the Isthmus of Darien, although the feasibility of constructing a canal is, after all, by no means well-established. As far as the engineering possibilities are concerned, the chance seems to be much more favourable by way of Nicaragua, the result of a late investigation by Mr. Sonnenstern, on behalf of the Nicaraguan Government, serving to show that a route of 220 miles can be found connecting the two oceans, 196 of which is a ready constituted by the rivers and lakes of the country, leaving only twenty-four miles of land to be excavated, with a maximum elevation of not more than twenty-six feet. A slight drawback, however, to the value of this line is to be found in the fact, stated in the same paper, that the harbour of San Juan del Norte has been nearly filled up by a sand-bar, entirely preventing the entrance of vessels!

## *SCIENTIFIC SERIALS*

THE *Zeitschrift für Ethnologie* for the present quarter begins with a critical paper on "Ethnological Classifications," especially those which rest on language. The writer comments on the arbitrary character of the division of languages into "isolating," "agglutinating," and "inflecting," and contrasts the comparatively exhaustive knowledge of animal types on which zoological classifications depend, with the very scanty acquaintance which ethnologists possess of the great mass of languages beyond the Indo-European group. Exact knowledge of these latter highly complex and differentiated languages is, he argues, of very little use in tracing the origin and affinities of more primitive speech. It is suggested that peculiarities of language often depend on local characters of climate rather than on race. Thus, short words may be the result of a warm and lazy climate, like Siam, while, on the contrary, the chilly Indians of Athapascow take an athletic delight in calling their feet "choachastlsokai." Many interesting examples are given of Dog-Latin, Pigeon-English, Chinook-French, and other bastard varieties of civilised languages, which appear to be modified in a certain definite way according to climate and to race. Here is an example of Monks' Latin of the date 1127. "*Donent illis in Dominicis diebus carnem Mottotinum* (Mouton) *in quartis feriis cicerones, cum lardo.*" The second article in the same journal, by Franz Engel, is on the national types and races of Tropical America. It contains an interesting account of the habits and characters of the Spanish Americans, the Creoles, Negroes, and Indians, with the various cross-breeds among them. But there is little addition to our knowledge of their anatomical and physiological peculiarities, and the whole description is written in a diffuse and affected style, including in one passage a very prosaic travestie of verses from "Das Lied von der Glocke." A much shorter but valuable paper, by Adolf Hübner, gives an account, with figures, of a great series of drawings he discovered on a flat slate rock in the Trans-Vaal Republic of South Africa. Indigenous wild animals of all kinds were, to judge from the specimens given, very fairly represented, with a few human figures, one holding a bow; but no domestic animals were to be seen, nor was there any appearance of alphabetical or even picture-writing. The same writer gives also an account (with plans) of ancient Caffre fortifications in Mosalikatzi's kingdom.

IN the Berlin *Academy of Anthropology*, &c., a sketch by Dr. H. H. Hildebrand was exhibited (and is reproduced in the *Zeitschrift für Ethnologie*) of one of the urns with sculptured faces which have attracted so much attention in this Academy before. It was found in Cyprus. Other communications were on pile-dwellings in the Kuder See (Holstein), by L. Meyer; on an instrument of bone, about eight inches long, shaped like a knife-blade, and jagged along the edge, found in Mecklenburg. about fifteen feet deep, and covered by ten feet of chalk; on a burying-place in East Prussia, which proved by the utensils discovered in the graves to have been used by Romans; on stone implements in East Greenland; and on the ethnological characters of the Turcos of the French Army, for the study of which recent events have offered unusual facilities. At a subsequent meeting of the Academy, Prof. Virchow read a paper on the use of *tibiæ* and and other bones as skates in early times; and Herr Jagor one on the discovery of kitchen middens in the Andaman Isles.

IN a reprint from the *Archæologia* (vol. xlii.) Prof. Rolleston gives an interesting account of his researches in a Roman-British cemetery at Frilford, near Abingdon. Superficial to the more ancient interments, which were mostly in coffins and belonged to Christian times, were found later remains of the Saxon Pagan period. The latter were placed promiscuously, the former with more or less orientation, and the fact that the direction of the grave usually deviates towards the south is ingeniously explained as due to the majority of deaths having taken place then, as now, in the winter quarter of the year, when the sun would rise south of the due east. From the character of the skulls, and the urns, weapons, nails, &c., found in the graves, various important conclusions are drawn as to the social condition of this country during the obscure period between the departure of the Romans and the conversion of the Saxons; and the whole paper is illustrated by a curious and felicitous erudition which reminds the reader of the account of a Roman cemetery at Old Walsingham, given in the "Hydriotaphia" of Sir Thomas Browne."

## SOCIETIES AND ACADEMIES

### London

Royal Society, May 4.—"On the Molybdates and Vanadates of Lead, and on a new Mineral from Leadhills," by Prof. Dr. Albert Schrauf, of Vienna.

"On the Structures and Affinities of *Gwynia annulata*, Dunc., with Remarks upon the Persistence of Palæozoic Types of Madreporaria," by Prof. P. Martin Duncan.

The dredging expedition which searched the sea-floor in the track of the Gulf Stream of 1868, yielded, amongst other interesting Madreporaria, a form which has been described by Count Pourtales under the name of *Haplophyllia paradoxa*, and which was decided by him to belong to the section *Rugosa*.

The last expedition of the *Porcupine*, under the supervision of Dr. Carpenter and Mr. J. Gwyn Jeffreys, obtained, off the Adventure Bank in the Mediterranean, many specimens of a coral which has very remarkable structures and affinities. The species is described under the name of *Gwynia annulata*, Dunc. The necessity of including it amongst the Rugosa and in the same family, the *Cyathoxonidæ*, as *Haplophyllia paradoxa*, is shown. Having this proof of the persistence of the rugose type from the palæozoic seas to the present, the affinities of some so-called anomalous genera of midtertiary and secondary deposits are critically examined. The Australian tertiary genus *Conosmilia*, three of whose species have strong structural resemblance with the Rugosa, is determined to be allied to the *Stauridæ*, and especially to the Permian genus *Polycœlia*. The secondary and tertiary genera with hexameral, octomeral, or tetrameral and decameral septal arrangements are noticed, and the rugose characteristics of many lower Liassic and Rhetic species are examined. The impossibility of maintaining the distinctness of the palæozoic and neozoic coral faunas is asserted; and it is attempted to be proved that whilst some rugose types have persisted, hexameral types have originated from others, and have occasionally recurred to the original tetrameral or octomeral types; and that the species of corals with the confused and irregular septal members, so characteristic of the lowest neozoic strata, descended from those *Rugosa* which have an indefinite arrangement of the septa. The relation between the Australian tertiary and recent faunas, and those of the later palæozoic and early neozoic in Europe, is noticed, and also the long-continued biological alliances between the coral faunas of the two sides of the Atlantic Ocean.

"Remarks on the Determination of a Ship's Place at Sea." In a Letter to Prof. Stokes, by G. B. Airy, LL.D., Astronomer Royal.

May 11.—"An Experimental Inquiry into the Constitution of Blood, and the Nutrition of Muscular Tissue," by William Marcet, M.D., F.R.S. The results obtained from the inquiry which forms the subject of the paper are as follows:—

First. That blood is strictly a colloid fluid.

Second. That although blood be strictly a colloid, it contains invariably a small proportion of diffusible constituents amounting to nearly 7·3 grms. in 1,000 of blood, and 9·25 grms. in an equal volume of serum, these proportions diffusing out of blood in twenty-four hours.

Third. That the proportion of chlorine contained in blood has a remarkable degree of fixity, and may be considered as amounting to three parts (the correct mean being 3·06) in 1,000.

Fourth. That blood contains phosphoric anhydride and iron in a perfect colloid state, or quite undiffusible when submitted to dialysis, the relative proportions appearing to vary from 78·61 per cent. of peroxide of iron and 29·39 of phosphoric anhydride, to 76·2 and 23·8 respectively, the proportion of phosphoric anhydride having a tendency to be rather higher.

Fifth. That blood contains more phosphoric anhydride and potash, bulk for bulk, than serum.

Sixth. That a mixture of colloid phosphoric anhydride and potash can be prepared artificially by dialysis, and that the colloid mass thus obtained appears to retain the characters of the neutral tribasic phosphate from which it originates; it exhibits an alkaline reaction, yields a yellow precipitate with nitrate of silver, and after complete precipitation the reaction is acid.

Seventh. That by dialysing certain proportions of phosphate of sodium and chloride of potassium during a certain time, proportions of phosphoric anhydride, potash, chlorine, and soda are obtained in the colloid fluid very similar to the proportions these same substances bear to each other in serum after twenty-four hours dialysis.

Eighth. That muscular tissue is formed of three different classes of substances; the first including those substances which constitute the tissue proper, or the portion of flesh insoluble in the preparation of the aqueous extract, and consisting of albumen and phosphoric anhydride with varying proportions of potash and magnesia; the second class including the same substances as are found in the tissue proper, and in the same proportions relatively to the albumen present in that class, but existing in solution and in the colloid state; the third class including the same substances as are found in the two others, and moreover a small quantity of chlorine and soda, which, although relatively minute, is never absent. The constituents of this class are crystalloid, and consequently diffusible, the phosphoric anhydride and potash being present precisely in the proportion required to form a neutral tribasic phosphate, or a pyrophosphate, as the formula $2KO\ PO_5$ can equally be $2KO\ HO\ PO_5$.

Ninth. That flesh contains in store a supply of nourishment equal to about one-third more than its requirement for immediate use, this being apparently a provision of nature to allow of muscular exercise during prolonged fasting.

Tenth. That the numbers representing the excess of phosphoric anhydride and potash in blood over the proportion of these substances in an equal volume of serum in the regular normal nutrition of herbivorous animals, appear to bear to each other nearly the same relation as that which exists between the phosphoric anhydride and potash on their way out of muscular tissue.

Eleventh. That vegetables used as food for man and animals, such as flour, potato, and rice, transform phosphoric anhydride and potash from the crystalloid or diffusible into the colloid, or undiffusible state; and it is only after having been thus prepared that these substances appear to be fit to become normal constituents of blood, and contribute to the nutrition of flesh.

A final remark, and one which is worth consideration, is the fact established by the whole of the present investigation, that there is a constant change, as rotation in nature from crystalloids to colloids, and from colloids to crystalloids.

"On Protoplasmic Life." By F. Crace-Calvert, F.R.S.

A year since the publication of Dr. Tyndall's interesting paper on the abundance of germ life in the atmosphere, and the difficulty of destroying this life, as well as others paper published by eminent men of science, suggested the inquiry if the germs existing or produced in a liquid in a state of fermentation or of putrefaction could be conveyed to a liquid susceptible of entering into these states; and although at the present time the results of this inquiry are not sufficiently complete for publication, still I have observed some facts arising out of the subject of protoplasmic life which I wish now to lay before the Royal Society.

As a pure fluid free from life, and having no chemical reaction, was essential to carrying out the investigation, I directed my attention to the preparation of pure distilled water. Having always found life in distilled water prepared by the ordinary methods by keeping it a few days, after many trials I employed an apparatus which gave satisfactory results, and enabled me to obtain water which remained free from life for several months.

The water had to be redistilled three or four times before it was obtained free from germs, and it was then kept in the apparatus in which it was distilled until wanted, to prevent any contact with air.

Some water which had been distilled on the 20th of November, 1870, being still free from life on the 7th of December, was introduced by a siphon into twelve small tubes and then left exposed to the atmosphere for fifteen hours, when they were closed. Every eight days some of the tubes were opened, and their contents examined. On the fifteenth, therefore, the first examination was made; when no life was observed; on the 23rd two or three other tubes were examined, and again no life was detected; whilst in the series opened on the 2nd of January, 1871, that is to say, twenty-four days from the time the tubes were closed, two or three *black vibrios* were found in each field. Being impressed with the idea that this slow and limited development of protoplasmic life might be attributed to the small amount of li e existing in the atmosphere at this period of the year,* a second series of expements was commenced on the 4th of January. The distilled water in the flask being still free from life, a certain quantity of it was put into twelve small tubes, which were placed near putrid meat at a temperature of 21° to 26° C. for two hours, and then sealed. On the 10th of the same month the contents of some of the tubes were examined, when two or three small *black vibrios*

* During the intense cold of December and January last, I found it t ok an exposure to the atmosphere of two days at a temperature of 12° C. before life appeared in solution of white of egg in the pure distilled water, whilst as the weather got warmer the time required became less.

were observed under each field. This result shows that the fluid having been placed near a source of protoplasmic life, germs had become impregnated in two hours in sufficient quantity for life to become visible in six days instead of twenty-four. Other tubes of this series were opened on the 17th of January, when a slight increase of life was noticed; but no further development appeared to take place after this date, as some examined on the 10th of March did not contain more life than those of the 17th of January.

This very limited amount of life naturally suggested the idea that it might be due to the employment of perfectly pure water, so that Mr. Calvert commenced a third series of experiments.

On the 9th of February 100 fluid grains of albumen from a new-laid egg were introduced as quickly as possible and with the greatest care, into ten ounces of pure distilled water contained in the flask in which it had been condensed, and an atmosphere of hydrogen kept over it. On the 16th some of the fluid was taken out by means of a siphon and examined, and no life being present, twelve tubes were filled with the fluid exposed to the air for eight hours and closed. On the 21st the contents of some of the tubes were examined, when a few vibrios and microzyma were distinctly seen in each field. On the 27th other tubes were examined, and showed a marked increase in the amount of life. In this series, in which a *fermentable substance* was employed, life appeared in five days, and an increase in ten, instead of requiring twenty-four days, as was the case when pure water only was employed.

As the weather had become much warmer, and a marked increase of life in the atmosphere had taken place, some of the same albumen solution as had been employed in the above experiments was left exposed in similar tubes to its influence, when a large quantity of life was rapidly developed, and continued to increase. This result appears to show that the increase of life is not due to reproduction merely, but to the introduction of fresh germs; for, excepting this fresh supply, there appears to be no reason why life should increase more rapidly in the open than in the closed tubes.

"Action of Heat on Protoplasmic Life," by F. Crace Calvert, F.R.S.

Those investigators of germ-life who favour the theory of spontaneous generation, have assumed that a temperature of 212° Fahr., or the boiling-point of the fluid which they experimented upon, was sufficient to destroy all protoplasmic life, and that the life they subsequently observed in these fluids was developed from non-living matter.

I therefore made several series of experiments, in the hope that they might throw some light on the subject.

To carry out the experiments I prepared a series of small tubes made of very thick and well-annealed glass, each tube about four centimetres in length, and having a bore of five millimetres. The fluid to be operated upon was introduced into them, and left exposed to the atmosphere for sufficient length of time for germ-life to be largely developed. Each tube was then hermetically sealed and wrapped in wire gauze, to prevent any accident to the operator in case of the bursting of any of the tubes. They were then placed in an oil-bath, and gradually heated to the required temperature, at which they were maintained for half an hour.

*Sugar Solution.*—A solution of sugar was prepared by dissolving one part of sugar in ten parts of water. This solution was made with common water, and exposed all night to the atmosphere, so that life might impregnate it. The fluid was prepared on the 1st of November, 1870, introduced into tubes on the 2nd, and allowed to remain five days. On the 7th of November twelve tubes were kept without being heated, twelve were heated to 200° Fahr., twelve to 300°, and twelve to 400° Fahr.

The contents of the tubes were microscopically examined on the 1st of December, twenty-four days after heating.

In the sugar solution which was not heated there were about thirty animalcules under each field of the microscope, principally *small black vibrios*; two or three microzymes swimming slowly about; three or four *ordinary swimming vibrios*, and a few bacteria. In that which was heated for half an hour at 212 F., a great portion of the life had disappeared, and no animalcules were swimming. Four or five *small black vibrios* were observed moving energetically to and fro; two or three *ordinary vibrios* were also observed moving energetically *in the same position of the field, that is, without swimming about.* Heated for half an hour at 300° F. the sugar was slightly charred, but one or two *ordinary vibrios*, and one or two *small black vibrios* were observed in motion under the field of the microscope. In the solutions heated to the higher temperatures there was no trace of organisms.

*Remarks.*—The black vibrios here referred to are far more opaque than the other varieties of vibrios, and are the most important of all, as I have found them to resist not only very high temperatures, but all chemical solutions. I shall, in my paper on putrefaction and the action of antiseptics, describe the various vibrios, and give various drawings of them.

*Hay Infusion.*—An infusion of hay was made by macerating it in common water for one hour, then filtering the liquor, and leaving it exposed to the atmosphere all night, when it was sealed in the small tubes, twelve of which were used for each experiment. The infusion was made on the 4th of November, sealed in tubes on the 5th, and heated on the 7th.

The results were examined on the 1st of December, 1871, twenty-four days after being heated.

The hay infusion not heated contained "fungus matter" and other low organisms. The tubes, which were heated to 212° F. and 300° F., contained a few small "black vibrios," [but whether they were living or dead is not stated]. The tubes exposed to higher temperatures showed no trace of organisms.

*Gelatine Solution.*—A solution of gelatine, prepared of such strength that it remained liquid on cooling, was exposed for twenty-four hours to the atmosphere. It was then introduced into the small tubes, and the tubes sealed. The solution was made on the 4th of November, the tubes sealed on the 5th, and subjected to the different temperatures on the 7th.

The fluids were examined on the 1st of December, 1871, twenty-four days after being heated.

In the gelatine solution which was not heated, there were seven or eight animalcules under each field, five or six of which were quite different from anything observed in the other fluids. They had long thin bodies, swimming with a peristaltic motion. One or two ordinary swimming vibrios were also present; but the small black vibrios were absent. In the gelatine solution heated for half an hour at 100° F., the organisms ceased to exhibit any active movements; and in that which was heated for half an hour at 212° F., a very decided diminution in the quantity of life present was noticed. In the solutions heated to the higher temperatures no life was found.

*Putrid Meat Fluid.*—Water was placed in an open vessel, and a piece of meat suspended in it until it became putrid and contaminated with myriads of animalcules. This fluid was placed in the usual tubes, which were sealed on the 7th November, and heated on the same day.

The contents of the tubes were subjected to examination on the 1st of December, or twenty-four days after having been heated.

In the solution which was not heated, a large quantity of life was present, namely, microzyma and several distinct species of vibrios, among which were a number of the small black ones frequently mentioned. In that which was heated for half an hour at 100° F., this temperature had but slightly affected the life present, the animalcules being as numerous as in the liquid not heated, and not moving as usual. In that which was heated for half an hour at 212° F., although heat had *deprived the animalcules of the power of locomotion*, still they retained a sufficient amount of vital force to "place it beyond a doubt that life was not destroyed." In that which was heated for half an hour at 300° F. a large quantity of the life in the fluid was destroyed, but some vibrios still remained, the small black ones being the most numerous. In the solutions exposed to the higher temperatures there was no trace of organisms.

Although perfectly aware of the interesting researches of Prof. Melsens, proving that the most intense cold does not destroy the active power of vaccine lymph, still I thought it desirable to ascertain the effect of a temperature of 15° F. on well-developed germ-life, similar to that which had been subjected to the action of heat.

Some putrid-meat liquor, therefore, containing a large quantity of microzyma and vibrios, was subjected for twenty hours to the influence of a temperature ranging between the freezing-point of water and 17° below that point, when the ice was melted, and the liquor examined. The animalcules retained their vitality, but appeared very languid, and their power of locomotion was greatly decreased. Two hours after melting the ice the liquor was again examined, when the animalcules appeared to be as energetic as before.

In the discussion which followed the reading of Dr. Calvert's papers Dr. Charlton Bastian remarked upon the number of assumptions which were introduced, and gave reasons for his opinion that the experiments were wholly inconclusive in their nature. He was not aware that Prof. Tyndall had ever revealed an "abundance of germ life" in the air, whilst M. Pasteur had distinctly stated that he had been unable to recognise Bacteria or their germs in the dust filtered from the atmosphere. Even if Bacteria were widely diffused in the air, it would still have to be shown that they were alive. From the fact that some eminently inoculable fluids might be pretty freely exposed to the air for two or three weeks without showing the least signs of turbidity—though they could always be rendered turbid in two or three days after bringing them into contact with actual living Bacteria—he thought there was reason to believe that *living* Bacteria were by no means abundant in the air. And as he had found that all other naked lower forms of life with which he had experimented were unable to survive the effects of even short periods of desiccation, he thought there was much reason for the belief that the same rule would hold good for Bacteria. Dr. Bastian failed to find in Dr. Calvert's papers sufficient evidence that the organisms found in some of the solutions were really alive, and with regard to those experiments in which fermentable substances had been employed, it was assuming the very point at issue to suppose that the more numerous organisms which were present in them could only have come from the atmosphere. With regard to the influence of heat upon the life of Bacteria and many other organisms, Dr. Bastian gave some particulars concerning experiments, which tended to show, as he thought, conclusively, that they were all killed by an exposure in fluids, for ten minutes, to a heat of 60° C. (140° F.) There was no difficulty in ascertaining when Amœbæ or Ciliated Infusoria were killed, though with respect to Bacteria there was much more difficulty. Where the movements were not of an active character, after the Bacteria had been subjected to different degrees of heat, no reliable opinion as to their life or death could be arrived at. Bacteria which were really living might in many cases exhibit movements differing in no respect from those which dead Bacteria would display. From the exhibition of such movements, therefore, it could not be positively affirmed that the organisms were living, or that they were dead. The case was different, however, with regard to reproduction—dead organisms could not multiply. Having found a fluid, therefore, which was most suitable for the nourishment of Bacteria, but which seemed to be wholly incapable of giving origin to them *de novo*, he inoculated portions of it with living Bacteria, and then found that those fluids which had been heated to 50° C. or 55° C. for ten minutes became quite turbid in two or three days, whilst others, heated for the same time to 60°, 65°, 70°, 75° C. and upwards, invariably remained clear and showed no signs of turbidity. As living Bacteria will always multiply under suitable conditions in suitable fluids, their failure to multiply was the best evidence that they had been killed. The conclusion that Bacteria were killed by exposure in fluids to a heat of 60° C. was one which had been previously arrived at by Prof. Wyman and M. Pouchet, though such a conclusion was now much strengthened by Dr. Bastian's recent experiments. These results were harmonious also with the fact that Amœbæ, Ciliated Infusoria, and almost all the other lower organisms with which experiments had been made, were also killed by even a shorter exposure to a temperature of 60° C. (140° F.)

Geologists' Association, May 5.—Rev. T. Wiltshire, president, in the chair.—"On the Fauna of the Carboniferous Epoch," by Henry Woodward. In this paper the author protested against that mode of thought which seemed to imply that the globe was, during the various geologic periods, a vast aquarium, and urged the similarity of the conditions which now prevail with those that were obtained during the deposition of the various systems of the stratified rocks. Mr. Woodward combated the opinion of many, that during the Carboniferous period the atmosphere was heavily charged with carbonic acid gas, which, while it supplied the profuse vegetation of that epoch with carbon, prevented the radiation of heat from the earth, and thus produced an abnormal warmth which, with abundant moisture, was the cause of the vast growths that formed the beds of coal we now use for fuel. It was contended that the atmosphere under normal conditions was quite sufficient to supply all the carbonic acid that was required for the vegetation which composed all the beds in the world, and that, as we find the Gulf Stream exerting a great influence on the climate of England at the present time, so unusual warmth and humidity and great alterations of the isothermal lines of the globe, might have been produced by ocean currents consequent upon changes of coast-lines and other causes of which we can know little. The animal life of the epoch was then described, and some valuable lists of species were appended to the paper. The Rev. Mr. Henslow, referring to Mr. Woodward's remarks respecting the discovery by Prof. Morris of the "Mother coal" of Bradford being made up of spores and spore cases, stated that Prof. Huxley had concluded that coal generally was formed in this manner. Prof. Morris heartily approved of Mr. Woodward's opinion in favour of the contemporaneity of formations usually considered to be of successive epochs, and pointed out the great differences in the thickness in the underlying beds, and in other stratigraphical conditions of the Carboniferous limestone, millstone grit, and coal measures, in various districts. In Shropshire, for instance, the Carboniferous beds repose upon Silurian rocks, and in Scotland the coal seams are intercalated with the main limestone. Mr. Woodward, after passing a high eulogium on Prof. Morris, whose knowledge of the subject was most varied and extensive, briefly described several species of crustacea of which diagrams were exhibited, and directed the attention of the Association to a cast of the head of an undescribed species of *Anthracosaurus* allied to *A. Russelli*, recently obtained by Mr. George Maw, from the coal measures of Coalbrook Dale.

PARIS

Academy of Sciences, April 10.—M. Chasles read a very interesting paper on the properties belonging to a system of cones. Every one of these properties discovered by the law of analogy relates to a series of certain geometrical objects, compared with a series of other objects of the same nature. The demonstrations are not given except by the arrangement of the different propositions, which are sixty in number, and fill twelve closely printed pages of the *Comptes Rendus*.—Dr. Declat, who does not belong to the Academy, read a memoir on the effects of phenic acid. He attributes to this specific the power of curing the German cattle plague, or at least of preventing it. The experiments do not appear, however, very conclusive.—M. Aubert presented a memoir on the moral causes of the inferiority exhibited by French armies during the last campaigning. These causes are very numerous, and the principal of them is the making of the army an instrument for the protection of an internal despotism. The discussion of these subjects was considered as being out of the limits and province of the Academy. The memoir in former years would have been rejected under the old rules, but the president, M. Delannay, referred it to a special committee, composed of General Morin, director of the Conservatoire des Arts, and M. Amiral Paris. The *Comptes Rendus* for this sitting publishes a list of periodicals which were offered to the French Academy in the month of March. As many as fifteen publications were special periodicals, which resumed their publication during the few weeks of the cessation of fighting round Paris. The celebrated Abbé Moigno has lost no time in starting his *Les Mondes*, as the whole set for March was presented to the Academy. The foreign list was very short and incomplete, as NATURE, which had been presented, was omitted. The only English paper mentioned was the *Monthly Notices of the Royal Astronomical Society*.—M. Barral, the editor of two agricultural papers which are mentioned in the aforesaid list—*Bulletin hebdomadaire du Journal d'Agriculture* and *Journal de l'Agriculture*—having taken steps opposed to Communist rule, was obliged to leave Paris. His papers are now published at Versailles. M. Wolf, an Austrian subject, was present at the sitting. He is conducting observations at the National Observatory, where the instruments were not packed as on the occasion of the former shelling and investment. They run the risk of being smashed to pieces.

April 17.—M. Payen read an important paper on Cellulose. It is known that stony fruits or stony parts of fleshy fruits, like cherries and peaches, are composed of cellulose, impregnated with incrusting materials. The digestion of this cellulose is rendered more easy and complete by giving to the animals some fatty matters. The same may be said of stems of vegetables and straw. The application of this theory to the breeding of cattle is obvious. M. Payen exhibited some reactions, which show theoretically that the results obtained in Germany by the analysis of evacuations are truthful and genuine. The paper elicited some observations from M. Chevreul, who read afterwards on his own account a short report on a small pamphlet written by him during the investment of Paris. The title of this *brochure* is somewhat long, and explains clearly the meaning of the work: "On a fault of reasoning which is committed very often by people engaged in natural philosophy when reasoning on the concrete. The explanations are drawn from the last writings of M. Chevreul." These writings are mostly communications presented by M. Chevreul to the Academy for the last three years, when he was strenuously advocating a new classification of sciences as well as of the different objects of nature.—M. Trécul read some interesting observations on the Vegetation of Ferns. —M. Sant Venant, a member of the Section of Mechanics, presented a paper written by M. Boussinerg, a promising young French mathematician, who does not belong to the Academy. The paper related to the observations of an immense number of transcendental equations which present themselves to the mathematical inquirer when studying physical phenomena.

April 24.—The account of this sitting was published in the last number of NATURE. We have nothing of importance to add to it. All the *Comptes Rendus* of the period are signed by M. Elie de Beaumont, acting as perpetual secretary. M. Elie de Beaumont was formerly a senator, although it can hardly be said he has ever meddled with politics. M. Delaunay, the present director of the National Observatory, has inaugurated the regular publishing of a monthly abstract of meteorological observation. It is a practice which is revived from Arago's, but was stopped by M. Leverrier when he stepped into office in 1854.

### BERLIN

**Royal Prussian Academy of Sciences**, March 6.—M. Roth read the continuation of his historical remarks upon the theory of metamorphism, and the production of crystalline slates. —Prof. A. W. Hofmann read papers on phosphuretted hydrogen, and on the direct substitution of the alcoholic radicals for the hydrogen in that compound.

March 9.—M. Riess read a paper on the action of the subsidiary currents of the electrical battery upon the main current and upon each other.

March 30.—Prof. Hofmann described an eudiometer with movable wires. The apparatus consists of a glass tube, with two short narrow tubes attached to it at right-angles, and opening into it opposite to each other; these are closed by steel caps, through which steel screws pass, bearing the platinum wires. The screws are furnished with loops for the attachment of wires. —Prof. Hofmann also read a memoir on isodicyanic æthers, compounds which occupy a middle place between the cyanic and cyanuric æthers; and another on biuret and allied compounds. —Prof. Dove read a paper on the behaviour of agate in the magnetic field.

### VIENNA

**I. R. Geological Institution**, April 18.—Prof. Peters, of Gratz, sent a communication about a newly discovered mineral spring at Hengsberg, near Gratz. Besides a large quantity of free carbonic acid and carbonate of lime, the water contains chlorine, bromine, traces of iodine, boracic acid, and among the alcalies a considerable quantity of lithium.—M. J. Pauer related a remarkable phenomenon which has occurred during recent years in the large lake of Neusiedel, near Oldenburg, in Hungary. This lake, which measured nearly six German (about 150 English) square miles, was entirely dried up in the year 1865, and the ground was gradually converted into arable land. During the last winter, however, the water regained its territory, and to the great damage of the cultivators the basin is again filled up nearly to the same extent which it occupied formerly. Documents were found which prove that similar events took place in former centuries, and on one spot were discovered trunks of trees rooting in the ground as much as three feet in diameter. They prove that at a former period the lake was dry through a long series of years.—M. E. v. Mojrisovics presented a memoir on the so-called alveolaric Orthoceratites from the Triassic and Liassic deposits of the Alps. He proved that they all are the phragmocones of a particular genus of the family of the Belemnitidæ, and that their isolated rostra are the forms which Gümbel called Attractites. The genus was described many years ago by Fr. v. Hauer and named Aulacoceras, but he had united it with the family of the Orthoceratidæ. To Aulacoceras belong all formerly so-called Orthoceratites with a marginal sipho, from the mesozoic formations of the Alps, and only the species with a central sipho are real Orthoceratites.

## BOOKS RECEIVED

ENGLISH.—Discourses on Practical Physic: Dr. B. W. Richardson (Churchill).—The Coming Race (Blackwood and Sons).

FOREIGN.—(Through Williams and Norgate)—Vorwort zu der Physik der Erde: Dr. R. Reuschle.—Grundzüge der technischen Natur-lehre: Dr. Ph. Huber.—Leh-buch der Physik und Mechanik: Dr. L. Blum.—Lehrbuch der Physik: Dr W. Eisenlohr.—Anatomie und systematische Beschreibung der Alcyonarien: Dr A. Kölliker.

## DIARY

*THURSDAY*, MAY 18.

SOCIETY OF ANTIQUARIES, at 8.30.—Exhibition of Stone Implements (Palæolithic), with Papers by A. W. Franks, V.P., and J. Evans, F.R.S.
CHEMICAL SOCIETY, at 8.—On a New Double Salt of Thallium: R. J. Friswell.—On a New Benzolic Derivative: Dr. Armstrong.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall, F.R.S.

*FRIDAY*, MAY 19.

ROYAL INSTITUTION, at 9.—On Bishop Berkeley and the Metaphysics of Sensation: Prof. Huxley, F.R.S.
ROYAL UNITED SERVICE INSTITUTION, at 3.—The Winds of the North Atlantic: Captain H. Toynbee, F.R.A.S.

*SATURDAY*, MAY 20.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.
ROYAL INSTITUTION, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

*MONDAY*, MAY 22.

ROYAL GEOGRAPHICAL SOCIETY, at 1.—Anniversary Meeting.

*TUESDAY*, MAY 23.

ROYAL INSTITUTION, at 3.—On the Principle of Least Action in Nature: Rev. Prof. Haughton.

*WEDNESDAY*, MAY 24.

GEOLOGICAL SOCIETY, at 8.—Geological Observations on British Guiana: J. G. Lawkins, F.G.S.—On the Principal Features of the Stratigraphical Distribution of the British Fossil Lamellibranchiata: J. Logan Lobley, F.G.S.
LINNEAN SOCIETY, at 3.—Anniversary Meeting.

*THURSDAY*, MAY 25.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall, F.R.S.

## CONTENTS

THURSDAY, MAY 25, 1871

## *THE SMALLER LECTURESHIPS AT THE LONDON MEDICAL SCHOOLS*

### II.—THE TRUE FUNCTIONS OF THE SMALLER SCHOOLS

IN a recent article* we pointed out the prodigious waste of time and energy that results from the existence of no less than eleven medical schools in the metropolis, with from thirteen to twenty-one lectureships attached to each, and called attention to a scheme by which it is proposed that an amalgamation should take place between several of them.

It is maintained by those who have proposed this scheme that by its means a reduced number of central institutions would be created in which the preliminary subjects of medical education, such as natural philosophy, mechanics, rudimentary chemistry, and botany, could be taught in a much more satisfactory manner than at present, since the increased value of the lectureships would enable the lecturer to devote more time to their preparation, and to supply much greater wealth of illustration, whilst the larger number of students in attendance would correspondingly stimulate his zeal. At the same time the smaller hospitals and schools might still fulfil a very important *rôle* as supplying the means for the practical or clinical study of disease—certain lectureships still remaining attached to them.

The importance of good preliminary education in natural philosophy, taking it in its widest sense, for the medical practitioner, seems to us to be by no means sufficiently recognised. Up to a very recent time it has been almost entirely ignored. It is only within the last few years that any steps in the right direction have been taken by the great examining boards. Fifteen years ago the College of Surgeons required little more than a good knowledge of anatomy and the principles of surgery in those they admitted as members, and even now the acquaintance with preliminary subjects they demand is of a very rudimentary nature, as may be gathered from the fact that it includes only writing from dictation, arithmetic, algebra, geography, English history, the first two books of Euclid, and a little Latin translation, with one optional subject, which may be either Greek, French, German, mechanics, chemistry, botany, or zoology. We cannot but think that this programme might be advantageously extended.

Does it not stand to reason that the lad who is about to enter upon the study of Anatomy and Physiology ought to possess a competent knowledge of the principal facts of Natural Philosophy? How is he to comprehend the contraction of muscles, the action of the valves of the heart, the phenomena of respiration, the construction of the eye and ear, unless he is well grounded in Mechanics, already understands the lever and the pulley, and knows the principles of Hydraulics and Pneumatics, the laws of refraction of light, and the conduction of sound? Nor can it be said that such knowledge is of temporary value only. The surgeon and physician must daily and hourly see cases which can only be treated properly by reference to such knowledge.

If we might venture to suggest a scheme for the education of the student intended for the medical profession, we should recommend it to be commenced while he is still at school, at the age of sixteen, by passing the matriculation examination of the University of London. The following year should be devoted to the study of Natural Philosophy, rudimentary Chemistry, and Botany; and there could be no doubt that these subjects could be admirably taught at all the larger and better appointed schools through the country. But these are precisely the subjects that might be taught to large classes in a most superior manner in the four or five institutions with which it is proposed that the smaller schools should be amalgamated. The preliminary examination, in which considerable knowledge should be demanded, might take place at the age of seventeen. In the following winter session the student, now thoroughly grounded, should begin the study of Anatomy and Physiology, and the following summer might be employed in pursuing Chemistry in its application to Medicine; Botany in the same relations; and the recently introduced subject of Practical Physiology; the first embracing such subjects as the chemistry of the excretions and secretions, &c.; the second, the orders containing medicinal plants, and the composition and formation of the vegetable alkaloids, &c.; and the last, such points as the action of the valves of the heart, the processes of respiration and digestion, &c. The second winter session should be taken up in completing the knowledge of Anatomy and Physiology; and at the expiration of this session the first examination should take place, comprehending the subjects of Anatomy, Physiology, Chemistry, and Botany, the two latter points being at present almost wholly neglected. The second summer session might be occupied with the so-called Materia Medica, formerly merely consisting of the driest of all possible discourses on the composition and form of the various drugs, but with which our student is already perfectly familiar, and which might now be advantageously replaced by an account that could, in the hands of an efficient lecturer, be made deeply interesting, on the physiological action of drugs, and the effects of remedies on man and animals.

Up to the present time the whole work of the student could be conducted at one of the central institutions. From henceforward he might with great advantage be allowed to elect whether he would remain at this central institution, or go to one of the amalgamated schools. These might be made most serviceable as means of instruction in chemical medicine, surgery, and midwifery; and lectureships on these subjects, to avoid the loss of time to the students that would otherwise be involved in going to and fro, might be retained at the smaller schools. The instruction on these subjects would extend over the third and fourth winter sessions, at the end of which the final examination should take place. Thus it appears to us a vast improvement in the education of the medical student might be effected. He would enter the portals of his profession with a good general knowledge of the subjects he is about to study. The first years would be spent under circumstances in which he would obtain the best education on preliminary subjects the kingdom can afford, whilst the last two years would be spent under conditions in which the great field for clinical instruction possessed by the smaller hospitals could be

* See NATURE, vol. iv. p. 1.

utilised to the utmost. This is, indeed, the special field which we look to the smaller hospitals to occupy in the future. Clinical instruction is pursued to a far greater advantage with a smaller than with a larger number of pupils.

## M. TAINE ON INTELLIGENCE

*On Intelligence.* By H. Taine. Translated from the French by T. D. Haye, and revised by the Author. Part I. (London: Reeve and Co., 1871).

IN a notice, some months ago* in these columns, of M. Ribot's clever exposition of English psychology, mention was made of M. Taine's work, *De l'Intelligence*, then newly come forth, as a striking evidence of the revival of French interest in the scientific investigation of mind. The first part of the work is now put before English readers in a translation satisfactory on the whole, and the second part is announced as soon to follow.

The first part, as readers of the original must be aware, easily admits of being published separately. This happens because M. Taine's exposition, while presenting in the detail all the best qualities of his admirable style, is in its main lines laid out with a strict regard to principles of logical method. It falls into two sharply marked divisions, an analytic and a synthetic. No explanation of the different heads of knowledge making up our intelligence is attempted, until, by an analysis expressly performed, the ultimate elements of human cognition are come at. Often our English works on psychology, while they pass for, or claim to be, analytic, and do contain many cases of special analysis, are, in strictness, synthetic; the foregone general analysis being kept out of sight, and its sufficiency being left to appear from the character of the explanation which its results, as brought forward, may be made to yield. Of this description are the works of Prof. Bain, and even James Mill's professed "Analysis." M. Taine, on the other hand, prefers to do his analysis not in the secret laboratory of his own mind but under the eye of the reader; and the operation takes up the whole of his first part here translated.

Obviously, when the phenomena are so complex and manifold as in the case of mind or intelligence, the analysis, if it is thus to be exhibited, and if it is to be brought to anything like a definite issue, must be of facts carefully selected for their illustrative or representative character; and this M. Taine well apprehends. Nor does he less clearly see that normal facts or events of consciousness no more suffice for psychological science than can everyday observation take the place of artificial experiment in physical science. At different stages, therefore, he looks about him for cases either of what may be called artificial mental action, as in the ingenious processes resorted to by mathematicians, or of abnormal mental action, such as the phenomena of madness, hallucination, &c., which are a sort of nature's experiments on a field where, for moral reasons, the freedom of experimenting is greatly limited. So, at the stage of the senses where experiment becomes perfectly feasible, he effectively turns to account the most advanced results got out in late years by psychologists or physiologists; and, again, at the last stage of the analytic sounding, when he strikes upon a bottom of bare physiology, he makes apt selection from the most recent experimental work.

He begins by resolving thoughts, or (in the strict philosophical sense of the term) ideas, into images, on principles of thorough-going nominalism. Ideas the least general are shown to be impossible as mental experiences, and to need representation by particular signs, and ideas the most general and abstruse are shown to come within the mental grasp still by signs or symbols. There is the difference that in the case of natural objects, like tree or dog, the substituted sign, generally a name, is the direct expression of a mental "tendency" arising under actual impressions, varied at the same time that they are similar; while to conceptions like those of mathematical science there may correspond no distinct impressions, and the sign is struck out according to an elaborate system of indirect substitution—substitutions upon substitutions. But always some definite image is present to the mind. The question, then, is to investigate the nature of particular images; and, by a very instructive muster of normal and abnormal instances, the laws of their retention out of consciousness and revival in consciousness are brought out, with the result that the image is itself seen to be a substitute of sensation below it. Must the analysis then end in a mere description of the kinds of sensation, with account taken of physical conditions? M. Taine thinks it need not, and wisely selects for special inquiry the sensations of sound—wisely, not merely because Helmholtz's classical investigations lie ready to the psychologist's hand, but also because no other set of sensations is at once so varied in character and so free from admixture with extraneous elements. The result thence obtained, confirmed more or less from the senses of sight, smell, and taste, and not contradicted by the sense of touch, is that all qualitative differences of consciousness within each sense are explicable as different compounds of an elementary sensation not conscious; such elementary sensations, different in the different senses, being further conceivable as themselves developed by composition out of a single infinitesimal "event," of course imperceptible to consciousness, the truly ultimate element of all that appears as mind. But in relation with this there will stand a molecular displacement in nerve; for, as the physiological analysis, taken up when the psychological reaches its term, finds in the sensory ganglia the seat of crude sensation, and in the cerebral lobes with their cortical layer a "repeating and multiplying organ" through which sensations are associated and revived as images, and thus become knowledge, so it may see in the reflex action of lower nerve-centres the physical correlate of the simple unconscious "events" or elements of sensation. And thus the complete analysis of intelligence discloses two worlds, the moral and the physical, in mutual correspondence down to the lowest depths of human nature, and, by analogy, to the very foot of the zoological scale.

Save that M. Taine's method of procedure is his own, and his expression is always striking, there is little thus far in which he has not been anticipated by one psychologist or another among ourselves, notably by Mr. Spencer in the resolution of sensation. Nor in breaking up, in the last chapter of this part of his work, the metaphysical entities self and matter, regarded either as substances or as systems of faculties and forces, does he do more than

* See NATURE, vol. ii. p. 331.

follow the English authorities, as they follow their great master, Hume. But in giving final expression to the relation between the two series of "events," psychical and physical, shown by the analysis to be involved in the varied phenomena of intelligence, he is more strictly original; or, at least, his view is stated with peculiar neatness and force. How shall sensation, whether in the crude form or in the intellectual condition of the image or in the elemental state, be conceived as joined to something so disparate as a molecular movement in nerve: that is the difficulty. To say that what we have united is rather an idea of sensation had by (or, in the case of the ultimate psychical element, conforming to the type had by) direct personal consciousness, and an idea of nerve got indirectly by way of external sense, though this is philosophically true, touches the difficulty without removing it; for the two ideas are still irreducible to each other. But it may then more readily be suspected that the "events" are not two, but one, with two permanently distinct faces to cognition; and this, in M. Taine's view, is the final outcome of the analytic, though as regards the duality he hints at a possible reduction in his second part. Meanwhile, taking the physical aspect as secondary and the sign of a properly mental event, he seeks to illustrate the view and to enforce his theory of universal correspondence by a remarkable analogy. He supposes an original text with an interlinear translation; the translation plain and legible at the outset, but becoming confused farther on, and before the end no longer to be made out; on the other hand, the text very clear at the last but fainter higher up, and about the beginning not to be traced at all. The writing may represent nature; clear text and undecipherable translation mark the states of full intellectual consciousness so vaguely referred, at least in detail, to the complex of the brain; faint text and translation not too plain mark the cruder mental events referred to less but still highly complicated centres; finally, visible translation and blank instead of text mark the well-ascertained physiological phenomenon of reflex action, for which it is as legitimate, if not necessary, to suppose a psychical obverse, albeit unconscious, as it is to assume for highest consciousness a physical correspondence in brain-processes eluding our finest observation.

The analogy is instructively worked out further by M. Taine; but enough for the present. Another time we may better estimate the value of parts of his analysis, when considering how, from the materials it affords, he is able to build up the edifice of human knowledge.

G. Croom Robertson

## A STORM-ATLAS FOR NORWAY

*Storm Atlas of the Meteorological Institute of Norway.* (H. Mohn.)

A KNOWLEDGE of the laws which regulate the progress of storms would be of comparatively little practical interest without the telegraph, but, since the speed of electricity outstrips that of wind, the information by telegraph that a storm has appeared at an outpost may be of great importance to a maritime country like our own, provided we know the path which the storm is likely to pursue.

Of late years practical meteorologists have devoted a great deal of attention to this branch of their subject, and the memoir before us is not the least interesting of the various contributions which have been made. It is unnecessary to enter into the details of M. Mohn's observational system; let us rather invite attention to the general conclusions at which he arrives.

"Barometric maxima," he tells us, "often remain during a considerable period over the places where they have been formed, while, on the other hand, barometric minima are almost always in motion over the surface of the earth, transporting themselves (in Europe) almost always towards the east." He further finds that the barometric minima represented in his charts have their greatest velocity of motion before they arrive at the west coast of Europe, and a smaller velocity when they pass by Scandinavia; in Russia the velocity is again greater.

As regards the component of the movement which leads the centres towards the east, he finds a greater regularity exhibited, inasmuch as this component diminishes continuously as the minima move from the sea towards the interior of the continent. The curved paths of these minima are at north Europe sometimes very regular and sometimes very sinuous; in general they are concave towards the south. The mean movement in the direction of the meridian is towards the north in the Atlantic; towards the south, but feebly, in Scandinavia; and more strongly towards the south in Russia; in this latter country they appear to lose themselves.

Let us now invite attention to the following remarks of the author with regard to vapour:—"Vapour tension is an element of which the importance for the theory of tempests was not so evident to me until I had commenced the construction of these charts. . . . . . Charts giving the relative humidity are without any value, nor do they present any trace of that continuity which shows itself so strikingly in the charts of vapour tension." Further on he says:—"I have frequently remarked in this memoir that watery vapour is one of the most important elements in studying the movement of air; it is therefore much to be desired that the publication of meteorological observations should embrace vapour tensions (which is not always the case), and if only one element can be given, let it be rather that than the relative humidity. . . . . Charts representing the distribution of the vapour of water over the surface of the earth analogous to the temperature charts of M. Dove or the isobaric charts of Mr. Buchan would be of the greatest possible utility."

It may not be out of place to make a few remarks upon these observations of M. Mohn. Meteorologists have been in the habit of discussing in two ways the state of the air with regard to vapour. They have in the first place studied the vapour tension present in the atmosphere, and secondly, they have studied the relative humidity, or what M. Mohn calls "*l'état hygrométrique*." This latter element, representing the proportion between the vapour actually contained in the air and the full amount due to the present temperature, is an element that varies very greatly with the temperature, and is, therefore, of comparatively little use in meteorological researches.

Besides these two elements, the author of this review has suggested the *hygrometric quality* of the air as a subject of importance in meteorological discussions. This is a very different thing from what is termed by Mohn "*l'état hygrometrique*," and denotes the chemical composition of the air, as regards moisture, or the weight of vapour contained in a hundred parts by weight of air. Now, as long as the pressure remains the same, this hygrometric quality will be represented by the tension of vapour, and, since on the surface of the earth the variations of pressure are comparatively small, the vapour tension will approximately represent the hygrometric quality. If this be borne in mind, the physical significance of Mohn's conclusions will become apparent; his remark, that the charts of vapour tension present a continuity and simplicity of distribution of that element, will now mean that the distribution of *types* or *kinds* of air is of a very simple nature.

We should, in fact, endeavour to find the distribution of air of various qualities over the surface of the earth just as we should endeavour to trace on the surface of the ocean sections of different saltness. But, while on the earth's surface vapour tension will approximately represent hygrometric quality, the case will be altered if we study strata of different elevations, and therefore of different pressures. If for instance, we ascend a mountain or take a trip in a balloon, the tension of vapour will no longer approximately represent the hygrometric quality of the air; but it will be the ratio between the pressure of aqueous vapour and that of air, which will truly represent the hygrometric quality in those regions. These considerations may, perhaps, throw some light upon the formation of clouds. If, for instance, air of the same quality extends a great way up, we shall have no cloud formed in the stratum as long as the rate of decrement of temperature does not exceed a certain limit; but when this limit is exceeded, there will be a deposition of cloud through the lowering of temperature alone, even while there is no admixture with air of another quality.

B. STEWART

## *OUR BOOK SHELF*

*Ueber Entwickelung und Bau des Gehörlabyrinths, nach Untersuchungen an Saügethieren.* Von Dr. Arthur Boettcher, o. ö. Professor der allgemeinen Pathologie und pathologischen Anatomie, a. d. Universität Dorpat. Erster Theil mit zwölf Kupfer Tafeln. (Leipzig: Wilhelm Engelmann, 1871. London: Williams and Norgate.)

THE successful investigation of the structure of the internal ear must always be regarded as the crown and glory of histological research, for whilst the structures that compose the auditory organ are of extreme delicacy, they are enclosed in a bony capsule of such density as to appear to bid defiance to all attempts to exhibit them in their natural state. Yet by careful decalcification with dilute acids and by immersion in various fluids, as those of Müller Schultze's solution of chloride of palladium, Cohnheim and Gerlach's solution of chloride of gold, &c., the most delicate details have been followed out, and the structure of the ear is now almost as well known as that of the eye.

M. Boettcher's observations on the labyrinth of adult animals have been largely supplemented by his numerous examinations of the same part at various periods of fœtal life, which have led to some interesting results.

The very earliest rudiment of the labyrinth in the mammalian fœtus is not yet accurately ascertained, but in the embryo of a sheep, of which a sagittal section of the head is only a millimetre in length, it appears as a sac with a small external opening formed by an inflection of the horny layer in close contact with this and opposite the second visceral arch. The wall of the sac is formed by cylindrical cells. In a somewhat more advanced stage the vesicle becomes elongated into a tube, the upper extremity of which is divided by a fold into an internal and smaller cavity—the recessus labyrinthi, and a larger, broader cavity, the aquæductus vestibuli. The inferior extremity is pointed, and is in immediate relation with the rudiment of the cochlea.

The semi-circular canals are formed by an inflection of the wall of the labyrinth vesicle opposite to the recessus labyrinthi, the horizontal canal being the last formed. In embryoes of 2·2 cm. long, the several parts above mentioned are more fully formed, and a projection appears, the fundus of which is directed towards the brain, which is the rudiment of the sacculus rotundus, from which the utriculus or sacculus ellipticus soon becomes differentiated. The separation of a scala vestibuli from a scala tympani in the cochlea is only apparent in embryoes that have attained a length of 8·5 cm. M. Boettcher clearly shows that the recessus labyrinthi found at a very early period subsequently becomes the aquæductus vestibuli, which remains permanently in connection with the sacculus rotundus and utriculus, and contains the same fluid (endolymph) as they do. The aquæductus cochleæ, on the other hand, is a totally different formation, and is in no way connected with the interior of the labyrinth. It conducts a vein, and might more correctly be styled, as Wildberg has suggested, the canalis venosus cochleæ.

In regard to the cochlea, he shows how the canalis cochlea, or scala media, is first developed, and how the two principal scalæ (scala tympani and vestibuli) are formed by the gradual breaking down of spongy cellular tissue on either side of the scala media, and he traces out in the most interesting manner the development, chiefly from epithelium, of the complicated organ of Corti, including under this head the so-called habenula perforata, the rods, and arched fibres, &c.

He describes a remarkable ganglionic mass, the ganglion spirale, the section of which is seen in this section of the cochlea close to the attached border of the lamina spiralis. In this the cochlear nerve appears to terminate, whilst from it fresh fibres take origin, and then, having passed through the openings of the habenula perforata, join certain cells, some of which are placed outside and some inside the arcuate fibres or rows of Corti. The former kind of auditory cells are, some of them, seated with a broad base on the membrana basilaris, whilst the attenuated extremity of the cell runs upwards. Others, however, are intercalated with these, which have a broad base attached to the membrana reticularis above, and then narrow apices interposed between the broad bases of the former. The cells that point downwards are Corti's cells, and are arranged in three rows. They possess a centrically directed process. The cells that point upwards are the so-called hair-cells, which receive this name on account of their terminating at both ends in a hair. The membrana basilaris he describes as consisting of a hyaline lamella on which is a fibrous layer, both layers having a peculiar form of epithelial investment. Beneath its proximal attachment is a spiral vessel. The development of all these parts is carefully traced. A very full account is given of Corti's membrane. He denies the existence of muscular elements in Todd and Bowman's ligamentum spirale. The essay concludes with a description of the ultimate distribution of the auditory nerve. The drawings, which are upwards of sixty in number, and in some instances of large size, are very beautifully executed. On the whole, the work of Boettcher appears to be well worthy the attention of microscopists and physiologists, and to contain many facts possessing both novelty and interest.

H. P.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Thickness of the Earth's Crust

UPON my return to London yesterday, I received the two last numbers of NATURE (May 11 and 18), in both of which I find communications on this subject. In the first of these, by Archdeacon Pratt, that gentleman inserts a quotation from a lecture delivered by me, on January 29, this year, "On the Nature of the Earth's Interior" (*vide* NATURE, February 9, 1871), to the effect that the recent experimental researches of the eminent astronomer and mathematician, M. Delaunay, had destroyed the basis upon which the late Mr. Hopkins's reasonings, as to the solidity of the earth's interior, were founded, and asks the lecturer, *i.e.*, me, "I wonder why he has taken no notice of my letter in reply to M. Delaunay, which was printed in your journal for July 1870, six months before the lecture was delivered, and which also appeared about the same time in the *Philosophical Magazine* and the *Geological Magazine*. In this I showed that M. Delaunay had evidently misconceived the problem, and that Mr. Hopkins's method is altogether unaffected by his remarks."

As Archdeacon Pratt has the candour to admit that "any one with an ordinary degree of knowledge of popular astronomy and of mechanical action is quite competent to form a good opinion on the point in dispute," I would, in answer to the question he puts to me, simply state that, after a careful study of the letter he refers to, upon its first appearance in the *Philosophical Magazine*, I *purposely* avoided referring to it in my lecture, since I failed to discover that the author had in it "showed that M. Delaunay had evidently misconceived the problem," or any reasons whatsoever which could shake my faith in the conclusions of M. Delaunay, subsequently confirmed experimentally by M. Champagneur. I would also mention that, previous to this lecture, I attended the meeting of the Royal Society on the 22nd December, 1870, expressly to hear a subsequent paper by Archdeacon Pratt "On the Constitution of the Solid Crust of the Earth," on which occasion the opinions of Professor Stokes and the experimental demonstration of Mr. Siemens, as to the untenable nature of the author's conclusions, still further confirmed me in the views I put forth subsequently in my lecture.

It is now superfluous to specify in detail the precise reasons for my rejecting the arguments of Archdeacon Pratt, as I have, in a great measure at least, been anticipated in so doing by the substance of two letters, signed respectively "A. J. M." and "A. H. Green," which appeared in my absence in the last number of NATURE; to these I may refer in support of my view, in which I may also add one of our first English mathematicians has concurred; that M. Delaunay has not changed his will be seen from the Proceedings of the Academy of Sciences at Paris, March 6, 1871.

Having always entertained the highest opinion of the scientific labours of the late Mr. Hopkins, I have taken pains to make myself acquainted with his writings as far as possible for me; but when Archdeacon Pratt states "what Mr. Hopkins did may be divided into two parts—he first *conceived an idea*, which was to be the basis of his calculation; and then he made the calculation," I regard the whole pith of the question as embodied in these words, which admit that Mr. Hopkins based his elaborate calculations upon *an idea*, now shown by M. Delaunay to be incorrect, whilst the latter gentleman, on the contrary, founds his deductions upon premises which he first proves to have stood the test of experiment. Where eminent scientific men are arrayed on each side of a question of this nature, the remarks made in the last paragraph of the archdeacon's communication seem rather out of place, and might be applied with equal force in an entirely opposite sense to that intended by their author.

May 20 DAVID FORBES

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### The Geographical Distribution of Insects

IN NATURE (No. 74, p. 435) was a very interesting article on geographical distribution by Mr. Wallace, combating some recently-urged views of Mr. Murray's. Mr. Wallace took, as an example, the Madeira Islands, and sustained his position upon the numerical statistics furnished by Mr. Wollaston in his books. That these conclusions are very different from those arrived at by Mr. Wollaston is evident and as a six months' residence in the more remote group of the Canary Islands confirmed to my mind Mr. Wollaston's position, while bringing into relief facts utterly incompatible with Mr. Wallace's, I have ventured to publish a few remarks on the question.

Mr. Murray's views of the distribution of beetles seem to me resolvable into saying that there are two faunas, a tropical (Brazilian and Africo-Indian) and an extra-tropical one. My own slight researches in exotic coleoptera (confined hitherto to the Coccinellidæ) strongly confirm this; and a curious instance of the connection between the northern and southern extra-tropical faunas occurred to me the other day. *Eriopis connexa*, a rather pretty little ladybird, occurs from Hudson's Bay and Vancouver's Island all the way to the Straits of Magellan; following, of course, the line of the Andes. But my object was principally to question some of Mr. Wallace's conclusions with regard to the Madeiran fauna. First of all, I was struck by the absence of any hypothesis for the origin of the very curious endemic forms which form the most important part of the fauna, and which most closely unite it to that of the Canaries and Azores. These Mr. Wollaston, myself, and apparently Mr. Murray regard as affording proof that these islands, or rather groups of islands, were once parts of a considerable continent, and I certainly am at a loss to see how else they are to be explained; for though Mr. Wallace regards the Madeira islets as possibly formerly connected, he would, I suppose, be unwilling to extend this to the other groups. Mr. Wallace appears to regard Mr. Murray's hypothesis to be that the Atlantic continent, of which Madeira is a remnant, derived its fauna from Europe; but it seems rather to be that in the Miocene period (or earlier) there was a similar continent, connected indeed with Europe, not deriving its fauna from Europe any more than Europe from it. Perhaps the best way of answering Mr. Wallace's view will be to take the case of the Canary Islands, whose fauna, resembling the Madeira as it does so closely, must have had a similar origin. Here the argument from apterous genera fails to a very great extent. Thus *Carabus* is represented by three species, while in S. Spain there is one, and in N. Africa only six or seven. *Thorictus* has three representatives, and here it may be noticed that ants'-nest beetles are decidedly not numerous in the islands, so that the "unusual means of distribution" fail on the whole to get them across the water. *Rhizotrogus* is represented by the closely allied also N. African genus *Pachydema*. Of the very numerous European *Rhizotrogi* only two Sicilian ones are apterous, so that its absence in Madeira tells either way. *Otiorhynchus* is no doubt absent, but its place is more than supplied by *Atlantis* (20 sp.) and *Laparocerus* (30 sp.). *Pimelia* again is represented in the Canaries by twelve species, and the apterous genera of Heteromera by more than fifty species, which almost demonstrates the necessity of looking for Tenebrionidæ in localities where they are likely to occur.

*Tarphius* it certainly is difficult to conceive carried across by winds or waves, seeing that its habits are so retired that it has escaped notice till very recently in Europe. Now, however, it is beginning to turn up in suitable mountain localities of Andalusia, Portugal, the Apennines, Sicily, and Algeria; four species are described, and I have seen two others, all agreeing *inter se* and differing in structure from any Atlantic species. Moreover, it must have been carried apparently to the Azores as well. Then of the peculiar apterous genera quoted, *Thalassophilus*, *Torneuma*, *Scoliocerus*, *Xenomma*, and *Mecognathus* occur now also in Europe, requiring only a collecting-power equal to that of Mr. Wollaston for their discovery. There remain as puzzles upon the hurricane theory twenty-two blind species in the Madeira and the Canaries, and the whole series of Euphorbia-infesting species, fifty in number, all winged, and forming for the most part special genera. Finally, with regard to the fauna of the Azores, the condition of the islands must be taken into account; if the species found round Santa Cruz, Oratava, and Funchal were enumerated, about this proportion of European species would be found. The best island, Pico, has not been worked, and in the others almost all the original vegetation has disappeared. The fact that in the scraps (as they literally were) of Euphorbias, *Tarphius* and *Acalles* occurred, shows that if any of the pristine-flora could be found a fair number of species might be expected. *Elastrus dolosus* may certainly have come from Madagascar by the very ingenious route sketched out by Mr. Wallace; but the occurrence of *Urania* in Madagascar, Brazil, and the West Indies suggests a possibly shorter route, even though no *Elastrus* be known as yet to occur in America.

In conclusion I may state that I am going to spend a year or perhaps two in the West Indian Islands, and hope there still further to investigate the theories of geographical distribution, especially endeavouring to see if they can in any way be regarded as having been connected with this submerged continent of Atlantis. G. R. Crotch

### The Coronal Rifts

Will Captain Tupman kindly explain what he means by "*actinic rifts*"? I should have supposed that the rifts are evidence of the absense of actinism at the places where they occur.

I am not at all surprised that anyone observing through a telescope should fail to notice the rifts. The eye would naturally be attracted by the bright light of the corona and the red prominences. It may be observed, also, that there is in the photographs a considerable amount of bright corona at the places where the rifts occur, so that Captain Tupman might have had the telescope pointed at the very places where the rifts were, and yet they would escape his notice. The rifts are *there*, unquestionably, in the two photographs taken (after the lapse of nearly an hour) at Cadiz and Syracuse, and the sketches taken in Spain also show the gaps. The evidence appears to me to be conclusive against Captain Tupman.

A. Brothers

### Spectrum of the Aurora

There is one point in Mr. H. R. Procter's letter in Nature, vol. iii. p. 468, which I do not agree with He says the bands of the auroral spectrum are seldom visible, with the exception of that whose wave-length is 557; whereas I have found two bands, doubtless Winlock's 464 and 431, to be invariably visible when the aurora is bright enough to show them. Also, I suspect the red line is always present when there is any red colour in an aurora, although our instruments do not show it unless the luminosity is considerable. Of the thirty-four auroras in whose spectra I have seen the line 557, fourteen showed the bands 46 and 431, and three others at least one of these, while eight showed the red line. In five auroras, all more or less red, I have seen a faint band, whose wave-length, I believe, is 500 or 510. I have never seen the line 532 (the coronal line), unless it be once; probably from want of instrumental power.

As regards the zodiacal light, I have looked at its spectrum several times when it has been at its brightest, but have never seen anything but a continuous spectrum. I am satisfied therefore that if the line 557 exists in it, it must be much fainter in proportion to the rest of its light than is the case with the aurora. T. W. Backhouse

Sunderland, May 16

### Science for Farmers

As the independent and powerful advocate of scientific education, will you allow me to draw your attention to the object of the enclosed letter?

Within a short period I have seen such remarkable results attended with such an enormous saving of money arising from a limited knowledge of science amongst a committee of farmers, that I am desirous the future generation should have at least a common sense idea of some of the laws of nature which more immediately concern their business and pecuniary interests.

I have the more faith in the success of what I am advocating, because the kind and amount of scientific instruction I propose is really a business necessity. I have not forgotten the results of the Great Exhibition of 1851, how the members of each particular profession or trade were interested, especially in such stalls or departments that concerned this main object of their lives, how to make their own calling more successful or profitable.

I believe therefore in the teaching of science a much greater prospect of success exists when it can be combined with a practical business pursuit. I have read with much interest your article "The Hope of France" on the paper read by M. Deville before the Academy of Science.

The advantages arising from scientific culture, in other words, the study of nature and her laws, are beyond appreciation, and for this reason a student of science must reason and think for himself; he must do his own thinking, and not allow any other person to do this duty for him; and it is my conviction that the real power of any State is exactly in proportion to the number of independent reasoners and thinkers that go to constitute it; and I know of no means so powerful to promote this as the extension of technical teaching applied to business pursuits.

May 18 W. Little

"TECHNICAL EDUCATION FOR THE SONS OF FARMERS

"*To the Members of the Lincolnshire Farmers' Association, and other Agricultural Associations in Great Britain*

"Gentlemen,—Will you allow me to ask your earnest consideration and reflection on a subject which I believe is of vital importance to the future generation of farmers? The question I would put to you is this: Do you wish your sons whom you destine to the pursuit of agriculture to be entirely ignorant of such of the simple elements of chemistry as would give them a complete knowledge of the application and properties of the various materials used in the manufacture of artificial manures, when such knowledge may be acquired with little trouble, in a short time, and at small expense?

"I cannot for a moment believe that any intelligent farmer, with the costly and bitter experience of the past few years in relation to the tricks and impostures of the artifical manure makers, can be so indifferent to the future success of his child as not to give him, by a brief course of practical scientific education, not only the power of protecting himself against fraud, but also the knowledge that will enable him to apply the gifts of science to the greatest possible advantage, and at the same time liberate himself from the large and plausible army of manure compounders.

"Why should the business and pursuit of agriculture be an exception in the rules of guidance for the successful pursuit of any other business or profession? For the practice of medicine, law, engineering, architecture, &c., a special course of study is required, and is really necessary. Agriculture as a business pursuit offers abundant occupation for the highest order of intelligence, and stands second to none in its claim to scientific skill and sound practical sense, and has therefore an equal claim with other professions, that those engaged in it should be properly qualified by a special form of education.

"What can be more embarrassing to the present generation of farmers than the reading of the reports of chemical analysts on the composition of soils and manures? What can they understand by 'water of combination' other than that it may have too near a relation to a pump; or the term 'organic matter,' which may mean flesh or bread, woody fibre, peat, sawdust, or coal dust, most likely a large proportion of the latter elements; or that very intelligible term 'soluble phosphate equal to bone earth or tribasic phosphate of lime made soluble;' or alumina, silica, alkaline, salts, &c., which generally mean clay, sand, and common salt, concluding with earthy matter as the dirty foundation upon which all the other perplexities stand.

"Is it not worth while, by a brief course of practical study, to rid one's self of the influence of all this chemical necromancy? The days of alchemy, witchcraft, and astrology have passed away, and so must the charlatanism and quackery of the inferior order of manure compounders.

"What would be the history of many of these occult persons if it could be traced? Should we find that they have at any time been diligent students under such guides as Liebig, Ville, Voelcker, and other honourable and distinguished chemists? No! I do not hesitate to say that many of them have been mere wandering vagabonds having no disposition or ability to get an honest living by ordinary industry, and as a last resource trade on the credulity of farmers as artificial manure makers. A case in point was recently reported to me. Two discharged lackeys, a butler and footman, embarked, for want of honest employment, in this trade. They are now millionaires; one is an M.P. and the other has received the honour of knighthood. Recently I was over the works of a large and respectable manufacturer of phosphatic manures, who was also a maker of sulphate of ammonia. He informed me that he mixed these two ingredients in such proportion that he could well afford to sell it for 6*l.* per ton. The mixture went in immense quantity to Liverpool, where it was christened under the name of phospho-guano, and was actually returned, more than a hundred miles, near to the original works, and sold for 12*l.* per ton. Are not such cases, and a thousand others, sufficient' to make every farmer ask himself f one object of his being born into this world was to feed and fatten knaves?

"A first-class tailor, hatter, shoemaker, butcher, or baker, desires before all things that his customer should thoroughly understand the composition and quality of the goods he has for sale. Can the same be said of the manure compounders? The remedy we have in our own hands: either mix for ourselves, or buy subject to analysis; but to properly understand the several terms of an analysis, a course of practical instruction in a laboratory is necessary.

"It is indeed surprising that in a country where the practice of agriculture is carried to such a high degree of perfection, and where it is one of the chief sources of wealth, and is besides the means of employing such an immense amount of labour and capital, so little should be done towards the scientific education of those engaged in its pursuit. As a rule, agriculture is practised almost exclusively under the guidance of a slowly-earned experience, and mere traditional principles and habitual routine, without those engaged in it having any appreciation of the phenomena and natural laws which govern the growth and production of animal and vegetable food, the first necessaries of man.

"The success we have already arrived to in agriculture is, I believe, more mechanical than scientific. Drainage, steam culture, and a liberal use of capital and labour, are amongst the chief causes; but now that chemistry in its relation to artificial manures is taking such a prominent position, it is of the first importance that the future generation of farmers should have such a general knowledge of science as will enable them to correctly appreciate the value and properties of the various compounds offered by the too numerous chemical manure makers.

"I cannot imagine a more dangerous, unfortunate, and lamentable position for any person to be in, whilst in the practice of his business, by which he hopes to gain his bread for himself and family, than being entirely dependent on the scientific skill and integrity of another man; or that his capital, time, labour, care, and hopes should be in many cases completely out of his own control. Such a state of things cannot—must not—last.

"What I propose to do to correct this state of things is this—at a very moderate cost to give to an agricultural pupil a six or twelve months' course of scientific education and practical laboratory teaching after he has left his regular school; and I will engage that any boy of average ability, at the expiration of this time, shall have such a knowledge of all the materials employed in the compounding of chemical manures as will enable him to dictate what should be used without the interested interference of the manure maker. He shall besides have such an insight into the science and laws of chemistry as to make the reading and future study of scientific agriculture not only perfectly easy, but a delightful and intellectual employment.

"To carry out this important object three conditions are necessary—a good qualified teacher, a laboratory, and pupils. The first could be had for a very moderate salary; the laboratory, with the necessary instruments and materials, would involve no serious outlay; the third condition, the pupils, gives rise to this question—Would farmers send their sons to supplement their previous education by a six or twelve months' practical scientific instruction? The want of such knowledge amongst the present generation must be so strongly felt that I believe they would be too glad to have the opportunity, especially if they knew that probably a moderate fee would more than pay the costs.

"The site of the laboratory might be anywhere—a small country village would in many respects offer advantages superior to a market town. The cost of erecting or hiring a building suitable for a laboratory, together with the instruments and materials, should be raised by subscription. The pupils' fees would, I have no doubt, pay all other charges, so that when once established it would ever after be self-supporting.

"I am aware that certain schools exist where agricultural science is professedly taught, but I consider such a combination as almost waste time, the two kinds of teaching cannot be well carried on together, and what is most important, the mind must arrive to a certain maturity before it can grasp with sufficient reasoning power the beautiful and wonderful phenomena arising out of laboratory practice.

"If the foregoing observations should be thought worthy of the serious consideration of persons interested in agriculture, I shall indeed be glad to receive any communications or suggestions in promotion of the object of this letter; and if our members will support this object by the subscription of a sum equal to only a tenth of one year's savings effected through the agency of our association, I will devote myself most earnestly to establish a school laboratory in this village that I trust may serve as a model and example to be followed by many other localities in Great Britain.

"I shall certainly not ask a single member of our Association to do that which I am not prepared to do myself. My consumption of phosphate manure—of course, I buy no mixtures or nostrums—is about twenty tons a year; and previous to the formation of our Association I paid to the most respectable makers 6*l*. 10*s*. per ton for manure, containing 25 per cent. of water. The percentage of soluble phosphate was entirely a matter of speculation. According to an elaborate report of Anderson and Way, from an analysis of 171 samples, the average per-centage of soluble phosphate at that time was 15 per cent., and a ton of this manure was valued by these chemists at 7*l*. 5*s*. per ton, Therefore, if a 15 per cent. manure was worth 7*l*. 5*s*. a 26 per cent. manure would be worth 12*l*. 10*s*.; but our Association price for the 26 per cent. manure is now 3*l*. 18*s*. per ton in bulk, delivered free at any station in Lincolnshire, with a further advantage of a watchful system of analysis, free of any cost, to ensure quality and dry condition.

My saving on these calculations would be 8*l*. 12*s*. per ton, or a total of 172*l*. on a consumption of twenty tons yearly. I state facts just as they are recorded by the most eminent chemists, and every farmer will believe me when I say that a very large proportion of the manure sold at that time had little or no value whatever, consisting as it did of *dried mud and road scrapings*, flavoured with a little gas water just to flatter the olfactory nerves of the wise and cautious farmer of that period. A tenth of 172*l*. would be 17*l*. 4*s*. for my contribution, but my requirements are much more modest. I think, therefore, I had better leave every member of our Association to form his own estimate of what he should contribute, suffice it to say that in the first instance the only contribution I ask for is a free and unprejudiced opinion as to the necessity and desirability of what I have in view, viz., the formation of a laboratory, in which agricultural chemistry shall be taught at small cost, in a short time, and in a practical way, to pupils who have received an ordinary education.

"I have only alluded thus far to the material advantage to be derived from a brief course of scientific instruction. Allow me, in conclusion, to quote the language of one of the best and most highly gifted of our chemical philosophers as to its moral influence. The late Dr. Faraday says:—'I do think that the study of natural science is so glorious a school that with the laws impressed on all created things by the Creator, and the wonderful unity and stability of matter, there cannot be a better school for the mind.' Vain and foolish ideas, the fruit of ignorance, cannot be uprooted and destroyed by violence, the natural and more gentle method must be adopted, what in chemistry is called the law of substitution; the mind must be fertilised by knowledge, then truth and useful ideas will take the place of error and ignorant conceits. It is the absence of the exercise of the higher and intellectual faculties that leads so often to vacuity of the human mind, and the consequent indulgence in grosser and more material excitements, injurious alike to body and mind. A better form of education would eradicate the greatest of all human enemies—Ignorance and Intemperance.

"W. LITTLE

"The Hall, Heckington, Lincolnshire"

### Degrees for Engineering Students at the University of London

IN one of your Notes for May 11 you refer to the failure of a motion which I brought before Convocation of the University of London at its recent meeting, to the effect that it was desirable that Greek should cease to be a compulsory subject at the matriculation examination. I have no wish to trouble your readers with a discussion of this subject, because it has been already so well ventilated in various quarters, and general opinion with regard to it has so nearly crystallised into form in other convocations than that of the University of London, that I have no doubt that the Senate of the University will ere long see the absolute necessity, if the University is to be kept *en rapport* with the scientific culture of the country as it has been hitherto, of adopting the course which I have urged, and the expediency of which has been endorsed by some of the highest educational authorities. The rejection of my motion is not the first illustration that Convocation has given of the highly conservative tendencies of many of its members, and of their incapacity to appreciate the liberal spirit in which the University was founded; and I am quite content to leave the case as it stands, with the remark made by one of old under similar circumstances—

Victrix causa Diis placuit, sed victa Catoni.

I shall, however, be glad if you can favour me with a little space in your columns to air another subject which I also brought before Convocation, with, I am sorry to say, equal want of success, and that is the desirability of modifying the examinations for the degree of Bachelor of Science by omitting the biological subjects, so as to induce engineering students to take it. At present the biological subjects required for the degree, viz., Zoology, Botany, Physiology, and Organic Chemistry, are so entirely foreign to the studies and requirements of such students that in most cases it is scarcely practicable, even if it were desirable, for them to travel so far out of their regular line of work, for the purpose of getting them up for the Bachelor of Science examination. Such a course would be precisely analogous to that which is now prescribed for medical students proceeding to their M.B. degree, who are required to take up those subjects of the B.Sc. examination which are cognate to their routine of study, and who then branch off to those of a purely professional character.

Only two objections were urged in Convocation against this scheme which are worth consideration. The first was that it would tend to lower the standard of the degree by diminishing the comprehensiveness of the examination. In order to meet this objection I suggested that candidates not wishing to take up the biological subjects should be required to substitute for them others of a mechanical nature, such as Applied Mechanics, Engineering and Architectural Construction, and Geometric Drawing, which, as all who have had any experience in teaching them know, are quite as capable of being made efficient educational tests as those which they would replace. The second objection was, that to make such a change would be equivalent to instituting a degree in engineering. That this would be the practical result of the suggested alteration I am prepared to admit, and it is the object which I had distinctly in view in proposing it. What there was in the suggestion to provoke the unconcealed opposition of so many of the members of Convocation, I am a loss to imagine, unless it was the illusion that the profession of engineering is a less scientific one, and the education of its members less worthy of being encouraged, than that of the professions of law and medicine, to which so large a proportion of the London graduates belong.

For my own part, it seems to me a scandal of no mean gravity that, whilst the practice of that profession requires intellectual qualifications of the highest order, and a scientific training of the widest kind, no means should exist in this country whereby either the public should be provided with any guarantee that those who practise it possess either of these qualifications, or its practitioners themselves should be enabled to give evidence of the fact of their own accord. I do not know of any department of education in which the University of London could, at the present time, do more service than in this, and, I trust, there are men in its Senate, who, with more breadth of appreciation than the majority of Convocation, will give the matter their earnest attention. FRANCIS T. BOND

Hartley Institution, Southampton

### Mechanical Equivalent of Heat

I AM afraid your publication, without adding the date, of my letter last week (which I only saw this morning) puts me in a false position in regard to Dr. Joule, inasmuch as it appears to ignore a correspondence of mine with him, which took place between the time that letter was written (now a long time since) and the time of your publishing it.

In that correspondence I allowed that Dr. Joule's theory remained the same in its main features, though I thought he virtually retracted one statement which I had particularly argued against. Dr. Joule, however, did not allow he had made any alteration.

He also informed me that a paper of mine had been read at the meeting of the Manchester Literary and Philosophical Society, in which I showed (as I believe) in a detailed examination that his theory was inconsistent with the results, both of his own and of M. Favre's experiments. Dr. Joule also kindly communicated to me the substance of the reply which he had made, but I have not seen either in print. Of course the question is one of facts; are facts consistent with the new laws of thermodynamics as supposed to have been established during the last twenty years? Tait, in his preface to his Thermodynamics, says: "The subject s one of vast importance, but very few indeed are yet acquainted with even its most elementary facts; and by many of these it is not yet accepted as true." These laws, therefore, can scarcely yet be put on a level with Newton's laws, even if they should be shown to be consistent with facts, which, at least in their present form, I believe to be impossible. H. HIGHTON

May 18

MR. HIGHTON'S letter in NATURE is almost identical with his communication to the *Chemical News*. My answer is similar to that which I sent to the latter publication, viz., that the object of my paper in the Proceedings of the Literary and Philosophical Society was simply to place the theory of the electro-magnetic engine in a form which might prove useful to those who had not worked on the subject, and not in any respect to withdraw the reasonings in what Mr. Highton is good enough to term my "famous paper." Mr. Highton handsomely acknowledged the justice of my note to the *Chemical News* in a letter addressed to me on the 28th ult. JAMES P. JOULE

### Optical Phenomenon

IN reading over Prof. Clerk Maxwell's paper on Colour in NATURE (Vol. iv. p. 13), I was reminded of the following, to me, curious phenomenon which was seen by me on several occasions in the summer and autumn of 1859.

Whilst standing before a black board, making geometrical figures in white chalk, I was struck by one side of each chalk line appearing blue, the remaining half retaining its proper white. The cause was at once evident to me, for I found that the sun shone fully upon one eye, but not upon the other. By closing the eye upon which the sun shone, the chalk marks appeared wholly white. Opening the eye again, the half blue, half white marks appeared; then closing the eye upon which the sun did *not* shine, the whole of the marks appeared a pale blue, scarcely so deep in colour as when in contrast with the white. By squinting, or forcing the eyes to see double, two sets of marks appeared, the one set all blue, the other wholly white.

Subsequently, with the sun upon both eyes, the whole of the marks were blue; whilst upon another occasion, when the sun shone very fully upon both eyes, only the white marks were evident; but shading the eyes by the hand, and allowing a ray to fall upon one eye, the usual half blue half white lines appeared.

On every occasion that I tried the experiment I met with the same results, and when I looked away a beautiful orange-coloured spot—the complementary colour of the blue, I suppose—appeared for some time wherever I looked. What is the cause why only the blue rays were visible? and why blue rather than red or yellow? THOS. WARD

### Yellow Rain

THE following notice will perhaps be of some interest to the readers of NATURE. In December 1870, after a heavy rain at Rosario de Cucuta (New Granada), a great many small round specks of a yellow clayish substance were found on the leaves of plants that had been exposed to the rain. A sample of this substance was sent to Dr. A. Rójas, of this town, who forwarded it to me in order to examine it under the microscope. It proved to be composed almost entirely of a species of *Triceratium*, and another of *Cosmarium*, which must have been carried away by a violent storm from their lacustrian abodes.

Caracas (Venezuela), April 1871 A. ERNST

### The Irish Fern in Cornwall

MY first impulse, on reading the note on this subject in NATURE for the 4th of May, was to apologise to Mr. Dymond for having caused him so much regret by making known a Cornish station for this fern. This first impulse was however checked by the reflection that something is due to the advancement of the study of distributive botany; and I could scarcely have expected even Mr. Dymond to place any very great degree of confidence in my bare assertion, unaided by any reference to localities.

Now that I have done the mischief and made known that the Trichomanes is a Cornish plant, and have been corroborated by Mr. Dymond, it would be interesting to know whether the writer of the note on this fern in the Cheltenham Natural History Society's report found his specimen at the same place, *i.e.* at St. Knighton's. EVERARD F. IM THURN

### Force and Energy

I HAVE been under the impression that the supposed magnetising power of the more refrangible of the solar rays, as first examined by Mrs. Somerville in 1826, had been long ago disproved by the researches of Moser and Riess. This appears, however, to be a mistake, for I see from the report (in a recent number of the *Illustrated News*) of a lecture on Force and Energy lately delivered at the Royal Institution by Mr. Charles Brooke, F.R.S., that the lecturer is stated to have quoted the results of Mrs. Somerville's experiments to "show the interchange of light and magnetic energy."

Is it too much to ask that the editor of a popular treatise on natural philosophy will give us his authority for a statement so contrary to general scientific opinion? EXACTITUDE

### Pangenesis

DOUBTLESS it is owing to a slight misprint in my communication in your number of May 11 that your correspondent, Mr. Meldola, has mistaken the gist of my objection to the theory of Pangenesis. I wrote, "a *seed* borne upon the graft would certainly be affected by the gemmules arising in the root and stem of the stock;" this was printed "a *bud* borne," &c. Now although it seems to me that much has been done to show that the stock will occasionally affect parts of the scion—and this my former letter does not for a moment contest—no evidence whatever has been brought to show that the sexual elements produced upon the scion have ever been affected by the stock without any intermediate change in the parts of the scion which may have borne the affected pollen grains or ovules.* And this certainly ought to be shown before it is assumed that every bud and every sexual element is formed by the aggregation of gemmules from all parts of the parents. Instead of which, in the vast majority of instances, we know that seeds borne upon the scion spring true to the scion. And if any instances to the contrary could be shown, it would then have to be proved that the part of the scion that bore the reproductive element was not a graft-hybrid. I may especially refer your readers to an interesting article by Dr. Masters, in the *Popular Science Review* for April, on "Grafting, its consequences and effects."

A. C. RANYARD

* I should much like to learn if the *Bizzaria orange* can be propagated by seedlings. *Cytisus Adami* is, I believe, always sterile.

## *SIR JOHN HERSCHEL*

FOR nearly one hundred and fifty years Europe has not seen a more accomplished philosopher than the great and good man whose mortal remains were last week consigned to their tomb in the national mausoleum, finding there a significant resting-place close to the grave of Newton. In sorrow and friendly reverence they were followed thither by nearly all that England values as the most eminent in the various domains of those many sciences which he, through a long life, had adorned and advanced.

John Frederick William Herschel was born at Slough, in the early part of 1792, being the only son of that great philosophical astronomer, of whom it were difficult to decide, and one cares not to inquire, whether the father was or was not even more illustrious than the son. Thus the boy was nurtured within sight of that remarkable telescope, wonderful indeed for the day of its construction, which, though in reality among the least of Sir William's achievements, had probably contributed the most to render the name of Herschel famous among men. His education was conducted chiefly at home, or at all events under home influences, and mainly in the society of persons considerably advanced in years; and it is probably to this circumstance that we may attribute much of that singularly retiring, though kindly and affectionate disposition, for which he was so greatly esteemed by all who had the privilege of his acquaintance.

In 1809 he was removed to St. John's College, Cambridge, where there are still retained among a few of its oldest members some curious traditions of his scrupulous attention to the duties of his position. Certain specified selections from the "Principia" of Newton formed of course a portion of the curriculum of study; in that day they came to the student in the form of manuscripts, translated and somewhat modified from the Latin text; John Herschel, however, conceived it his duty to read the entire work just as Newton had left it. We mention this circumstance solely because it furnishes us with an early indication of that staple quality of mind without which no true greatness is ever attained, namely, thoroughness of work. It is not surprising that such a man carried off the highest honours in the University examination, and that in 1813 he graduated as Senior Wrangler of the year; the first among a little phalanx of eminent men, than whom the University of Cambridge has seen nothing superior and not much that is comparable since.

His early lot at Cambridge was cast in times of a scientific transition. To the majority of Englishmen the Continent had long been sealed, and our few men of science were for the most part unacquainted alike with the languages and with the learning of the rest of Europe; indeed, it is scarcely too much to say that the science of mathematics in England had made very little advance beyond what had been known in the later years of Newton. John Herschel, however, possessed the great advantage of living in a house where the chief languages of the Continent were understood, and in which relations with abroad were still maintained. To the late Prof. Woodhouse the honour is due of having introduced to the notice of the Cambridge mathematicians the higher methods of analysis which had long been practised on the Continent, and he was soon ably seconded in his efforts by the young mathematician. In conjunction with his friend Mr. Peacock, who afterwards became the well-known Dean of Ely, John Herschel, in the year 1816, produced a Treatise on the Differential Calculus, for the use of the University, by re-casting rather than by translating a valuable work by Lacroix on that important subject, which hitherto had been studied in England solely to the most meagre extent, and encumbered by the unwieldy garb of the fluxional notation. This work was written in 1816, and before Herschel had taken his master's degree. In 1820 the translation of Lacroix was followed by another and more original work, containing a set of admirable examples and comments on almost all the more important methods of analysis by which mechanical and astronomical science had been so greatly extended by Newton's real successors, such as Euler, Lagrange, and Laplace. In this work Herschel was assisted not only by Mr. Peacock, but also by Mr. Babbage, who wrote that part of it which treated on functional equations. This admirable introduction to the higher forms of analysis is scarcely superseded even at the present day, and in some respects it remains unique. Thus John Herschel was instrumental in the promotion of that great reform in mathematical culture at Cambridge, which has never since ceased to bear most notable and excellent fruit.

It was shortly after his degree that we find the elder Herschel in one of his latest communications to the Royal Society referring with evident satisfaction to the fact that he had a son who was now capable of taking an important part in those astronomical, or rather as they may more properly be called, those cosmical researches which had formed the successful pursuit and the delight of his own life; and before his death he had the pleasure, we might not improperly call it the reward, of seeing his son in the year 1820 become one of the honorary secretaries of the newly-formed Astronomical Society. For fifty years and more he continued to be one of its most constant and loyal supporters, employing some of the last conscious moments of his life in compiling for its service a complete list, or, if we may be allowed the expression, a complete natural history of double stars, commencing with the father's first discoveries, and terminating only with the decease of the son.

Probably the busiest part, where all was busy, in the younger Herschel's laborious life, was passed between the ime of his Cambridge degree in 1813 and the period when he quitted England in order to supplement the exploration of the heavens in the Southern regions, which his father had so ably commenced, if not completed, in the North. Those who have access to the Transactions of the various British scientific societies, and to the learned journals of the day, between 1816 and 1833, will be sure to find at brief intervals some important communication of his, enlarging the boundaries of human knowledge, and bearing the stamp of natural genius, cultivated and developed by honest labour. His fertility in this respect is truly amazing. Partly in conjunction with Sir James South, he re-observed the nebulæ and double stars the existence and the cosmical significance of which had first been brought to light by his father: at the same time adding to the list some thousands of celestial objects which had escaped even his sagacious observation. Like his father also he constructed his telescope with his own hands; an instrument which for many years remained a specimen, unique in its optical capacities and in the efficient simplicity of its mechanical arrangements. Latterly it has been surpassed among amateurs by Mr. Lassell and by Lord Rosse, and among artists by Mr. Grubb; but it was the Herschels who pointed the way and encouraged their successors to stand upon the shoulders of those who preceded them.

Nevertheless astronomy was very far from engrossing his whole attention; we doubt whether it absorbed even the half of it, for those who knew him best knew that the bias of his mind was mainly directed towards chemistry and light, and their cognate branches of physical inquiry. In 1819, when philosophical chemistry in England was perhaps at its lowest ebb, he rediscovered, and for the first time ascertained, the leading properties of the hyposulphite salts, the existence of which had, unknown to Herschel, been previously surmised, and only surmised, by Berthollet. In particular he noted the property of the hyposulphite of soda, whereby, as he says, "chloride of silver newly precipitated is dissolved in this salt almost as readily as sugar." We mention this circumstance because it was owing to this property of the hyposulphite alone, that Daguerre twenty years after was enabled practically to realise the hopes of Davy and Wedgwood, that the photographic pictures which they had already obtained might one day be fixed and preserved, even when submitted to the action of light. Thus, indirectly, John Herschel may, in a strong sense, be regarded as the father of photography; and at subsequent periods perhaps no man has entered more fully and philosophically into the actinic relations of light. It was during this most active period of his philosophical life that, attracted by the marvellous discoveries of Fresnel in connection with the undulatory theory of light, and after having studied and mastered what others had done before him, he set his own original powers to the task, and soon added to our knowledge fresh facts which they, his masters, had themselves overlooked. He discovered, for instance, that the relative positions of the optic axes in certain biaxal crystals were functions of the index of refraction; and he for the first time ascertained certain other actions of crystallised media on polarised light, which placed him at once among the first rank of experimental physicists of his day. The results of these studies he embodied in a most remarkable treatise on Light, published in the "Encyclopædia Metropolitana," which up to the present date may be advantageously consulted by the most accomplished student in this branch of physical inquiry. In the same great work will be found other treatises of his, on Sound, on Heat, and on Physical Astronomy, all of them bearing the stamp of genius and industry, and each one of them containing some specific advance beyond the condition in which he had found the subject.

In 1830 Dr. Lardner induced him to join in the composition of what he designated as the "Cabinet Cyclopædia," and to this Herschel contribute the two most celebrated volumes, viz., the "Preliminary Discourse on the Study of Natural Philosophy," and subsequently the volume on Astronomy. There were but few, if any, men of that day who could have contributed either. The first of them has probably formed the delight, the instruction, and the encouragement of every person who has since pursued or admired a scientific career. In dignity, purity, and pregnancy of language; in profundity of thought, in copiousness of apposite illustration, in a certain indefinable sweetness of persuasion, it, even at this day, captivates every mind that applies itself to its perusal. Here and there indeed its author gives rein to certain metaphysical speculations on Causes and Force, which are now found not exactly to square with the conceptions of later psychological writers. But on such questions as Causation and Force, a man whose mind, like John Herschel's, had been for half a century steeped in the difficult philosophy which embraces and pursues them both, may surely be more safely trusted than other minds, however subtle, whose extent of opportunity and of practical exercise have necessarily been inferior to his. We entertain a strong conviction that when metaphysical science shall, "by taking thought," have arrived at the first cubit of its stature, the deliberate conceptions of Herschel will be found to be in the main correct. A mind like his could have no sympathy with a philosophy which logically admits the thought, that, under some possible state of things, two and two can be equivalent to five. Speculative, he was by nature constrained to be such, but the practical side of his disciplined intellect sufficed to adjust the balance, and to prevent him from going deliberately wrong in his philosophy.

We now reluctantly but necessarily pass over much that is interesting and instructive in the career of the younger Herschel, and approach that crucial period of his life, when, accompanied by his wife and family, he left England for the Cape of Good Hope in 1834. To most of us John Herschel is known chiefly as the most eminent of modern philosophical astronomers; but the pursuit of astronomy was not the voluntary choice nor the chief bias of his intellectual life, it was rather the recollections and the impressions of the happy home of his youthful years, and reverence for the illustrious head of it, which determined him to complete what his father had commenced with such imperturbable diligence, and such wonderful success. He became a great astronomer, rather through filial piety than through the promptings of a natural taste. As in the case of many other great men, some of whom still survive among us, his life-long career was determined by uncontrollable circumstances, while the inborn aptitude has lain in another direction. But passing over such thoughts, suffice it to say that Herschel quitted England for a long sojourn at the Cape of Good Hope, in order to survey those portions of the sidereal heavens which were beyond the reach of his own and his father's instruments. This he did, and wisely and generously did at his own personal expense; for happily, the possession of a moderate fortune enabled him to follow his own bent, and placed him beyond the necessity of the aid and the interference of a patron. How wisely, sedulously, and successfully his time was spent in this happiest of voluntary exiles, may be gathered from the perusal of perhaps the most remarkable volume on philosophical astronomy that has yet appeared.

The publication of this volume was, however, long delayed; he therein unconsciously followed the advice of the Roman poet, "nonum prematur in annum," inasmuch as it was not given to the world as a whole until the year

1847. The truth is that the numerical calculations necessarily entailed for the reduction and the discussion of the observations, occasioned an amount of labour inconceivable to those who are strangers to the requirements of exact astronomical research, and upon all this labour he personally entered. To explain what he required, to such a mind as his, would have been more troublesome and distasteful than to do it himself. As he had done before, so he did now, and so he did again and again while consciousness was accorded to him, he laboured with his own hands. But the book itself, ultimately published by the noble and well-judged munificence of the Duke of Northumberland, is by no means a monument of industry alone; it abounds, in almost every page of its many notes and appendices, with original discoveries, suggestions, speculations, grand and comprehensive conceptions of the distribution of the celestial universe, which will require many a long year to elapse before their significance is exhausted. We may take as an illustration the first instance that occurs to us, in a suggestion, made in a note of Herschel's which might, and for a long time did, escape the reader's notice. He suggests that the main difficulty which occurs in the observation of the sun's photosphere might be removed by viewing its light when reflected from the first surface of a glass prism, which, at the same time, permits the greater part of the heat to escape away from the observer's eye. This simple contrivance, thus rapidly suggested by the way, lies at the bottom of more than one discovery which has since been made relative to the constitution of the solar photosphere; but similar instances abound.

It is hardly necessary to refer to the multiplied and well-earned honours which awaited John Herschel on his return from the Cape of Good Hope. He might have been elected to the Presidency of the Royal Society, but he retired in favour of the Duke of Sussex; and shortly afterwards, not alone for his own sake but for the substantial recognition of an illustrious name among the worthy families of his country, he was made a Baronet of the United Kingdom. Like his great predecessor Sir Isaac Newton, he might have been returned as the representative in Parliament of that noble University where his intellect was nurtured and grew, until it became its brightest and fondest ornament; this honour he declined. Subsequently he was appointed, again like Newton, to the lucrative post of the Mastership of the Mint; but his gentle and unsophisticated nature was ill adapted to cope with the occasional unrealities and difficulties of an official life; it affected his health, and he retired after a tenure of a few years.

Our space forbids us, in this place, to enter upon the more recent portions of this illustrious man's public and scientific career; indeed it cannot fail to be sufficiently known to the great majority of our readers. His true place in the philosophy, and among the great lights of his age, cannot be accurately fixed, until his own generation shall have entirely passed away; for the feelings, the partialities, the prejudices of contemporaneous life, unavoidably warp and incapacitate the judgment, just as too close a proximity to a mass or a multitude is unfavourable to a correct appreciation of its true proportions. Some time after the death of Laplace, the writer of this notice, while travelling on the Continent in company with the celebrated French *savant* Biot, ventured to put to him the question, not altogether a wise one—"And whom of all the philosophers of Europe do you regard as the most worthy successor of Laplace?" Probably no man was better able than Biot to form a correct conclusion, and the reply was more judicious than the question. It was this,—"If I did not love him so much, I should unhesitatingly say, John Herschel." It is from a loving reverence for the memory of a great philosopher and a good man that we now venture to say no more.

Out of the large number of mourning friends who last week in Westminster Abbey gazed with reverential regret at the sorrowful procession which followed the mortal remains of John Herschel, till they were deposited among the best loved and most highly honoured of the worthies of past time, not a few must have recalled to their memories how in their scientific difficulties, or anticipations, or successes, they had betaken themselves to the aged philosopher of Collingwood, and had never failed to meet with the ready aid of a kindly and courteous sympathy.

C. P.

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## *SIR J. HERSCHEL ON OCEAN CURRENTS*

WE are permitted to publish the following letter (probably one of the last written by Sir John Herschel on scientific subjects) which was addressed by him to Dr. Carpenter, with reference to his paper in the Proceedings of the Royal Geographical Society, "On the Gibraltar Current, the Gulf Stream, and the General Oceanic Circulation," a copy of which had been forwarded to him by Dr. Carpenter on its publication, with a request that he would reconsider the opinions he had formerly expressed as to the inadequacy of differences of temperature and specific gravity to produce great movements of ocean water:—

"Collingwood, April 19, 1871

"MY DEAR SIR,—Many thanks for your paper on the Gibraltar Current and Gulf Stream.

"Assuredly, after well considering all you say, as well as the common sense of the matter, and the experience of our hot-water circulation-pipes in our green-houses, &c., there is no refusing to admit that an oceanic circulation of some sort must arise from mere heat, cold, and evaporation as *veræ causæ*, and you have brought forward with singular emphasis the more powerful action of the polar cold, or rather the more intense action, as its maximum effect is limited to a much smaller area than that of the maximum of equatorial heat.

"The action of the trade and counter-trade winds in like manner cannot be ignored; and henceforwards the question of ocean-currents will have to be studied under a two-fold point of view. The wind-currents, however, are of easier investigation. All the causes lie on the surface; none of the agencies escape our notice; the configuration of coasts, which mainly determines their direction, is patent to sight. It is otherwise with the other class of movements. They take place in the depths of the ocean; and their movements and directions and channels of concentration are limited by the configuration of the sea-bottom, which has to be studied over its whole extent by the very imperfect method of sounding.

"I am glad you succeeded in getting specimens of Mediterranean water near the place of the presumed 'salt spring' of Smyth and Wollaston, making it clear that the whole affair must have arisen from some accidental substitution of one bottle for another, or from evaporation. I never put any hearty faith in it.

"So, after all, there *is* an under-current setting outwards in the Straits of Gibraltar.

"Repeating my thanks for this interesting memoir, believe me, Dear Sir,

"Yours very truly,
"J. F. W. HERSCHEL

"Dr. W. B. Carpenter."

We congratulate Dr. Carpenter on having obtained from so eminent an authority, as one of the last acts of his honoured life, this cordial and well-considered acceptance of the doctrine he had previously opposed; and this distinct recognition of the new aspect in which Dr. Carpenter's own observations and reasonings had placed it. The success of his appeal shows that he did not underrate the noble candour of the great philosopher, to whom, more than thirty years previously, he had dedicated his first scientific treatise, as an expression of his gratitude for the moral and intellectual benefit he had derived from the "Preliminary Discourse on the Study of Natural Philosophy." We shall return to this subject next week.

## *PALÆOZOIC CRINOIDS*

AT the seventh and last of the ordinary monthly meetings of the Montreal Natural History Society for the season 1870-71 a communication on a Mineral Silicate injecting Palæozoic Crinoids was made by Dr. T. Sterry Hunt. The author described a gray granular palæozoic limestone from New Brunswick, which had been examined by Dr. Dawson, and found to consist almost entirely of the comminuted remains of brachiopod and gasteropod shells, crustacea, and the joints and plates of crinoids, cemented with a little calcareous spar. The crinoidal remains were, however, found to have their pores filled with a peculiar silicate, which is exposed in relief when the surface of the limestone is attacked by an acid, and then appears as a congeries of small cylindrical rods or bars, anastomosing and forming a beautiful network which, under a magnifying glass, exhibits a frosted crystalline surface, and resembles the variety of aragonite known as *flos ferri*. This silicate, which also fills small interstices among the other calcacereous fragments making up the limestone, is greenish in colour and forms about 5 per cent. of the rock. Though insoluble in dilute acids, it is completely decomposed by strong acids, and is found to be a hydrous silicate of ferrous oxide and alumina, with some magnesia and a little alkali, closely allied to fahlunite and to jollyte. The results of its analysis will appear in *Silliman's Journal* for May. Dr. Hunt remarked that this process of infiltration, by which the minute structure of these palæozoic crinoids has been preserved, was precisely similar to that seen in the glauconite casts of more modern foraminifera, and in the Eozöon of older times. This ancient calcacerous rhizopod, though most frequently preserved by serpentine, had been shown, both by himself in Canada and by Hoffmann in Bohemia, to be in some cases injected by silicate related in composition to that of these crinoids. The great class of silicates of which serpentine, loganite, pyrosclerite, fahlunite, and jollyte are members, are generally described as the results of pseudomorphic changes of pre-existing silicates or carbonates; but Dr. Hunt maintains them to be original aqueous depositions, similar in their origin to the related mineral glauconite; a view now adopted by such investigators as Naumann, Scheerer, Gümbel, and Credner. These facts have an important bearing on the Eozöon Canadense of Dawson, the organic nature of which, though almost universally admitted by zoologists and mineralogists, is nevertheless still questioned by Messrs. King and Rowney. These gentlemen object that the ancient rocks in which Eozöon is found are what are called metamorphic strata, which have been, according to them, subjected to pseudo morphic changes, and therefore the Eozöon may be the result of some unexplained plastic force, which has fashioned the serpentine and other mineral silicates into forms so like those of foraminiferal organisms as to deceive the most practised observer. This was going back to the notions of those who, rather than admit that mountains had been formed beneath the sea, imagined that the fossil shells which they often contain were not the real shells of animals, but the result of some freak of nature. The argument of Messrs. King and Rowney that the Eozöon rock is a result of pseudomorphic alteration because it contains serpentine, is a begging of the question at issue, by asking us to admit that the presence of serpentine is an evidence of metamorphic change, which is denied. The specimens of this organic limestone, with its injected crinoids, differs from Eozöonal rock only in containing at the same time recognisable fragments of other organic remains, and in presenting in its injected portions the differences which distinguish the minute structure of a crinoid from that of a calcareous rhizopod.

Principal Dawson has verified the observations of Dr. Hunt by microscopic examinations. Crinoids in the fossil state are generally filled with carbonate of lime so as to obliterate their pores. The infiltrating silicate in the present case, however, shows, especially in decalcified specimens, that these ancient crinoids closely resembled in their minute structure the modern forms lately studied by Dr. W. B. Carpenter and Prof. Wyville Thomson, especially *Comatula*. The process of filling up the porous calcareous skeleton of the crinoids has been clearly shown to be prior to the cementing and consolidation of fragmentary limestone.

To this we may add that fragments of the calcareous skeleton of Echinoderms infiltrated with silicates have been detected by Dr. Carpenter, together with *Polystomellæ* and many other foraminifera similarly infiltrated, in Capt. Spratt's dredgings from the Ægean. On placing these fragments in dilute acid, the calcareous network of which Dr. Carpenter nearly thirty years ago showed the skeletons of all Echinoderms to be made up, is dissolved away; and a perfect model is left in green or ochreous silicates, of the sarcodic network, with which, in the living state, the interspaces of the calcareous network are occupied. Dr. Carpenter, however, objects to the term "infiltration" as expressive of the process by which the replacement of the sarcodic substance by silicates has taken place. As this process is going on at the present time on the ordinary sea-bottom, he thinks that it can only be attributed to a process of "substitution," in which the decomposition of the sarcodic substance performs an essential part. Whatever may be regarded as chemically the most probable explanation of the replacement, it will be obviously the same for the ancient as for the modern examples of the process.

## *NOTES*

AT the Anniversary Meeting of the Royal Geographical Society held on Monday last, the address of the retiring president, Sir R. I. Murchison, was read by the secretary, Mr. Clements Markham, Sir Roderick thus closing an occupancy of the presidential chair extending over sixteen years. He is succeeded by Sir Henry Rawlinson. The Founder's Medal was on the same occasion awarded to Sir R. Murchison in recognition of the eminent services he had rendered to geography during his long connection with the society, in the course of which he had been associated with every exploring expedition for the last thirty years. The Patron's or Victoria Medal was presented to Dr. A. Keith Johnston for his long-continued and successful services in advancing geography. We regret to find that the retiring president found it necessary to send a letter to the secretary forbidding any hope that he would be able very soon to take an active part in the proceedings of the society, his progress towards complete recovery being slow. At the annual dinner which followed, the Dean of Westminster expressed a hope that we might yet see the foundation of a professorship of geography at each of the universities.

AT the Anniversary Meeting of the Linnean Society, held yesterday, Mr. Bentham delivered his annual address, which we hope to have an early opportunity of giving to our readers. Mr. Bentham was re-elected to the office of President, Mr. Wilson Saunders of Treasurer, and Messrs. F. Currey and H. T. Stainton of Secretaries, and the following gentlemen, in addition, were elected members of the Council for the ensuing year: Mr. John Ball, F.R.S., Mr. Alfred W. Bennett, Mr. J. J. Bennett, F.R.S., Mr. George Busk, F.R.S., Mr. F. Ducane Godman, Dr. J. D. Hooker, F.R.S., Prof. M. A. Lawson, Mr. Henry Lee, Mr. S. J. A. Salter, F.R.S., Dr. J. Lindsay Stewart, and the Rev. Thomas Wiltshire.

THE Board of Natural Science Studies for the Natural Science Tripos of the University of Cambridge has issued a set of schedules indicating the subjects to which the examination in 1872 and following years will be confined, and also those subjects which are suitable for the questions of the first six papers in the examination, as follows:—(1) Chemistry and certain other branches of Physic; (2) Mineralogy; (3) Geology; (4) Botany, Comparative Anatomy, Physiology, and Zoology, including (A) Morphology; (B) Physiology; and (C) Distribution.

THE Slade Professorship of the Fine Arts referred to in our last number has been established in connection with University College, London.

THE University of London has decided on the appointment of two assistant examiners in Experimental Philosophy at an annual salary of 25*l.* each; and a salary of 30*l.* in place of 25*l.* to each of the assistant examiners in Chemistry; as it is thought expedient to charge them with the superintendence of the practical and laboratory examinations at the preliminary scientific and first M.B. examinations.

DR. C. R. A. WRIGHT has been appointed Lecturer on Chemistry and Practical Chemistry at St. Mary's Hospital, *vice* Dr. Russell, who is now the Professor of Chemistry at St. Bartholomew's Hospital.

A LECTURE was delivered on May 17 by Dr. W. B. Carpenter, F.R.S., in the Comparative Anatomy Lecture Room of the New Museum, Cambridge, on the results of the Deep Sea Explorations during the last summer. After a brief *résumé* of the results of the deep-sea dredging up to the end of 1869, the lecturer gave an account of the configuration of the Mediterranean basin, of the singular difference in the laws of temperature at various depths in it and in the neighbouring Atlantic, of the proof of an outward current flowing beneath the surface in-current in the Straits of Gibraltar, of the additional proof of a great oceanic circulation between the waters of Polar and Equatorial regions, which he considered to have a far greater effect on climate than the Gulf Stream, and upon the fauna of the deeper parts of the Lusitanian seas, concluding with some remarks upon the comparatively azoic condition of the Mediterranean basin and the bearing of this upon some geological questions. The lecture was most attentively listened to by a crowded audience, and at the conclusion a vote of thanks, proposed by the Vice-Chancellor and seconded by the venerable Prof. Sedgewick, was carried by acclamation.

THE number of the members of the French Academy of Sciences is fast diminishing. The late Sir John Herschel was a foreign associate member, which is a very rare honour, as there can only be five such members created. Profs. Faraday and Graham were amongst the foreign associates.

THE distinguished members of the medical profession have mostly deserted Paris, although there are many thousand sick and wounded to be taken care of within the walls of the unhappy city. But there are also sick and wounded by scores of thousands outside. Amongst the distinguished practitioners who are in Paris we see the names of M. Broca, the celebrated anthropologist, Lassergue, Maison Neuve, and Axenfeld. But although some professors of the École de Médecine are to be found amongst these medical gentlemen, the School of Medicine is closed. It appears, however, that an irregular course of lectures is kept up open at Hospital Beaujon, which hospital does not belong to the Government.

M. ELISÉE RECLUS, the newly-appointed director of the National Library, asked from his subordinates to give their adhesion to the Commune. Almost everyone refused, and were instantly dismissed.

THE Annual Conversazione given by the President of the Institution of Civil Engineers will be held on Tuesday evening, June 6th. A collection of Models of Engineering Construction, of small and light pieces of Mechanism, and of Scientific Instruments, as well as of Works of Art, by ancient and modern masters of eminence, depicting some engineering work, object, or matter of interest, as "a bridge, lighthouse, aqueduct, or harbour, &c., set in its appropriate landscape," will be exhibited.

THE following Excursions have been arranged by the Geologists' Association for the ensuing month:—Excursion to Yeovil: Monday, May 29, and three following days, under the leadership of Prof. Buckman and Mr. Lobley, including the following points of interest: Yeovil Junction (Fine Section of Upper Lias Sands); Closeworth (Rectory), inspection of large collection of Mesozoic fossils, belonging to Rev. E. Bower, M.A.; Babylon Hill (Inferior Oolite); Halfway House (Inferior Oolite, very fossiliferous); Sherborne (Inferior Oolite, very fossiliferous); Bradford Abbas (Inferior Oolite); inspection of Prof. Buckman's collection; Handford Hill (Upper Lias Sands); Ham Hill (Inferior Oolite, very large quarries, ancient encampment); South Petherton (Middle and Upper Lias, very fossiliferous); Chard (Chloritic Marl, base of Chalk series). Should time permit, Crewkerne (Inferior Oolite) will be visited on return. Excursion to Ilford: Saturday, June 17, under Mr. Henry Woodward.—On arriving at Ilford, the Mammalian beds in the Newer Pliocene deposits at this place will be inspected, and subsequently a visit will be paid to the residence of Sir Antonio Brady, who has kindly invited the Association to inspect his fine collection of stone implements and mammalian remains. Excursion to Caterham: Saturday, July 1, under the management of Mr. Lobley. Fine Sections of the Upper Chalk are exposed in the neighbourhood of Caterham. Excursion to Warwick: Monday and Tuesday, July 10 and 11, under the Rev. P. B. Brodie.—On arriving at Warwick the party will visit the Museum, in which will be found the finest collection of Triassic fossils in England. The Keuper sections near the town will then be inspected. At Wilmcote very interesting sections of the Insect Bed of the Lower Lias are exposed. Should time permit, visits will be paid to the Permian sections at Kenilworth, and to the fine Lower Lias section at Harbury.

WE learn from the *British Medical Journal* that the Committee of the College of Physicians has produced a scheme of amalgamated examination for consideration by the Joint Committee. Under this scheme, the examination would be a minimum examination, and essential for all the universities and licensing bodies of Great Britain. It would be carried out by examiners appointed by a Joint Committee, with a sole view to the fitness of such examiners. The fee for the joint examination would be one calculated only to cover the expenses—about fifteen guineas. The licensing bodies would not confer any licence except upon those who had passed the examination, and each would fix the fee for its licence. If the College of Surgeons will on its part accept this scheme, it will establish its claim to be considered sincere in the cause of medical reform.

WE have received the first four fasciculi of a new Italian Journal of Chemistry, edited by Prof. Cannizarro, of the University of Palermo. It contains a number of origi[illegible]es by the editor, Dr. U. Schiff, Profs. Lieben and [illegible]r well-known chemists, translations of importa[illegible] and an abstract of the proceedings of the [illegible] Italy, Germany, and England.

THE Scottish Arboricultural Society [illegible] of Transactions for the past year, ed[illegible] The objects of the society are the [illegible] Arboriculture in all its branches [illegible] members for the reading of [illegible] reports on the practical opera[illegible] the same; and by such oth[illegible] and it numbers among its [illegible] culturists, and practical foresters of Scotland. In the present volume are a number of practical essays on various points of tree cultivation, and for the current year no fewer than nineteen prizes are offered, the competition for some being limited to working foresters and woodmen.

REFERRING to the statement of the disappearance of Aurora Island (one of the New Hebrides group), recently printed in the American newspapers, Mr. Tryon exhibited to the Conchological Section of the Academy of Natural Sciences of Philadelphia, at their meeting on January 5th, two species of shells from the collection, supposed to be peculiar to this island, remarking that in the event of the reported submergence of the island being confirmed, these must be classed among the lost species. In his report on the mollusca collected by Wilkes's U.S. Exploring Expedition, we learn from the *American Naturalist*, Dr. Gould gives the following account of Aurora Island :—"The little island of Metia, or Aurora Island, to the northeastward of Tahiti, is one of peculiar interest. It is a coral island which has been elevated two hundred and fifty feet or more, and has no other high island near it. On it were found four small land shells belonging to three genera, viz., *Helix pertenuis*, *Helix Dædalea*, *Partula pusilla*, and *Helicina trochlea*. None of these were found upon any other island. They seem to have originated there, after the elevation of the island, and have a significant bearing upon the question of local and periodical creations in comparatively modern times."

IN these high-pressure days, it is astonishing what a saving in money and temper results from an exact punctuality. One would have thought that in every town which possesses a railway station, precise London time would be kept by the public clocks. This, however, appears to be by no means the case. To remedy this inconvenience, we notice that the Rev. H. Cooper Key is urging on the authorities of Hereford the importance of a daily time signal, preferring a time-gun to the dropping of a ball, as more certain to arrest attention. Certainly every town of the size of Hereford ought to have some means of keeping correct time.

A SLIGHT shock of earthquake was felt in Salvador in Central America on the 24th March, $1^h\ 45^m$. On the night of the 30th there were two shocks, with the sky clear and the moon bright. The Salvador earthquake very nearly coincides with two very severe shocks felt all over the Republic of Chile on the 25th March. Since then other shocks have been felt. In Valparaiso the first earthquake occurred at 11.5 A.M. and lasted about a minute, there being no previous noise. A little after 12 a slight shock was experienced, another shortly after 1, and at 5.30 P.M., a shock as strong as that of the morning ; walls were cracked.

ON the 26th March there were slight shocks of earthquake at Arequipa, in Peru.

WE have been favoured by the president of the Halifax (Nova Scotia) Institute of Natural Science with a report of their most recent meetings, which will be found in another column. We congratulate the Institute on the good work done by its members in illustrating the natural history, past and present, of the colony.

THE *Pioneer* of Allahabad contains a communication on snake bites. The writer is inclined to believe there is no antidote, but he thinks it useful to put on record an experiment. About three years ago he saw a bullock which had been bitten by a snake and was lying prostrate on the ground retching. Having heard from an old Brahmin that aniseed soonf was a remedy, he was induced to try it. He mixed aniseed 2 chittacks, pepper ¼, aniseed leaves 1, aniseed bark 1. This was administered internally with great difficulty down the bullock's throat and externally. In a few minutes the bullock lifted his head, in an hour he stood up and began to chew, and in two hours was all right.

## PROF. WYVILLE THOMSON'S INTRODUCTORY LECTURE AT EDINBURGH UNIVERSITY*

IT is too often the first duty of a professor on taking office, to lament the loss of a predecessor who has lately left a blank in the ranks of literature or of science. I am happily relieved from this sad task, for although my friend, Dr. Allman, has found it necessary to retire from the active duties which he has performed so well for many years, his retirement may be looked upon as in a certain sense a gain to science, since he now enjoys a greater amount of leisure to carry on those admirable researches in one of the most difficult and obscure provinces of biology, which have already placed his name high on the roll of those who have added to the store of human knowledge. Although I sincerely trust it may be long before the inevitable time arrives for summing up the labours of George Allman, I believe I may be pardoned if I say a few words about the nature and method of his work, for there is no sounder example which I can cite for your emulation and my own. While keeping pace with the rapid advance of knowledge, and contributing to the general literature of biology the intelligent commentaries and criticisms of an accomplished teacher, Dr. Allman has steadily pursued for many years one special line of research. Whatever he takes up he works out thoroughly and well. His results are fearlessly quoted in all languages as entirely reliable. His straightforward statements and exact descriptions are warped by no preconceived theories, and need no revision or corroboration, and his beautiful drawings are as true to nature as the objects themselves.

No one appreciates more than I do the value of well-founded generalisations. They are the silken threads on which the pearls of truth are strung, and without them we could never realise the full beauty of the gems, their relative value, and their subtle harmonies in form and lustre and tone of colouring ; but the first thing is to dive for the pearls, and a good pearl-diver is immortal !

There is another matter to which I wish to refer, and I do so with unmixed pleasure—the appointment of my friend Prof. Geikie to a separate Chair of Physical Geology and Mineralogy in the University. You are all aware that it is to the munificence of Sir Roderick Murchison that this most valuable addition to our teaching staff is due. Sir Roderick Murchison is a Scotchman who has done more to advance the knowledge of geological and geographical science than any other man living. It is needless for me to speak in terms of eulogy of this last benefit which he has conferred upon the cause of scientific instruction in his native country, but I could not from feelings of personal gratitude allow this opportunity to pass of saying that it is to Sir Roderick Murchison that I owe my first encouragement and assistance in Natural Science ; and, like all who have received favours at his hands, I have found him a kind and steady friend through life.

We have now in the University three distinct departments of Natural Science, and as it taxes any man's energies to the utmost to keep up with the rapid advance of any one of them, it is of incalculable advantage that their teaching should be in different hands. Broadly speaking, Prof. Geikie now takes the inorganic kingdom of Nature, while Dr. Balfour and I divide biological science between us—Dr. Balfour taking the vegetable kingdom, and I the animal kingdom chiefly. The three subjects, however, meet and inosculate at every point, and the more one branch derives illustration from the other the better. This is especially the case with reference to Physical Geology, and the two departments of biology go between them, and demand full illustration from each. A mass of facts has of late years been developed, which group themselves into a special science of boundless interest—the Science of Palæontology. Still there can be no doubt that Palæontology is simply the biology of the present carried back continuously into the past. I will accordingly, with Prof. Geikie's full concurrence, incorporate pure Palæozoology with my Zoology course, and my colleague, Prof. Balfour, will doubtless do the same by Palæophytology ; and we shall both materially trespass upon the domain of Prof. Geikie for the necessary illustration which his special subject affords, fully aware that he must make even heavier requisitions upon us.

Gentlemen, I have already alluded to the division of the Natural Science group of subjects into three—the study of inorganic nature from a natural history point of view, and the study

* Introductory Lecture to the Natural History Class. By Prof. Wyville Thomson, LL.D., D.Sc., F.R.S.

of the two organic kingdoms. As a certain amount of latitude is allowed in a first lecture, I will crave your indulgence while I direct your attention for a few minutes to a group of forms which are said to belong to none of these kingdoms, and to form a kind of Bohemia of their own.

Prof. Ernst Haeckel of Jena, one of the most profoundly learned naturalists and one of the most sensible thinkers of the day, advocates the separation into a distinct kingdom of an immense series of simple organisms, some of which had been hitherto regarded as plants and others as animals.

Prof. Haeckel's opinion on a matter of this kind is of the highest value, not because he has made a bold and certainly premature attempt to rearrange the universe on Darwinian principles, but because he has been a most sagacious and careful student of the lower forms of life. He has raised for himself a monument in his wonderful monograph of the Radiolaria, which will endure along with Darwin's researches on coral reefs and on the Cirripeds, when the Darwinian theory of modification through Natural Selection may possibly be remembered only as one of the most brilliant of those broken lights which have been shed from time to time by gifted men on the plan of the Divine Creator.

According to Professor Haeckel, the material universe, so far as we at present know it, resolves itself into minerals, protista, plants, and animals. I may say at starting that, along with most of my brother naturalists in Britain, I regard the introduction of this new "kingdom," the Protista, as a mistake; but as the proposal even involves most interesting questions as to the relations between the three recognised kingdoms, it is well worthy of careful consideration. Haeckel ranges among the Protista the Monera, a group of peculiar forms, of which he himself has been the most successful student, and these may be taken as the type of the "Protistenreich;" the protoplasta, containing amœba, difflugia and their allies; the diatoms, the flagellate infusoria; the fungi; the noctiluca; and the rhizopoda.

It is foreign to my present purpose to trace in detail the various steps by which our views of the ultimate process of organisation have been modified during the last few years; how after the publication of the brilliant observations of Schleiden and Schwann, the nucleated cell was almost universally regarded as the physiological unit; how the researches of Max Schultze, Leydig, Beale, and Hofmeister gradually shook our faith in the earlier conceptions of this cell-unit, and did away at all events with the necessity of a cell-wall, proving such a wall, when it existed, to be an excretion, and showing that the vital activity of the cell resided solely in the nucleated spherule of contractile sarcode which forms the cell-contents of every living cell—and how finally Cohn, Max Schultze, Huxley, and Haeckel cast doubts upon the value of the nucleus and upon the necessity of any cell-like limitation, and seemed to render the view highly probable that the vital activity of all organisms, even of the most highly organised, resides essentially and ultimately in a diffused homogeneous "germinal matter," or "protoplasm," of which all formed tissues are modifications or excretions.

It is impossible in the present state of knowledge to subject any view as to the ultimate mechanism of the formation of tissue through the means of protoplasm to direct proof. It seems now to be a very generally received opinion, supported by Huxley, Max Schultze, Hofmeister, Beale, and many others, and notably by Oscar Schmidt, who would seem to bring it almost to demonstration in his beautiful researches on the sponges of the Adriatic, that protoplasm is simply converted, with a certain change of composition, into tissue or "formed material." There are, however, almost insuperable objections to this view. The secondary products of organisation (formed material) are most various in their chemical constitutions, and it involves the admission that protoplasm may change in its chemical composition till it is *almost* carbonate of lime, or silica, or starch, or horn, or cellulose; the last stage of the metamorphosis being its absolute separation as one or other of these bodies. Another view which I have always regarded as more probable is that protoplasm, the substance which is endowed with the peculiar vital property, has always the same composition, and that it acts simply by catalysis, inducing, under certain known laws, decomposition and recombination in compounds which are subjected to its influence, without itself undergoing any change, absorbing the nascent products of combination and decomposition, and recombining them and reserving them with reference to the development or maintenance of the organ to which it gives its life.

The researches of Prof. Haeckel on the Monera have perhaps been of higher value in support of the protoplasm view than any others, for they have given abundant proof that independent beings may exist and may show all the essential phenomena of life without the slightest trace of differentiation of any part of their substance into investing wall or nucleus or distinguishable part of any kind, simply as masses of contractile jelly, particles of albumin endowed with the faculties of nutrition and reproduction.

The positive character to which Prof. Haeckel trusts for the definition of his protista kingdom is the entire absence in all the groups which it contains of sexual reproduction. He contends that all protista are monogenetic, reproducing by gemmation or fission alone. This character he conceives separates them definitely from true animals and true plants. Before passing to the consideration of the general relations of the animal and vegetable kingdoms and the position of these questionable forms, I wish to say that I do not attach much importance to this negative character. Of late years enormous advances have been made in our knowledge of the process of reproduction in lower organisms, and we find that multiplication by gemmation in various forms is infinitely more common than was supposed; that in some cases multitudes of individuals and apparently several generations are reproduced by gemmation alone, without the intervention of sexual reproduction; but at the same time I believe that the tendency of modern research is to make it more and more probable that in all cases conjugation or some other form of sexual reproduction comes in at some part of a definite or indefinite series of broods, and starts as it were an entirely new stock. I am inclined to think that in those cases where we find only monogeny, it is probably from a want of knowledge of the life history, not of an individual, but of a complete cycle of individuals. Besides, the reproductive process in some of these lower forms is very obscure. Not many years ago, accepting Prof. Haeckel's test, all the orders of cryptogamic plants would have belonged to the protista. Who could have anticipated the obscure and beautiful process of fertilisation in ferns? It seems scarcely possible that there should be no equivalent process in fungi, if we could only find it out.

Let us now consider for a moment the characters on which the older kingdoms have been founded, and see how they have stood the test of advancing knowledge. We shall thus be the beter able to judge of the stability or otherwise of the proposed new kingdom.

The consideration of the inorganic kingdom need not, I think, detain us long. Any two groups of things conceivable must have some analogies or resemblances; but it seems to me that any essential relations which have been founded on the resemblances between inorganic substances and organised beings are purely fanciful. Of course, it is impossible to say that a point of continuity may not be discovered, but as yet the boundary line seems sufficiently trenchant.

Inorganic substances never *live*, they are either simple (according to our present state of knowledge) or they may originate from the combination of two or more substances which unite in definite proportions; they may exist in any physical condition from solid to gaseous, but they are homogeneous, that is to say, any portion which may be detached exactly resembles the remainder in composition and in properties; they increase by the addition of like particles from without to the external surface; they may be indefinite in external form or amorphous, but almost universally they tend to assume the form of regular geometric solids bounded by planes, which have a definite relation to one another in position—to crystallise. Internally inorganic substances are at rest, unless their atoms be set in motion, or unless they be otherwise affected by forces acting from without; they initiate no motion nor change. If one could imagine a quartz crystal absolutely isolated from all external influences, it might remain unchanged for ever.

An organised being, on the other hand, either *lives* or has lived during some part of its existence; if living, every part of it is in constant motion and change; it increases by the imbibition of heterogeneous matter from without, by its assimilation, and by the intercalation of the particles of the assimilated food among the particles of the substance already laid down, by molecular intussusception; and old molecules which have undergone change are constantly being removed and replaced by new ones. An organised being always contains a mixture of solids, liquids, and gases; it is never homogeneous nor uniform in structure, but consists of structural elements which are distinct in character, and each of which has its part to play in the production and regulation of the movements and changes which are unceasing; it always contains certain substances in what is called "unstable equilibrium," which become decomposed and reduced to more stable compounds the moment the peculiar vital property is lost. An organised being is not produced by the direct union of definite proportions of two or more simple substances; it arises by the growth of a germ, a portion separated from the body of a pre-existing organised being of the same kind. Finally, organised beings never assume accurate geometrical forms, but under the influence of life each kind of organised being assumes a characteristic, though not absolutely definite shape, which is the resultant of the sum of the shapes of all its structural elements, which has a very close relation to the shape of the organised being from which it was derived as a germ, though it is not identical with it, and which is called its individual form.

I have thus far contrasted inert matter with organised beings possessing life. That the term life indicates a very special property there can be no doubt, but, as yet, an impenetrable veil seems to shroud its ultimate processes. I believe, however, that the veil is at the far end of the labyrinth in which we are now wandering, and that patient observation and guarded generalisation may yet enable us greatly to narrow the limits of the unknown—to approach some steps nearer to the veil. I must premise that, as I am now looking at the subject from a purely physiological point of view, I regard life simply as a condition capable of producing certain perceptible phenomena, and can take no cognizance whatever of that mysterious union between spirit and matter which is broken in passing through "the valley of the shadow of death." Material processes and material changes only are subject to the material instruments of biological research. Those inner mysteries are now and must probably ever remain—in our present condition of existence—beyond the veil.

It becomes daily more manifest with the advance of knowledge that the action of known physical laws—such as chemical affinity and capillarity as manifested by porous media and by colloids—are most intimately interwoven with all organic processes, and it is, as yet, impossible to say how far life may influence, in the sense of modifying or directing, the action of these laws. Life has been called the vital force, and it has been suggested that it may be found to belong to the same category as the convertible forces heat and light. Life seems, however, to be more a property of matter in a certain state of combination than a force. It does no work in the ordinary sense. If a man lift a weight a couple of feet off the ground, many of the so-called vital actions are called into play, but yet every part of the work done can be accounted for by the action of the ordinary physical forces. The act of the will, in regal phrase the "mere motion," which induced the lifting of the weight, can be referred, we can scarcely doubt, to the mechanical action of some part of a large and complicated apparatus, the cerebral hemispheres, and was accompanied by a waste of its substance.

The telegraphic communication to the muscles involved which harmonised their several acts and signalled the contraction of their fibres, was conveyed through a cord whose molecules were set in vibration by a force very probably convertible with the physical forces, generated by chemical change and the waste of tissue; and in the muscle, the organ by which the weight was actually raised, an amount of waste took place—that is to say, an amount of carbon was combined with oxygen precisely equivalent theoretically to the quantity of coal which must have been burned in a perfectly constructed engine to do the same work.

Chemical forces act in living beings under very special circumstances. For a series of years a mass of substances are held undergoing constant change and throughout in the most unstable state of chemical combination. The instant the condition of life is removed, decomposition commences, and the complex constituents of the body are resolved into more simple and stable combinations. But yet it may be fairly questioned whether the chemical relations of the component elements of an organised body are in any way directly affected or controlled by life. It has become quite conceivable, especially through the researches of the late Master of the Mint, that a constant adjustment and re-adjustment of membranous and colloid diaphragms in the presence of powerful catalytic agents may possibly explain the maintenance of almost any chemical conditions however complicated.

The one function of living beings whose explanation it seems at present impossible to imagine except by regarding it as the manifestation of a special property, is what has been called the "moulding of specific form;" the building up of a heterogeneous and complicated organism, which shall repeat, not rigidly but with a certain flexibility, the characters which have been transmitted to it through a germ from a parent, every molecule of every part having thus a direct relation in form, in position, and in composition, to every other molecule of the body. At present, regarding it from a purely material point of view, we are scarcely justified in regarding life as more than that condition of an organised being in which the products of chemical and physical changes taking place within it are stamped with a specific organic form.

(*To be continued.*)

## *SCIENTIFIC SERIALS*

*Journal of the Ethnological Society of London* (January 1871). A paper by Mr. E. B. Tylor on "The Philosophy of Religion among the lower Races of Mankind" gives in a condensed form his views on the development of "Animism," *i.e.* the doctrine of the soul, and of spirit and deity in general, a subject which is treated at length in his recently published work on "Primitive Culture."—Prof. Huxley's address on the "Geographical Distribution of the Chief Modifications of Mankind" is accompanied by an ethnological map, which curators and lecturers will do well to adopt as a wall-map. The principal races of mankind are defined as the Australioid, Negroid, Mongoloid, and the Xanthochroic and Melanochroic (fair and dark whites) Among the special features in Prof. Huxley's scheme of the races of mankind, the following are prominent. The indigenous non-Aryan tribes of Central and Southern India, and less closely the ancient Egyptians and their descendants, the modern Fellahs, are referred to the Australioid type. The Negroid type of Africa is divided between the Negroes proper and the Bushmen of the extreme south, the Hottentots being considered a cross-breed between these two races. The Mongoloid type is made to include not only the brachycephalic Tatar races, but classification by skulls is set aside, and the group is arranged to include the Chinese and Japanese. The "absurd denomination" of "Caucasian" is abandoned, and the nations thus described by ethnologists come under the titles of Xanthochroi, fair whites, who are classed as of special type, and Melanochroi, dark whites, which latter Prof. Huxley is disposed to consider as sprung from intermixture of Xanthochroi and Australioids. In this classification of human types or races, Prof. Huxley rests on physical characteristics, treating language as subordinate. In his remarks on "The Ethnology of Britain" he again states his views as to the great division of European men between the fair whites of the centre and west, and the dark whites of the south. Both types occur in the early population of our islands, the use of Celtic language not corresponding with a racial distinction.—Dr. Nicholas's paper on the "Influence of the Norman Conquest on the Ethnology of Britain" is in strong antagonism to the view that Englishmen are ethnologically "Low Dutch." In his view, the old British race, in great measure, kept its early type, the Saxon, Danish, and Norman invasions affecting language, government, &c., rather than replacing the population itself.—Among the papers on Prehistoric Archæology are Sir John Lubbock's description of the Park Cwm Tumulus, and an account of remains of "Platycnemic Men in Denbighshire," by Mr. W. Boyd Dawkins and Prof. Busk.—Canon Greenwell's paper on "The Opening of Grime's Graves in Norfolk," gives full particulars as to the site of a Stone Age manufactory of implements from the excellent flint of the district. The chalk was systematically mined for the flint, and the so-called "Grime's Graves" are ancient pit-workings of this class. Colonel Lane Fox is disposed to explain in the same way the "Danes' Holes" in Kent, long a puzzle to antiquaries.—Looking at the number of the journal of the Ethnological Society, it is to be hoped that the journal of the new Anthropological Institute will maintain its very high standard of succinctness, solidity, and general interest.

THE *Geological Magazine* for the present month (No. 83) contains only four original articles, of which the first is an account by the editor, Mr. H Woodward, of the objects which more particularly attracted his attention during a recent visit to the Brussels Museum. He notices especially the fossils of the Antwerp crag, and a fine example of the mammoth found at Lierre, in the province of Antwerp, in a sufficiently perfect state to be mounted as a skeleton. Two figures of this interesting specimen are given. Mr. Woodward also refers to a fine series of skulls of *Ursus spelæus* from the Belgian caves contained in the Museum at Brussels.—Mr. Whitaker describes the chalk of the cliffs from Seaford to Eastbourne in Sussex, which he illustrates by a section, and compares with that of the Kentish cliffs.—In a paper (illustrated with a map) on the Denudation of the Coalbrook Dale coal-field, Mr. Daniel Jones endeavours to explain the puzzling arrangement of the coal measures in that locality by demonstrating that the southern portion of it has been largely denuded, and subsequently overlain by coal measures of younger age, so that the deposits are not uniform and persistent.—Mr. W. Davies gives us an alphabetical catalogue of type specimens of fossil fishes in the British Museum, in continuation of the similar lists already published by Sir Philip Egerton and Lord Enniskillen of the type specimens in their collections.—The remainder of this number is occupied as usual by notices, reviews, reports and correspondence.

THE *Proceedings of the Royal Irish Academy*, Series II. No. 2 of vol. i. has just been presented to the members. This Part contains Mr. Andrews's notice of the capture of *Ziphius Sowerbi.* The President's Annual Address. W. Archer "On some new or little-known Freshwater Rhizopods (Plates 12 and 13). Mr. R. C. Tichborne, Laboratory notes. G. J. Stoney "On the Cause of the Interrupted Spectra of Gases." Prof. R. Ball "On the Motion of Vortex Rings in Air." C. E. Burton "On Results obtained by the *Agosta* Expedition to observe the Recent Solar Eclipse." Principal Dawson, Note on *Eozöon Canadense.* Prof. T. S. Hunt, Notes on Messrs. King and Rowney on *Eozöon Canadense.* Prof. Macalister, "On Human Muscular Anomalies." The Appendix contains the minutes of the Proceedings of the Academy, and the Correspondence relative to the Bombardment of Paris.

IN the *Journal of Botany* for May, Mr. C. E. Broome describes a new British fungus *Scleroderma Geaster*, with a lithograph. The contributions to local botany are a continuation of Mr. More's Supplement to the "Flora Vectensis," and Notes of plants of the neighbourhood of Oxford, by Prof. Thistleton-Dyer. We have also a further instalment of Dr. Hance's "Sertulum Chinense," and the usual short notes, reports, reviews, and proceedings of societies.

## SOCIETIES AND ACADEMIES

### LONDON

Zoological Society, May 16.—Prof. Flower in the chair.—The Secretary read a report on the additions that had been made to the society's menagerie during the month of April 1871; and called particular attention to a female of the lately-described Prince Alfred's deer (*Cervus alfredi*), which had been received in exchange, and was stated to have been originally brought from the Philippines.—An extract was read from a letter addressed to the secretary by Dr. R. A. Philippi, Director of the National Museum at Santiago, stating that no species of the tortoise was known to occur in Chili, and that the specimens upon which the so-called *Testudo chilensis* had been based had been received from Mendoza, in the Argentine Republic.—Prof. T. H. Huxley communicated a paper by Dr. P. Martin-Duncan, F.R.S, containing descriptions of the Madreporaria (stony corals) dredged up during the expedition of H.M.S. *Porcupine* in 1869-1870.—Sir Victor Brooke, Bart., F.Z.S., read a paper on Speke's Antelope (*Tragelaphus spekii*) and the allied species of the genus *Tragelaphus*, in which the distinguishing characters of these animals were pointed out, and their synonymy and distribution given.—Mr. P. L. Sclater communicated some notes on a collection of birds made in the vicinity of Lima, Peru, by Prof. W. Nation, of that place, with notes on their habits by the collector.—A second communication from Mr. Sclater contained a continuation of his notes on rare or little-known animals now or lately living in the society's gardens. Mr. Sclater also gave the description of a new parrot, now living in the society's gardens, which he proposed to call *Lorius tibialis.*—Mr. R. B. Sharpe read a note on *Macheirhamphus anderssoni*, a very rare Accipitrine bird from Damara Land, and gave a history of the two species of *Macheirhamphus* now known to science.—Mr. J. Gould exhibited and pointed out the characters of a new humming bird, lately discovered by Mr. H. Whitely in Peru, which he proposed to call *Helianthea osculans;* and likewise characterised five other new species of the same family of birds.

Geological Society, May 10.—Prof. Morris, vice-president, in the chair. Dr. Henry Nyst, of Brussels, was elected a foreign member, and Prof. G. Dewalque, of Liège, a foreign correspondent of the Society. The following communications were read:—1. On the Ancient Rocks of the St. David's Promontory, South Wales, and their Fossil contents, by Prof. R. Harkness, F.R.S, and Mr. Henry Hicks. In the Promontory of St. David's the rocks upon which the conglomerates and purple and greenish sandstone, forming the series usually called the "Longmynd" and "Harlech Groups," repose, are highly quartziferous, and in many spots so nearly resemble syenite that it is at first difficult to make out their true nature. The apparent crystals are, however, for the most part angular fragments of quartz, not possessing the true crystalline form of the mineral. The matrix does not exhibit a crystalline arrangement, and contains a very large proportion of silica, much exceeding that which is obtained from rocks of a syenitic nature. These quartziferous rocks form an E.N.E and W.S.W. course. The arrangement of these rocks, which seem to be quartziferous breccias, is somewhat indistinct. In the immediate neighbourhood of St. David's they have associated with them irregular bands of hard, greenish, ashy-looking shales, much altered in character, but often presenting distinct traces of foliation. In a ridge running from the S.E. of Ramsey Sound in a north-easterly direction, the greenish shales are more compact, and resemble earthy greenstones. The quartziferous breccias and their associated shales form two anticlinal axes, contiguous to each other, and have on their S.S.E. and N.N.W. sides purple and green rocks. The order of the rocks from the quartziferous breccias upwards, when not disturbed by faults, is as follows:—

*Lower Cambrian.*

| | feet. |
|---|---|
| 1. Greenish hornstones on the S.E., and earthy Greenstones on the N.W., forming the outermost portions of the so-called Syenitic and Greenstone ridges. | |
| 2. Conglomerates composed chiefly of well-rounded masses of quartz imbedded in a purple matrix . . | 60 |
| 3. Greenish flaggy sandstones . . . . . | 460 |
| 4. Red flaggy or shaly beds, affording the earliest traces of organic remains in the St. David's Promontory, namely, *Lingulella ferruginea* and *Leperditia cambrensis* . . . . . . . . . . | 50 |
| 5. Purple (sometimes greenish) sandstones . . . | 1000 |
| 6. Yellowish-grey sandstones, shales, and flags containing the genera *Plutonia*, *Conocoryphe*, *Microdiscus*, *Agnostus*, *Theca*, *Protospongia* . . . . . | 150 |
| 7. Grey, purple, and red flaggy sandstones, containing, with some of the above-mentioned genera, the genus *Paradoxides* . . . . . . . . . | 1500 |
| 8. Grey flaggy beds . . . . . . . . . | 150 |
| 9. The true beds of the "Menevian Group," richly fossiliferous, and the probable equivalents of the lowest portions of the Primordial Zone of M. Barrande . | 550 |

The discovery of a fauna specially rich in trilobites, among these rocks of the St. David's Promontory, affords very important information concerning the earlier forms of life of the British Isles. Until the discovery of this fauna, these rocks and their equivalents in North Wales were looked upon as all but barren of fossils. We have now, scattered through about 3000 feet of purple and green strata, a well-marked series of fossils, such as have nowhere else been obtained in the British Isles. In the Longmynds of Shropshire the only evidence of the existence of life during the period of their deposition is in the form of worm-burrows, and in the somewhat indistinct impressions, which Mr. Salter regards as trilobitic, and to which he has given the name of *Palæopyge Ramsayi*. If we assume the purple and green shales and sandstones, with their associated quartz rocks of Bray Head and the drab shales of Carrick M'Reily, county Wicklow, to represent the old rocks of St. David's, they afford only very meagre evidence of the occurrence of life during the period of their deposition in the form of worm-burrows and tracks, and in the very indeterminate fossils which have been referred to the genus *Oldhamia*. One very prominent feature about the palæontology of the ancient rocks of St. David's is the occurrence of four distinct species of the genus *Paradoxides;* and this is in strong contrast with the entire absence of the genus *Olenus*. On a comparison of the palæontology of the St. David's rocks with those of the continent of Europe and of America, which seem to occupy nearly the same horizon, we have like features to a very great extent presenting themselves. With reference to the distribution in time of some of the earlier genera of trilobites, it would appear that the genus *Olenus* is represented in Britain and Europe by twenty-two species, confined to the Lingula-flags and Tremadoc rocks, and not occurring so low as the Menevian group. The absence of this genus from the Menevian group, and its occurrence throughout the whole of the Lingula-flags, and in the Tremadoc rocks, along with the fact that so far as present observations go, no species of *Paradoxides* ranges higher than the Menevian group, have afforded good palæontological grounds for placing the line of demarcation between Upper and Lower Cambrian at this spot, and for including the Menevian group in the Lower Cambrian, to the bulk of which it is intimately united palæontologically. Mr. Hughes bore testimony to the admirable work done by Mr. Hicks, who had, almost unaided, worked out the geology of that district. Allowing that many subdivisions and new specific names had with great advantage been introduced into petrology, he defended the Survey nomenclature by reference to the then received definition of syenite and greenstone, terms still perfectly understood and applicable to the main mass of the rocks in question, though possibly subsequent closer examination and new sections may have rendered some modification of the boundary lines desirable. He was prepared to allow the metamorphic origin of all rocks of the classes under consideration, but did not think there was sufficient evidence to show that the divisional planes in the syenite and greenstones of St. David's were due to original stratification, but might correspond rather to the great joints of most granites. Mr. Hughes pointed out that the conglomerate contained fragments of the hornstone and quartz of this older series, which he considered was probably part of an old ridge or shoal, possibly of Laurentian, but certainly of Pre-Cambrian age, and thought that there were slight differences in the lithological character of the beds on either side, such as might be explained on this supposition. He agreed with Prof. Ramsay in thinking that there was evidence of the proximity of land in early Cambrian times, but was not prepared to refer these red rocks to inland seas or lakes as opposed to open sea; the whole seemed rather the deposit of an open sea encroaching during submergence. He did not attach very much importance to the restriction of genera to limited horizons in these older rocks of St. David's. For, as it was reserved for Dr. Hicks to discover these fossils after so many other observers had examined the district, he anticipated that further researches must certainly result in finding links which will connect together more closely beds, the stratigraphical relations of which seem to indicate so clearly an unbroken though varying series. Mr. Gwyn Jeffreys had been struck by the intercalation of non-fossiliferous beds from time to time among the fossiliferous beds described in the paper. This was the case in beds now in course of formation, and appeared to arise from the great deposits of mud brought down by rivers and redeposited in certain positions in the sea-bed. That this was the case had been proved by recent dredging operations both in the Atlantic, off Spain, and in the Mediterranean. Mr. Boyd Dawkins called attention to the gap which had been filled by the discoveries recorded in the paper, inasmuch as the Molluscan, Annelid, and Crustacean forms were now carried back far into the Cambrian period, and yet without any trace of their convergence, so that the origin of life might be as far removed from that period as was the Cambrian from the present time. The difference in the colours of the rocks he was inclined to refer to the different degrees of oxidisation of the iron they contained, which might supervene in a comparatively short time. The Rev. W. S. Symonds had, in visiting the spot, been much struck by the rocks, at that time termed syenite, which he believed might be an extension of those on the Carnarvonshire peninsula, and which he thought supported the whole series of the Cambrian rocks, so that they might after all be the Laurentian, the same as those of Sutherlandshire and Assynt. If this were the case the nomenclature of the Geological Survey would have to be altered, and the rocks of Pistyl and Holyhead no longer termed metamorphosed Cambrian rocks, but Laurentian. Mr. Hicks, in reply, stated that the quartziferous breccias forming the central ridge contained so many rolled pebbles, and were, moreover, in places so distinctly bedded, that there could be no doubt of their being sedimentary. Other beds, described as greenstone in the maps of the Geological Survey, were also distinctly laminated. The non-occurrence of fossils in the more sandy beds he attributed to their having been deposited in very shallow water. The fossils occurred principally in fine-grained beds of a flaggy nature. "On the Age of the Nubian Sandstone," by Mr. Ralph Tate, F.G.S. The author remarked that the sandstone strata underlying the Cretaceous limestones, and resting upon the granitic and schistose rocks of Sinai, had been identified with the "Nubian Sandstone" described by Russegger as occurring in Egypt, Nubia, and Arabia Petræa. In the absence of palæontological evidence, this sandstone has been referred to the Mesozoic group, having been regarded by Russegger as Lower Cretaceous, and by Mr. Bauerman and Figari-Bey as Triassic, the latter considering an intercalated limestone bed to be the equivalent of the Muschelkalk. The author has detected *Orthis Michelini* in a block of this limestone from Wady-Nasb, which leads him to refer it to the Carboniferous epoch, as had already been done by the late Mr. Salter from his interpretation of certain encrinite-stems obtained from it. The author mentioned other fossils obtained from this limestone, and also referred to the species of *Lepidodendron* and *Sigillaria* derived from the sandstone of the same locality. He regarded the Adigrat Sandstone of Mr. Blanford as identical with the Nubian Sandstone.—3. "On the Discovery of the Glutton (*Gulo luscus*) in Britain," by Mr. W. Boyd Dawkins, M.A., F.R.S. The author in this paper described a lower jaw of the Glutton, which had been obtained by Messrs. Hughes and Heaton from a cave at Plâs Heaton, where it was associated with remains of the wolf, bison, reindeer, horse, and cave-bear. He remarked that he could detect no specific difference between the *Gulo spelæus* Goldfuss, from Germany, and the living *Gulo luscus*, except that the fossil Carnivore was larger than the living, probably from the comparative leniency of the competition for life in postglacial times. He referred to the distribution of the Glutton in a fossil state, and argued that its association with the reindeer, the marmot, and the musk-sheep would imply that the postglacial winters were of Arctic severity, whilst the presence of remains of the hippopotamus, associated with the same group of animals, would indicate a hot summer, such as prevails on the Lower Volga. Mr. Hughes indicated the exact position in which the jaw of the glutton was found, but pointed out that, owing to the excavations of keepers, badgers, rabbits, &c., the earth was so much disturbed in that part that it was impossible to be sure of the original relative position of the bones. He showed that the Plâs Heaton Cave was on a hill rising from the top of the plateau, while the Cefn, Brysgill, and Galltfænan Caves were in the gorge cut through that plateau, and therefore that the Plâs Heaton Cave was probably formed, and might possibly have been first occupied, at a much earlier period than the others. As it appeared to pass under that part of the hill which is overlapped by heavy drift, he thought it quite possible that this may have been a preglacial cave, and that by and by we may find evidence of preglacial fauna in it. The Rev. W. S. Symonds mentioned that in some of the pot holes in the roof of the Cefn Cave he had procured silt containing remains of shells determined by Mr. Jeffreys to be marine. Mr. Hughes explained that these shells had probably been washed in from the superficial drift of the district. Mr. Dawkins, in reply, expressed his belief that though the excavation of the caves in question might have taken place at different periods, yet that their occupation was, geologically speaking, contemporaneous.

Mathematical Society, May 11.—Mr. W. Spottiswoode, president, in the chair. Mr. C. J. Monro, B.A., late Fellow of Trinity College, Cambridge, was elected a member; and Mr. J. Griffiths, M.A., Fellow of Jesus College, Oxford, was proposed for election. The Hon. J. W. Strutt, fellow of Trinity College, Cambridge, was admitted into the Society. Prof. Henrici indicated the method of treatment he had employed in his paper "On the Singularities of the Envelopes of a non-unicursal Series of Curves." Mr. Strutt then read his paper "On the Resultant of a large Number of Vibrations of irregular phase, as applied to the explanation of Coronas." Sir W. Thomson, Prof. Clerk Maxwell, and Mr. Strutt made some further remarks on the subject of the paper. Mr. Maxwell then gave a description of two solar halos he had recently seen, and Prof. W. G. Adams gave some additional particulars in the case of one of the phenomena which had also been noticed by himself. Prof. Cayley then communicated an account of a paper by Mr. J. Griffiths "On the problem of finding the circle which cuts three given circles at given angles." The president next requested assistance in the solution of a "Question on the Mathematical Theory of Vibrating Strings," which he had been unsuccessful in solving. Mr. Strutt mentioned some results he had arrived at in reference to the subject of inquiry. A communication from Prof. Cayley respecting the extension of the Society's sphere of action was laid before the meeting by the president; it was determined that the matter should be discussed at the Society's next meeting. Prof. Maxwell asked for information as to the convention established among mathematicians with respect to the relation between the positive direction of motion along any axis, and the positive direction of rotation round it. In Sir W. Hamilton's Lectures on Quaternions the coordinate axes are drawn, $x$ to South, $y$ to West, and $z$ upwards. The same system is adopted in Prof. Tait's Quaternions, and in Listing's Vorstudien zur Topologie. The positive directions of translation and of rotation are thus connected as in a left-handed screw or the tendril of the hop. On the other hand, in Thomson and Tait's "Natural Philosophy," p. 234, the relations are defined with reference to a watch, and lead to the opposite system, symbolised by an ordinary or right-handed screw, or the tendril of the vine. If the actual rotation of the earth from west to east be taken positive, the direction of the earth's axis from south to north is positive in this system. In pure mathematics little inconvenience is felt from this want of uniformity, but in astronomy, electro-magnetics, and all physical sciences, it is of the greatest importance that one or the other system should be specified and persevered in. The relation between the one system and the other is the same as that between an object and its reflected image, and the operation of passing from one to the other has been called by Listing *Perversion*. Sir W. Thomson and Dr. Hirst stated the arguments in favour of the right-handed system, derived from the motion of the earth and planets, and the convention that north is to be reckoned positive. The right-handed system, symbolised by a corkscrew or the tendril of the vine, was adopted by the society.

### Halifax, Nova Scotia

Institute of Natural Science, March 13.—Mr. J. M. Jones, F.L.S., president, in the chair. D. J. B. Gilpin read a paper on the Mammalia of Nova Scotia, being the ninth part of a series on that subject delivered before the institute. The present paper included the common hare (*Lepus americanus*) and the cariboo (*Rangifer cariboo*) or reindeer of the province. The author stated that whilst Newfoundland and the country around Hudson's Bay were represented by the polar hare (*L. glacialis*) which varied in colour even to pure white, and New England on the south by the wood hare (*L. sylvestris*) which never varied, Nova Scotia had the American hare (*L. americanus*) which varied to a soiled rusty-white, and which had been confounded with both the other species. A specimen of this last species taken early in November, and which might be considered as in summer pelage, was sepia-brown with a yellow wash and coarse black hairs on the back, breast, belly, and inside the legs white, tips of ears black, and pads light rusty. One taken in December of the same year and which may be taken as a winter specimen, was soiled white with rusty streaks on the back and sides; nose and circlet around the eyes rusty; under parts, pure white; a rusty streak on fore arm always, and often upon the thigh. The only parts which remained unchanged were the white of the belly, the black ear tips and the rusty pads, and that all the hair, both, summer and winter, had a lead coloured base. The change of colour takes place during the month of December, and is the result of the summer coat being shed and replaced by the winter one. The American hare abounds in the province, keeps close covert, and is nocturnal. In concluding his remarks upon this the last of the list of rodents found in Nova Scotia, Dr. Gilpin stated that although the equator produced no arctic forms, yet we find equatorial fo rms side by side with boreal ones at the north; and that although the furry foot of the lynx and ermine, and the feathery one of the day owl, the winter falcon, the ptarmigan, and grouse, are the true livery of the north, yet the shrews with satin coats and naked needle-like legs brave cold 20° below zero, and the red squirrel sports with naked palms on snow of similar temperature. Passing by the three orders *Edentata*, *Solidungula*, and *Pachydermata*, one of which, Solidungula, was represented by the horse, an introduced species on Sable Island, and there allowed to assume the feral state, the author arrived at the *Ruminantia*, two genera of which only exist in Nova Scotia—the cariboo or reindeer (*Rangifer cariboo*) and the moose (*Alces americanus*). He stated that the cariboo attain in Nova Scotias the enormous height of four feet ten inches; that the horns differs in every individual, but agrees in certain typical marks. In summer they are in colour rich brown, with white necks and shoulders; in winter, all soiled white; legs brownish, with white fringe on he hoofs extending to the back hoofs. They are seen in droves of seven or eight usually, and now and then of a hundred, but are fast diminishing; not, however, by the hand of man or teeth of wild beasts, but in that noiseless way wild creatures disappear as their range is contracted by new settlements; the does producing fewer fawns, and the males becoming early barren. Nova Scotia is the most southern latitude in which the cariboo is found on the American continent, but there is a "permanent variety," according to Richardson, one third the size of the southern form, with larger horns and no gall bladder, inhabiting the polar region. The President read a paper on the Diurnal Lepidoptera of Nova Scotia, being the second part of a series in process of delivery before the Institute. He remarked how visibly insect faunas differed according to the geological and botanical character of the districts visited by the entomologist, and more particularly alluded to the smaller size of certain insects inhabiting the extreme north-eastern portions of the American continent, compared with individuals of the same species taken farther south. This fact was first brought to his notice by the Rev. C. J. S. Bethune, secretary of the Entomological Society of Canada, three years ago, who while identifying a small collection of *Heterocera* taken in Nova Scotia, observed the smaller size of Nova Scotian forms when compared with those of Western Canada. Since that date the author has compared species of other orders with British types, and found a similar peculiarity, the British being larger than the Nova Scotian. This specific change is probably owing to the difference existing in the botanical character of these separate districts, which is not far removed from each other; but he hopes to be able to pay a second and longer visit to the valley of Annapolis and the slopes of the North Mountain during the coming summer.

### Vienna

Imperial Academy of Sciences, March 9.—Several memoirs were communicated, of which the titles only are given, namely, "On the conversion of formic acid into methylic alcohol," by MM. A. Lieben and A. Rossi, of Turin; "On the structure and development of the earliest plumage observed in the chicken," by Dr. E. Pernitza; "On the solution of algebraic equations of any degrees, even with complex co-efficients, by means of Gauss's scheme for complex magnitudes," by M. A. Raabe; and "On the heat equilibrium between polyatomic gaseous molecules," by Prof. L. Boltzmann. Two sealed papers were also deposited.—Dr. L. Fitzinger presented the sixth section of his critical revision of the family of the Bats (*Vespertiliones*), embracing the genera *Vespertilio* and *Myotis*.—Prof. R. Maly communicated the results of some investigations made in the chemical laboratory of the medical faculty at Innsbruck, including an analysis of the fluid from an ovarian cyst, made by himself, with investigations of the constituents of its ash, by Prof. E. Hofmann; a notice of Trommer's sugar-reaction in the urine, and of a simple mode of preparing muriate of creatinine from that fluid, by himself; and researches upon the bodies containing sulphur in the urine, by Dr. W. Löbisch.—Prof. von Hochstetter communicated some microscopic investigations on opals, by Dr. H. Behrens, in which the author states that most opals are mixtures of various minerals, including a colourless fundamental mass, containing (microscopically discoverable) hydrophane, cacholong, quartz, hydrated and anhydrous oxide of iron, ferriferous silicates, metallic sulphurets and carbonates, and organic substances:—fire-opal, glass-opal, noble-opal, and hyalite are free from admixture, and the first two are structureless. The colours of the noble-opal are interference-colours, caused by their lamellæ, which, however, are not tabular crystals. The double refraction discovered by Schultze in hyalite is caused by differences of elasticity such as occur in dextrin, amber, and compressed glass. The author also noticed the spheroidal structure which frequently occurs in opals.—A memoir on the circum-anal glands of man, by Dr. Gay, of Kasan, was presented by Prof. Brücke. The author describes these glands as having the greatest similarity to the large sudorific glands of the axillary cavity.—Dr. Tschermak presented three memoirs, namely, an analysis of the meteoric iron from the desert of Atacama, by Prof. E. Ludwig, as a further demonstration of its similarity to the meteoric iron of Jewell Hill; a notice of the microscopic constitution of the Lavas of Aden, by M. J. Niedzwiedski, who distinguished three species of rocks:—an obsidian containing sanidine, a trachytic lava containing plagiocuase and algite, and a felspathic basalt; and a contribution of his own to the knowledge of salt-deposits, in which he refers especially to the deposit at Stassfurt, which consists of two stages (rock-salt and kieserite-carmallite), the upper of which appears to be wanting in other salt-deposits. The author notices the minerals sylvine, and kainite, which occur scattered in this upper stage at Stassfurt, and mentions their occurrence in the salt-deposit at Kalusy, in Galicia, and partially at Hallstadt, as indications of the upper stages. He also notices the crystalline forms of the kainite and sylvine of Kalusy, and of the kieserite of Hallstadt.—Prof. L. Ditscheiner presented a memoir on some new Talbotian phenomena of interference, describing the phenomena manifested in the spectrum when the object-glass of the telescope is half-covered with crystalline plates of various thickness, whilst two Nicol's prisms are placed before the fissure and before the eye-glass.—The same gentleman also communicated a paper on a simple apparatus for the production of complementary pairs of colours with Brücke's schistoscope, and a notice supplementary to his determinations of wave-lengths, published some years ago.—M. Franz presented a memoir on the theory of simultaneous substitutions in double and triple integrals; and M. Oskar Simony noticed three mathematical problems, one belonging to the integral calculus and the two others to algebraic analysis.—Prof. A. Bauer presented a memoir on some compounds of lead with other metals, in which he showed that lead combines both with palladium and with mercury to form definite chemical compounds, having the formulæ $Pd^3 Pb$ and $Hg^3Pb^2$. The same gentleman communicated a paper by M. J. Stingl, an analysis of rocks and spring deposits of the Teplitz thermal district.

PARIS

**Academie des Sciences, Morales et Politiques**, April 29.—The French Institute is divided into five branches, of which the Académie des Sciences is considered the senior. All the branches meet in the same hall on different days of the week; the meetings of the Académie des Sciences take place on the Monday, and those of the Académie des Sciences Morales on the Saturday; the other sittings are not public, and the three other sections do not issue a special periodical, although they keep regular records. The Académie des Sciences Morales was not less determined than its elder brother to maintain its sittings, and they were not interrupted up to April 29. But the number of the members, which had been five for the sittings of the 15th and the 22nd, had diminished again to only three, which is the smallest for making a quorum. The presidency was given to M. Naudet, the senior member by age, who is close to his 88th year. The Académie des Sciences Morales et Politiques, which had been suppressed by Napoleon I. as being tainted with "ideology," was restored by Louis Philippe after the revolution of 1830, and M. Naudet is one of the original members, and was during many years perpetual secretary, resigning five years ago as being unable to fulfil the duties of his post. M. Leveque, one of the younger members, acted as perpetual secretary, and read over a short account of the proceedings of the last sitting. M. Pellat, the only third member present, sat on the benches, and held up his hand to approve the record. Then the reading of memoirs was proceeded with. A member of the Académie des Beaux Arts, whose name is not given to us, availed himself of the privilege granted to the academicians of every section, and took his seat by his colleague Pellat. The general public was represented by three persons. One of them was M. Mangin, a literary gentleman attached for years to the editorial staff of the *Patrie*. The *Journal Officiel* of the Commune took no notice of the proceedings, which were reported in the Versailles *Officiel*. It is very likely that the Académie des Sciences Morales et Politiques will be extinguished for the time by the Communist rule, and there is only a faint hope that the Académie des Sciences itself will be able to find the three members required for a quorum. But some academicians propose to advise the five academies to hold a general sitting every week, so that the chance may be increased.

## BOOKS RECEIVED

ENGLISH.—The Sub-Tropical Garden: W. Robinson (Murray).—Horses: their National Treatment, &c., by Amateur (Ballière, Tindall, and Cox).—The Builders of Babel: D. McCausland (R. Bentley).—The Meteoric Theory of Saturn's Rings: A. M. Davies (Longmans).—The Physiological Anatomy and Physiology of Man, vol. i. pt. 2: Todd, Bowman, and Beale (Longmans).

## PAMPHLETS RECEIVED

ENGLISH.—Report of the Observing Astronomical Society, Bristol.—Brown on the Throne.—Biology v. Theology, No. 2, by Julian.—Transactions of the Society of Engineers of Scotland.—Memoirs of the Geological Survey of Ireland, Nos. 104, 113, by G. H. Kinahan.—Report of the Rugby School Natural History Society for 1870.—First Annual Report of the Natural History Society of Derry.—A First Catechism of Botany, by John Gibbs.—On the Physics of Arctic Ice, by R. Brown.—Descriptions of some New Oaks, by R. Brown.—On Double Spectra, by W. M. Watts.—Proceedings of the Bath Natural History Field Club, vol. ii., No. 2.—The Gold Fields of Nova Scotia, by A. Heatherington.—Report of the Palestine Exploration Fund.—Science and Revelation, by R. P. Smith.—Materialistic Theories, by the Archbishop of York.—Transactions of the Clifton College Scientific Society.—Address delivered at the Anniversary Meeting of the Geological Society, by J. Prestwich.—On the Physiology and Pathology of the Lower Animals, by Dr. Lauder Lindsay.—The Historical Difficulties of the Old and New Testament, by Rev. G. Rawlinson.—Positivism: a Lecture by Rev. W. Jackson.—Transactions of the Scottish Arboricultural Society for 1870.

AMERICAN AND COLONIAL.—Third Annual Report of the Noxious and Beneficial Insects of the State of Missouri, by C. V. Riley.—Report of the Fruit-Growers' Association of Ontario.—On the Solar Corona, by Prof C. A. Young.—On a Method of Fixing Photography, and Exhibiting the Magnetic Spectra, by Dr. A. M. Mayer.

FOREIGN.—Plaidoyer en faveur de Paris: W. de Fonvielle.

## DIARY

*THURSDAY*, MAY 25.

ROYAL SOCIETY, at 8.30.—On the Temperature of the Earth as Indicated by Observations made during the Construction of the great Tunnel through the Alps: D. T. Ansted, F.R.S.—Some Remarks on the Mechanism of Respiration: F. Le Gros Clark.—On a New Instrument for Recording Minute Variations of Atmospheric Pressure: W. Whitehouse.—Note on the Spectrum of Uranus, and of Comet I., 1871: W. Huggins, F.R.S.
SOCIETY OF ANTIQUARIES, at 8.30.—Ballot for the Election of Fellows.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall, F.R.S.

*FRIDAY*, MAY 26.

QUEKETT MICROSCOPICAL CLUB, at 8.
ROYAL INSTITUTION, at 9.—On Bishop Berkeley and the Metaphysics of Sensation: Prof. Huxley, F.R.S.

*SATURDAY*, MAY 27.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.
ROYAL INSTITUTION, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

*MONDAY*, MAY 29.

ANTHROPOLOGICAL INSTITUTE, at 8.—On the Quissama Tribe of Angola: F. G. H. Price.—On the Races of Patagonia: Capt. Musters.—On Chinese Burials: Dr. Eatwell.

*TUESDAY*, MAY 30.

ROYAL INSTITUTION, at 3.—On the Principle of Least Action in Nature: Rev. Prof. Haughton.

*WEDNESDAY*, MAY 31.

SOCIETY OF ARTS, at 8.—On the Employment of Women: Mrs. Grey.

*THURSDAY*, JUNE 1.

LINNEAN SOCIETY, at 3.
CHEMICAL SOCIETY, at 8.—On Ozone: Dr. Debus, F.R.S.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall, F.R.S.

## CONTENTS



ERRATA.—Vol. iv. p. 40, in Table of Contents, for "Lieut. S. P. Oliver, R.N." read "Lieut. S. P. Oliver, R.A."; p. 45, 2nd column, line 16, for "slow enough to exceed" read "slow enough not to exceed"; p. 47, 1st column, line 28, for "December 29" read "December 15"; same column, 2nd line of footnotes, for "1871" read "1870."

THURSDAY, JUNE 1, 1871

## *SCIENCE LECTURES FOR THE PEOPLE*

IT is the great weakness of Science in this country that its professors are rather a mass of incoherent units than an organised body eager to influence others and themselves enjoying the privileges of such influence.

Each one is apt to work too much by himself, and while he often exhibits the most rare skill in discovering truth, he too frequently leaves to others less able than he the task of bringing his labours before the world at large.

Now, while the man of science complains with much justice that his pursuits have not been recognised by the rulers of our country, he ought not to forget that it is likewise his duty to help others, in doing which he will help himself. Whatever be the faults of our rulers, they are eminently sensitive to public opinion; men of science, therefore, have only to prove to the people that they are a useful class in order to have their services recognised. It is really absurd to suppose that one of the most intelligent and useful bodies of men in this country could not obtain their just demands if they set themselves earnestly and unitedly to the task. They have hitherto tried to prove to our rulers that the promotion of science will benefit the country, but have met with only indifferent success; let them supplement their endeavours by convincing our rulers that to promote it will be for their own benefit, and they are sure to succeed. Success, in fine, will not be attained by a policy of isolation, but by leavening the whole mass of the community with the love of science, and when this is done science will rise to its just place in the councils of the nation.

Many of its chiefs have now begun to perceive this, and we are glad to record the success of one of the best organised attempts that have hitherto been made to extend the knowledge and love of science among the working classes. The Science Lectures for the People, lately delivered in Manchester, have been a very great success, whether we regard the numbers who attended them, or the standing of the lecturers, many of whom came from a considerable distance in order to give their information to the people of Manchester. In the cheap and simple form in which these lectures are now published they constitute an eminently readable and instructive book, suitable for all classes. The titles of the lectures are as follows: (1) Coral and Coral Reefs, by Prof. Huxley; (2 and 3) Spectrum Analysis, by Prof. Roscoe and Dr. Huggins; (4) Coal, by Mr. Dawkins; (5) Charles Dickens, by Prof. Ward; (6) The Natural History of Paving Stones, by Prof. Williamson; (7) Temperature and Life of the Deep Sea, by Dr. Carpenter; (8) Formation of Coal Strata, by Mr. Green; (9) The Sun, by Mr. Lockyer. We are much indebted to Dr. Roscoe for arranging this admirable series of lectures, and also to Mr. T. J. P. Jodrell, who has generously defrayed the heavy expenses connected with their publication. Surely, too, the men of Manchester owe a debt of gratitude to Dr. Roscoe and his friends for this intellectual feast, the elements of which are at once so excellent and so varied. It would be presumptuous in any one man to criticise such lectures, but let it be said no more that the chiefs of science are either unable or unwilling to explain to others the discoveries which they themselves have made. They are at last emerging from their seclusion, and recognise their functions as teachers of truth. "A people," says Dr. Roscoe, "whose masses are without knowledge and without tastes for higher things than the mere struggle for existence can come to no good." These are truthful and noble words, and point to what ought to be the future action of men of science. Their author, we learn, is constantly asked about science lectures, and he thinks that if there were the means of sending lecturers to various localities they might be of the greatest value. But to do this a common action is necessary; for it is surely too much to expect that each large town should independently obtain such lecturers, and publish such a volume as that now under review. Indeed, the question is a more important one than at first sight appears; for a national society, formed with the view of diffusing scientific information among the populace of large towns, would be the beginning of a powerful union capable of forcing the claims of science before the Government of the country. Most of the leaders of science are disposed to admit that such a union is desirable, but many of them object to the formation of a new body. For, curiously enough, in matters of administration we are all of us evolutionists, and dislike very much the appearance of a new organisation that has not been developed by insensible degrees from some previous organisation of a humble character and living under other conditions.

Now, such a nucleus exists at Manchester; and as the necessity for an extended union of scientific men is strongly felt, might it not be desirable to extend the Manchester organisation into one for supplying the scientific wants of the whole community?

We make this suggestion with the view of eliciting the general opinion of the scientific public. This is a transitional age, and the social elements around us appear to be ripe for such a transformation.

## *CROOKES'S CHEMICAL ANALYSIS*

*Select Methods in Chemical Analysis (Chiefly Inorganic).* By William Crookes, F.R.S. Illustrated by twenty-two woodcuts. Pp. 468. (London: Longmans and Co.)

THE title of this book fails to convey any adequate idea of its true province. It is not a mere text-book of quantitative analysis after the manner of Fresenius; nor is it, as one might be inclined to suppose, a collection simply of analytical examples designed to illustrate to students the more important determinative methods, as in the well-known and deservedly appreciated "Handbuch" of Wöhler. It aims rather at being a laboratory *Vade-mecum*—a sort of "Chemists' Constant Companion"—designed alike for the teacher and the taught. It presents in a remarkably clear and well-arranged manner a number of thoroughly reliable methods of analysis—some original, others modifications of older and well-known methods—of which the greater portion have been rigidly tested by the author in his own laboratory. Every working chemist must have repeatedly felt the need of a book such as this, and in its publication Mr. Crookes has rendered an important service to the analytical chemistry of the day. The author's connection with the *Chemical News* has doubtless afforded him great and peculiar advantages in the compilation of the materials for his work; indeed, we notice that not a few of the most valuable processes he describes have already appeared in that journal.

Not the least admirable feature in the book (and herein it differs from the ordinary run of quantitative manuals) is the prominence given to methods for detecting and estimating the so-called "rare" metals. Thus we have methods given for the extraction and quantitative separation of lithium, cæsium, and rubidium; cerium, lanthanum, and didymium; glucinum and yttrium, &c. Bunsen's method of analysing platinum ores is also fully described. The discoverer of thallium may justly say that if investigators were more in the habit of looking for the "rare" elements, they would doubtless turn up unexpectedly in many minerals.

But for fear that some "practical man" has already made up his mind about the character of this book, we hasten to say that by far the larger portion of it is devoted to the analytical processes connected with the more important metals and their ores. The sections on iron and copper are particularly complete, all the newest and best methods being minutely described. We may instance Matthiessen's process for the preparation of pure iron, the Mansfield method of copper assay, and Meunier's methods for the immediate analysis of meteoric iron. Under the article "Soda-ash," attention is very properly directed to the absurdity practised in the alkali trade of at one time using the old atomic weight (24) of sodium, and at another the real number in stating the value of soda-compounds, the manufacturer invoicing the strength of his ash in accordance with the basis of calculation which he knows will be employed in reporting on its quality. This custom in many cases amounts to a positive abuse, and is a constant source of vexatious complaint between buyer and seller. The author quotes an instance in which soda-ash of identical quality has been known to be invoiced, part to one customer as containing 48 per cent., part to another as 49 per cent., and part to a third as 50 per cent.; the actual percentage being $48\frac{1}{4}$; the separate consignments being reported also of these different strengths by the analysts in the different towns to which the goods were sent. Inasmuch as soda-ash is usually valued at so much per cent., this amounts to a fraud on the purchaser. Surely the Alkali Manufacturers' Association would consult their real interest by putting an end to such a petty sham as this. But after all this is only on a par with the iniquitous system of "high" and "low" analyses, which is a scandal to the chemistry of the day. *Apropos* of this we would add that those interested in the agitation which has arisen in certain chemical quarters respecting the proper methods for estimating superphosphates and phosphoric acid generally, will find ample details in this work of the reliable processes which have hitherto been devised to that end. Under the head of carbon the method of assaying animal charcoal is described, and this includes an account of Schiebler's calcimeter and the mode of using it; the account of Heinrich's inquiry into the methods for the proximate analysis of coal, which is here introduced, together with the blowpipe assay, constitute one of the most complete things in the book.

In the midst of so much that is excellent it may seem invidious to seek for real or supposed omissions, but we venture to think that a few addenda in a future edition would add to the value of the book. For example, Bunsen's method of preparing pure platinum tetrachloride from scrap platinum, and his simple and expeditious method of recovering this metal from the residues in the process, might be an advantageous addition to the condensed description by Messrs. Teschemacher and Denham Smith of the ordinary method of estimating potash. Without doubt not a few of the errors to which a potash determination made by this method is liable are due to the use of impure platinum. A chapter devoted to the description of useful or improved forms of apparatus, such as the new filter-pump, would also form a valuable addition. Still we must not forget to add that Dr. Carmichael's neat and ingenious method of analysis receives its due share of attention. Nor do we see the reasonableness of making the selection of methods strictly inorganic; for surely the modifications in the process of ultimate analysis introduced by Mr. Warren cannot constitute the sum of the improvements in organic analysis, proximate and ultimate, which have come under the author's notice.

But we have said enough to indicate the character and scope of this work, and imperfect as our sketch is, it will at least serve to show that the book admits of very general application. It will doubtless attain to the popularity which it merits, and the chemical community will thank the author for the worthy contribution he has rendered to its literature.

T. E. THORPE

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Sun

IN NATURE for May 18, I find a review by Prof. Newcomb of my book on the Sun, and I beg leave to give some explanations respecting it. It is a pity that Mr. Newcomb has received my book only so lately (about one year after the printing was finished). On this account the criticism which he makes respecting spectrum analysis and the defects of the work on that head is entirely admissible; but this is no fault of the author. Since the book was written sixteen months have elapsed, and during this time great progress has been made in this branch of science, and nobody knows it better than myself, since I find so many things to add to that chapter.

But the criticism seems not so just when he reproaches the work with appearing to undertake to spare the reader the trouble of having recourse to elementary books. At the time of writing, neither of the valuable books which we have now by Roscoe and Schellen were published or had reached me, and the common treatises are very scanty in this respect, so that it was indispensable then, and perhaps even now, to indicate to the general reader the principles of the new science.

In the rapid progress of discovery, a book becomes old very soon, and a reviewer must not forget that printing requires time, and that this work appeared at the very moment of the breaking out of the war, so that it was shut up in Paris for almost another six months.

But a more serious criticism is that which refers to the temperature of the sun. As this is a repetition of M. Zöllner's critique, I beg to give some explanation. It is true that I have assumed that radiation is proportional to temperature without regard to the law of Dulong and Petit, and to the condition of the surface of the body. I did not, however, ignore that law, and I have taken it into account when speaking of Prof. Kirchhoff's discoveries. But here it was quite out of place to refer to it. Indeed, the consequence of this law is that a body may have a very high temperature, and yet radiate but very little; but the law does not state that a body may radiate more than its own temperature allows. When we judge, therefore, of the temperature from the radiation, we certainly commit an error; but so we always judge the temperature from one part of the effect that it is capable of producing; and taking into account the law of Dulong, we should find even a higher temperature in reality in the radiant body, as is the case with the gases.

The conclusion, therefore, to which I have arrived, after Mr. Waterston, is, I think, by no means-excessive, but if there is an objection possible to be made, it is exactly in the direction opposite to that of my reviewer. Certainly this conclusion is at variance with that of M. Zöllner, but it agrees with the results of other observers. This high temperature besides is really a virtual temperature, as it is the amount of radiation received from all the transparent strata of the solar envelope, and this body at the outer shell must certainly be at a lower temperature.

But this does not prove the incorrectness of my proposition that a thermometer dipped inside the solar envelope in contact with the photosphere, would indicate the enormous temperature that Mr. Waterston has found for the first time.

P. A. SECCHI,
Director of the R. College Observatory

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WILL you permit me to make a few comments on Prof. Newcomb's review of my treatise upon the Sun.

Soon after the work had appeared, I was informed that the account I had given of Mr. Stone's treatment of the transit observations in 1769 was not such as Prof. Newcomb would admit to be just. Knowing how much attention Prof. Newcomb has given to this subject, and his great skill as a mathematician, I was prepared to learn that I had misapprehended some points of the discussion between himself and Mr. Stone. I do not even now know to what specific statements of mine he objects; but he may rest assured that my sole object has been, and is, to give a just account of the matter. My account is in agreement with that given by Sir John Herschel, and by Admiral Manners when the Gold Medal of the Royal Astronomical Society was presented to Mr. Stone. I had also inferred from the nature of the discussions between Mr. Stone and M. Faye, and between Mr. Stone and Prof. Newcomb, that the truth lay much as in my narrative. At any rate, those who were present at the meetings of the Royal Astronomical Society in 1869 and 1870 will scarcely think that I have been led by any personal prejudices to advocate Mr. Stone's cause with undue favour, for Mr. Stone's strictures on some of my papers, and especially on papers relating to the subject of the transits of Venus, were severe even to bitterness.

I believe that Prof. Newcomb's mastery of this special subject is calculated to prevent him from rightly judging my treatment of it. He sees it from too near a stand-point, and therefore unduly enlarged. I am sure that on a careful reconsideration of the matter he will feel that I could not have given a fuller account of it than I have, without spoiling the symmetry of my book. Already a seventh part of the letter-press and more than a third of the appendix (besides three plates and twenty-four diagrams) have been given to the subject of the sun's distance. I do not think that more space could very well have been spared. It remains yet to be proved that a single statement in these pages is inaccurate. I deny confidently that the distortion of the limbs of Venus and Mercury in transit has been *proved* to be the product of insufficient optical power. Irradiation must produce such effects to a greater or less extent; and I renew "gravely" my proposal to measure the effect, whatever its cause or causes, during the next transit. I would remind Prof. Newcomb that every observer at Greenwich noticed the effect (more or less) during the transit of Mercury in 1868. Now the Greenwich instruments are not commonly supposed to be utterly imperfect, nor the Greenwich observers wholly unskilled. Even if we admitted both these points, I should still adhere to my proposal. For I have shown in the Monthly Notices of the Royal Astronomical Society for 1870, that those two observations which differed most widely are brought into perfect agreement when the relative breadth of the black ligament observed in each case is taken into account.

Prof. Newcomb has misunderstood my remarks about the D line in the spectrum. I have never concluded, and certainly I have nowhere stated, that "the light of the sodium lines proper is reduced." What I have pointed out is that where those lines fall on the spectrum of the electric light, and where, therefore, we should expect an increase of light, there seems to result darkness. In p. 118 I am careful to use in one place the word *seem*, in another the word *appear*.

I must remind Prof. Newcomb that three countrymen of his own, Professors Harkness, Curtis, and (quite recently) Young, have supposed, with me, that the theory *has* been maintained, that the light of the corona is due to sunlight directly illuminating our atmosphere, and that they and Mr. Baxendell have opposed that theory as pointedly as I have.

I have, however, to admit that some passages "indicating personal feeling" would have been better—much better—omitted. I should have remembered that the explanation of such personal feeling would be unknown to most of my readers. Those who know that because I advocated opinions respecting the corona, which are now all but universally admitted to be just, I was spoken of as "simply making myself ridiculous," will at least acquit me of responding as rudely as I had been attacked. But the generality of my readers had heard nothing of this and other assaults upon me.

I take this opportunity of noting that Dr. Armstrong, of the London Institution, has shown me that in my account of the researches of Mr. Lockyer and Dr. Frankland I have not done the former justice. Some alterations must be made also in my narrative of the work of Dr. De la Rue and P. Secchi in Spain in 1860; much more of the credit of the results then obtained being due to Dr. De la Rue than I had judged from the narrative in P. Secchi's "Le Soleil." Also the enunciation of the aurora theory of the corona must be assigned to Prof. Norton of America.

RICHARD A. PROCTOR

[With respect to the penultimate paragraph of the above letter, we need only refer to our own comments on two previous letters from Mr. Proctor, under date July 7 and August 4, 1870, which we now reprint. ED.:—"For an accurate though incomplete statement of Dr. Frankland's and Mr. Lockyer's theory of the Corona, we refer our readers to the first number of NATURE. Many of them will not be surprised to find that it is *not* what Mr. Proctor states it to be. Dr. Frankland and Mr. Lockyer, from their laboratory experiments, have shown that the pressure at the base of the chromosphere is small, and they have therefore stated that it is scarcely possible that a very extensive atmosphere lies outside the chromosphere. Mr. Lockyer has shown, moreover, that the height of the chromosphere as seen by the new method probably falls far short of its real height as seen during an eclipse as it was seen by Dr. Gould. A reference to the same number of this journal will also show that Mr. Proctor has misrepresented Dr. Gould's statements, which endorse the idea put forward by Dr. Frankland and Mr. Lockyer. Dr. Gould has expressly stated 'that there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon.' This is an opinion held by Faye and other distinguished astronomers, and Mr. Lockyer has simply shown that should this turn out to be the case, the continuous spectrum observed may be explained. Astronomers did not require Mr. Proctor to tell them what he has recently been enforcing; but, more modest than he, they have been waiting for facts, and Mr. Proctor surely is old enough to see that by attempting to evolve the secrets of the universe, about which the workers speak doubtfully, out of the depths of his moral consciousness, he simply makes himself ridiculous, and spoils much of the good work he is doing in popularising the science."—"Still holding to our comments, we gladly state that they were not written in the spirit in which Mr. Proctor has read them. He is known to all as an astronomical worker, and our objection to his mathematical result was that it was based upon data among which the principal point at issue was accepted as proved."]

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## Rain after Fire

IN Paris, on Wednesday the 24th inst., after describing the terrible conflagrations, one of the correspondents writes thus:—

"A more lovely day it would be impossible to imagine, a sky of unusual brightness, blue as the clearest ever seen, a sun of surpassing brilliancy even for Paris, scarcely a breath of wind to ruffle the Seine. Such of the great buildings as the spreading conflagration has not reached stand in the clearest relief as they are seen for probably the last time; but in a dozen spots, at both sides of the bridges, sheets of flame and awful volumes of smoke rise to the sky and positively obscure the light of the sun. I am making these notes on the Trocadéro. Close and immediately opposite to me is the Invalides, with its gilded dome shining brightly as ever."

Another as follows:—"As I drive along the green margin of the placid Seine to St. Denis, the spectacle which the capital presents is one never to be forgotten. On its white houses the sun still smiles; he will not refuse his beams spite of the deeds which they illumine. But up through the sunbeams struggle and surge ghastly swart waves and folds and pillars of dense smoke; not one or two, but I reckon them on my fingers till I lose the count."

Twenty-four hours later, the change has come. "The rain is now falling heavily, has been falling heavily all day, and may do something for burning Paris. The sound of artillery has died away;" and from another writer:—"A heavy smoke hangs over Paris and rain is constantly falling."

I believe it has often been remarked that rain generally follows a heavy cannonading, but in this case there is an almost unexampled artillery fire and tremendous conflagration at the same time, accompanied by a sudden and violent change in the atmospheric conditions. From where I am writing we noticed a remarkable change on Thursday morning, and about 2 P.M., after intense closeness and oppression, a rain of a tropical character set in for twelve hours or more. On many occasions in Queensland, I noticed that in seasons of drought, after extensive grass fires, causing intense heat, heavy thunderstorms generally followed.

GEORGE PEARCE SEROCOLD

Rodborough Lodge, Stroud, May 27

## Alleged Daylight Auroras

SEVERAL letters having appeared in recent numbers of NATURE, giving what the writers consider to have been undoubted instances of aurora visible in the daytime, you will, I hope, allow me to state the reasons why I still adhere to the views expressed in my former communication on this subject.*

And, first of all, I must beg your correspondent Mr. Jeremiah not to think me uncourteous if I dismiss at once, as unworthy of serious criticism, the cases which he has dug out of monkish chronicles. It is likely enough that some of these old records may be imaginative descriptions of nocturnal auroras, and as such they are not without interest, but I cannot admit them as competent witnesses on a point of nicety.

A more modern instance adduced by the same correspondent will be found at p. 7 of NATURE for May 4, under the title "Aurora Borealis, seen in the daytime at Canonmills." In this case it is difficult to know what relation is intended between the title and the account which follows. The account describes the clearing off of the clouds in a mass from the north-west, with the production of an "azure arch," the centre of which "reached an elevation of 20°." If I reply to this that the clearing off of clouds is not an aurora, even though they clear off in a compact body from the north-west, leaving an "azure arch," I may be met by the rejoinder that nobody said it was; and yet I strongly suspect that the writer had some confused idea that he was describing an auroral arch, and I am certain that nine out of ten readers, misled by the heading, would take the same view. Stripped of the cloud-phenomena, all that remains of the Canonmills aurora is the appearance of some "very faint perpendicular streaks of a sort of milky light," which could be traced across the segment of blue sky, but were "extremely slight and evanescent." Considering the probability that the observer regarded the cloud-arch as auroral, which it certainly was not, and considering how his judgment would be likely to be biassed by that idea in the interpretation of "extremely slight and evanescent" appearances, I think we may fairly regard this testimony as particularly weak.

In NATURE for Dec. 8, 1870, Mr. Cubitt describes and figures a double auroral arc which he saw in broad daylight on the 25th October. It was "some 25° above the horizon, and almost due east." In my first letter I expressed a doubt of the correctness of this observation on the ground that auroral arcs are not seen in the east. My criticism has since been challenged on two distinct issues. Mr. Jeremiah insists that an auroral arc may *extend* towards the east, and that what Mr. Cubitt saw may have been the eastern extremity of a northern arc. A reference to Mr. Cubitt's letter and illustration will show at once that if what he saw was any part of an arc, it was the apex and not an extremity. But another correspondent, Mr. Reeks, in NATURE of Dec. 29, 1870, in criticising my remark, makes a statement which is more difficult to answer. He affirms positively that in Newfoundland he has many times seen the arch nearly due east, that is, as he explains, with "the extremities pointing N.N.W. and S.S.E." I would suggest, however, in reply to this statement, that in an extensive auroral display there may be fictitious arches, produced by the accidental correspondence of streamers on either side of the "cupola." An arch of this kind may easily extend from N.N.W. to S.S.E., spanning the entire heavens. It is essentially different from the true auroral arc, which, until much stronger evidence to the contrary is adduced, I shall still believe to be invariably transverse to the magnetic meridian. Obviously, Mr. Cubitt's arc was not of the kind that Mr. Reeks describes.

I pass on to a record of daylight aurora, which, more than any other that I have seen, demands a careful investigation. I refer to "An Account of an Aurora Borealis seen in full Sunshine, by the Rev. Henry Ussher, D.D.," said to be taken from the Transactions of the Royal Irish Academy for 1788, and quoted by the Rev. T. W. Webb in NATURE for May 11. Dr. Ussher's account, it must be admitted, is most particular and complete. He describes "whitish rays ascending from every part of the horizon, all tending to the pole of the dipping needle, where, at their union, they formed a small thin and white canopy similar to the luminous one exhibited by an aurora at night." Nothing can be more precise. But is it not also a trifle too wonderful? Surely, if any part of an aurora is to be seen by daylight, it must be just one here and there of the most vivid beams. That the whole phenomenon should be visible at noon-day in all its completeness, just as at night, even to the faint extremities of the streamers in the magnetic zenith, is to my mind so entirely inconceivable that not even the authority of a doctor of divinity can command my faith in it. I can much more easily believe that the sky presented a remarkably symmetrical arrangement of radiating cirri, and that the observer, impressed by the recollection of the aurora of the previous evening, persuaded himself that the "rays coruscated from the horizon to their point of union." The confirmation by "three different people" is of little value unless their observations were independent.

To those who have no clear conception of the difference between cirrus and aurora, the foregoing arguments will be meaningless. Some persons write very loosely of "luminous cirri," and I have even seen described the transformation of cirrus cloud into aurora as it grew dark. I believe that there is no connection between the two phenomena beyond an occasional and purely accidental similarity of form, and that when the two co-exist, the cirrus, instead of being the seat of the aurora or deriving luminosity from it, only serves to obscure its brightness, and, if dense enough, may appear in the form of dark bands across the auroral light, the latter being, as I conceive, at a very much greater elevation.

I adverted in my former letter to the argument that may be drawn from the non-visibility in the day-time of other lights comparable with the aurora, and I will only now add the following suggestion. If the auroras that occur in this country are occasionally visible in daylight, it might be supposed that the much grander displays of the Arctic regions would be habitually visible in daylight. But is the fact so?

Clifton, May 23

GEORGE F. BURDER

* NATURE, vol. iii. p. 126.

## Aurora Australis

TRAVERSING the Indian Ocean 44° S. 65° E., I observed, September 24th, 1870, 4h. till 13h. Greenwich time, a south polar light of great intensity and splendour. After my arrival at Manado (Celebes) I was just writing a few lines about it for the readers of NATURE, with the purpose of knowing whether at the same time an aurora, or at least disturbance of the magnetic needle, had been observed on the northern hemisphere, when I saw in NATURE (Nos. 49, 50, and 51, 1870), several interesting descriptions of aurora borealis observed September 24 in England, &c. I am not aware whether many observations of southern polar lights have been recorded, but I remember that those which Cook described in the year 1773 were coincident with aurora borealis observed in Friesland, and others observed in 1783 at Rio Janeiro were coincident with polar lights in the northern hemisphere. At all events I believe that the attention of men of science is not sufficiently directed to this coincidence of northern and southern polar lights, at least not as much as it deserves in respect to the theory of polar lights at all; and I should be very glad if, in consequence of this notice, authorities would discuss this highly interesting phenomenon in NATURE.

I shall later, according to my diary, accurately describe the display of this splendid aurora australis, and mention the influence which it perhaps or probably had on the abnormal meteorological phenomena, which I observed during the succeeding days.

ADOLF BERNHARD MEYER

Manado (Celebes), January 9

P.S.—I beg to contribute to the records in NATURE of earthquakes, &c., over the whole globe:—

November 20, 1870, afternoon, an at first vertical, then horizontal, rather heavy shock at Manado.

January 28, 1871, 4h., a slight, very local shock in a part of Manado.

Manado (Celebes), March 5

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### The Eclipse Photographs

As an ardent and not inexperienced votary of photography, I am fully alive to the value of photographic evidence, and regard with enthusiasm each fresh victory which photography achieves, yet I cannot myself look with any very great degree of satisfaction upon the photographs of the late solar eclipse either as examples of photography or as evidence contributing to our knowledge of solar physics. In saying this I make no reflection whatever upon the ability or efforts of those by whom the pictures were produced. On the contrary, I am aware that when these pictures were taken the first grand requisite of photographic success—a clear view of the object to be represented—was scarcely to be obtained. Briefly; from a technical point of view, the pictures are of but indifferent definition, and the identity of the coronal rifts in the Cadiz and Syracuse photographs not satisfactorily conclusive, in addition to which in the picture by the American observers, the so-called coronal light extends a long way over the lunar disc, which seems to me to preclude the possibility of its being other than a phenomenon of terrestrial meteorology. A few weeks ago, when the sky appeared almost cloudless, I observed a beautiful lunar halo, very much resembling the so-called corona, which I apprehend no one would attribute to anything but atmospheric moisture. Why, then, in the instance of a sky burdened with innumerable clouds, should we attribute the halo of light surrounding the solar disc to other than atmospheric causes, even though there should be something which might be mistaken for a coincidence in two distinct photographs of one or other of the rifts which were characteristic of that halo?

Manchester, May 26 D. WINSTANLEY

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### Eozöon Canadense

PERMIT me to state that the presumed "important bearing" on the so-called "Eozöon Canadense," of the principal fact noticed in the communication entitled Palæozoic Crinoids, which appears in NATURE of May 25th, is discussed in a paper by Dr. Rowney and myself, contained in the forthcoming number of the Proceedings of the Royal Irish Academy, now on the eve of publication. The paper referred to is a reply to the articles by Drs. Dawson and Sterry Hunt, which appeared in the last (second) number of the Proceedings.

Glenoir, near Galway, May 29 WILLIAM KING

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WITHOUT going into the vexed question as to whether Eozöon Canadense is or is not of organic origin, I may be permitted to express some surprise at the new, and, to say the least of it, startling theory broached by Mr. Perry in last week's NATURE, of the vaporous formation of a certain limestone. The only facts brought forward in support of this view are, its occupying pockets, its foliations, and its conformation with irregularities of surface in the pre-existing rock. All these could be as well accounted for on the supposition of deposition from aqueous solution, without doing violence to the fact that carbonate of lime is not volatile at any temperature. E. T. H.

## *THE INEQUALITIES OF THE MOON'S MOTION*

THE following is an abstract of the method of computing the inequalities in the motion of the moon which are due to the action of the planets, proposed by Prof. Newcomb in the paper presented to the Academy of Sciences of Paris on April 3.

When we consider the movements of the sun, moon, and earth, under the sole influence of their mutual attraction, the position of each of these three bodies in space will be given in terms of eighteen arbitrary constants, and of the time. The problems of the relative movement of the moon around the earth, and of the movement of the centre of gravity of the earth and moon around the sun, have been solved with a degree of approximation sufficient at least for the purposes of astronomy. Thus, we have the co-ordinates of any two bodies relatively to a third, or relatively to the centre of gravity of the system, in terms of twelve elements and of the time. It only remains to add the expressions for the uniform movement of the centre of gravity in a straight line, to have the general expressions for the co-ordinates of each body.

We have then only to consider the action of the planet to vary the eighteen elements according to the method of Lagrange, to have the movements of each of the three bodies under the influence of the attraction of the planet. Unfortunately, the expressions thus obtained are at first extremely complicated. We have to compute a coefficient corresponding to each combination of the elements taken two and two. The entire number of the coefficients is, therefore, $\frac{17 \times 18}{2} = 153$. And each coefficient contains eighteen products of the partial differential coefficients of the co-ordinates of the three bodies relatively to the elements. These latter differential coefficients are so complex that the formation of any one product would be a considerable labour. The direct formation of the coefficients required is therefore impossible. The paper in question is principally devoted to an explanation of the simplifications which may be introduced into the problem.

It is first shown that all the coefficients formed by combining any one of the six elements which fix the position of the centre of gravity with any of the twelve elements of the relative motion, vanish identically, while the combinations of those six elements with each other give only the principle of the conservation of the centre of gravity. This leaves only sixty-six combinations. It is then shown that, if the elements are divided into two classes, the first class being the mean longitudes, the longitudes of the perigees, and the longitudes of the nodes of the sun and moon, and the second the mean distances, eccentricities, and inclinations, the coefficients vanish whenever the two elements combined belong to the same class. The number of coefficients is thus reduced to thirty-six, and they are simply the differential coefficients of six functions of the elements of the second class. These functions are formed an extremely simple process when we have the rectangular co-ordinates expressed as functions of the elements and the time.

The remainder of the process is simply one of the development of a very complex perturbative function, and is of no especial interest.

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## *THE HELIOTYPE PROCESS*

AT one of the recent *soirées* of the Royal Society given by General Sabine at Burlington House, Messrs. Edwards and Kidd exhibited at work the new heliotype process, whereby photographic pictures can be very rapidly copied in by the aid of the printing-press. The process is very inexpensive, and so rapid that if one of the pages of NATURE were sent to the works, it could be copied by photography, and within two or three hours after receipt, pictures could be turned out as fast as the printing-press could work them off. A few days ago I went over the works to examine the process, and a gentleman, who brought an engraving to the proprietors just as I arrived, saw the press printing off very good copies before I left, the interval being about two hours. The works are at some distance out of London, free from the smoke and dust.

The following is an outline of the history of the process:—Mr. Mungo-Ponton, of Clifton, discovered some years ago that if a dried film of gelatine and bichromate of potash be exposed to light, the film is afterwards insoluble in warm water. M. Poitevin afterwards noticed that where light had acted upon such a film, it took greasy ink just like a lithographic stone, whereas those parts on which light had not acted, absorbed water. In the attempt to produce pictures on this principle, he poured a mixture of warm gelatine and bichromate of potash over a lithographic stone, or plate of metal, and when the film was dry he exposed it to light under a negative. Where the light had acted the film became waterproof, and where it had not acted the gelatine swelled up like a sponge. This surface of hills and valleys prevented him from getting good pictures when he attempted to print from it on the lithographic principle.

Messrs. Tesse du Motay and Marechal tried the process just mentioned, and by carefully selecting their subjects, choosing those only in which there was little contrast of light and shade, they reduced the elevations and depressions on the surface of the film to a minimum, and thus obtained some very fair pictures, but after a very few had been printed off, the gelatine printing surface broke up. The next man who took up the process was Albert of Munich. Before his time, whenever a sufficiently thick film of gelatine to stand wear and tear had been used, the elevations and depressions were so great that the film could not be inked. Albert took a plate of glass about half an inch thick, covered it with a thick coating of bichromated gelatine, and after it was dry exposed it all over to light to make it insoluble. Afterwards he covered the surface thus prepared with a very thin coating of sensitive gelatine, on which the picture was printed from the negative. By this process he obtained some exceedingly beautiful and perfect pictures, and he is producing them by this plan at the present time.

Mr. Ernest Edwards took up the process at this point about a year ago. He made a thick leathery film at the outset by adding alum to the warm gelatine solution. He found that films so prepared still retained the lithographic-stone-like property; they will scarcely swell up in water at all. They are insoluble, and they resist the wear and tear of the printing-press very satisfactorily.

The working details of the heliotype process are as follows. The films are prepared upon large sheets of accurately levelled finely ground glass, technically known as "greyed glass"; about 22 inches by 18 inches is a convenient size. The surface of the glass is first polished by means of a clean piece of rag, with a little solution of wax in ether; the exceedingly thin film of wax thus left upon the glass permits the dried gelatine film to come off easily. The glass plates after being waxed are levelled, and then a measured quantity of a warm mixture of gelatine, bichromate of potash, chrome alum, and water, is poured upon each plate from a jug with a piece of muslin tied over its mouth. The temperature of the solution in the jug is about 150° Fahrenheit, and after it is poured over the plate it sets in a very few minutes, but it requires a much longer time to dry. Curiously enough, until it is dry it is not sensitive to light; this fact was found out accidentally, for at first this part of the operations was carefully carried on in yellow light.

After the film has set, the plates are taken into a dark room to dry. If any of the fumes given off by burning gas escape into this room, they act upon the film just as light would do, therefore although a gas stove is used to dry the plates, the products of combustion are very carefully carried off. The gas stove used in the works was invented by Mr. George, a dancing master at Kilburn. It is a closed iron cylinder, into which air is admitted by one pipe coming from outside the house, and the products of combustion are carried off by another. A third iron air pipe enters the bottom of the stove, curves round its sides in a spiral, and then emerges through the iron plate forming the top. Air from outside the house is warmed in this spiral, after which it escapes into the drying-room, which is kept at a temperature of from 90° to 120°. At a temperature of 90° the films take about twenty-four hours to dry. As they dry they contract slightly, and thus separate themselves from the glass. These dried films are technically termed "skins"; they are of an orange colour, and about one-tenth of an inch thick. The picture is printed on them from a negative, and a faintly visible image is formed; when this image is fully out the films are removed to a dark room.

Here each skin is floated in water, and caught upon the surface of a thick plate of zinc; a flat piece of wood, edged with india-rubber is then scraped with considerable pressure over the film, so as to squeeze out all the water between the skin and the zinc. As the film still continues to absorb moisture, it is thus fixed to the zinc with the whole pressure of the atmosphere. After this the zinc with its attached film is left for half-an-hour at least in a large vessel of water, for the superfluous bichromate of potash to soak out, and then the film is no longer sensitive to light. If the film be thus soaked for several hours, or even days, it does not suffer.

The film, upon its zinc plate, is now ready for the printing press. It is damped between each impression, just like a lithographic stone. Then it is inked, and the best roller for the purpose is found to be one made of india-rubber, backed inside with "india-rubber sponge" to give additional softness. Ordinary lithographic ink is used. If stiff lithographic ink be employed, the surface will only "bite" where light has acted most; if thin ink be used, the leathery surface will only bite in the half tones of the picture; hence each picture is produced by at least two inkings, and advantage is taken of this circumstance to use two colours, and get warm shades in the half tones. It is very interesting to see the picture gradually growing under the inking process. By this method double-printing is executed with a single pull at the press. Ordinary Albion hand printing presses are used.

The negatives worked from in this process have to be "reversed," and they may either be reversed at the time they are taken, or afterwards. In the former case, instead of the lens of the camera being pointed direct at the object or picture to be photographed, a mirror, silvered on its front surface, is interposed at an angle of 45°. Another method of reversal is to take an ordinary unvarnished negative, and coat it either with a solution of india-rubber, or a solution of gelatine and alum. When the film is dry the plate is accurately levelled; it is then coated with a pool of collodion as thick as it will hold, and this collodion is then allowed to dry. Next the film is cut through with a penknife near the edges of the picture, and the plate is placed in water, where the negative soon floats off the glass, after which it is dried between blotting paper. The flexible negatives thus obtained are very durable, except when bad india-rubber is used in reversing them.

When a batch of pictures has been printed from any particular skin, the film is taken off the zinc plate, and put away until wanted again. Mr. Edwards says the skins will stand a vast amount of wear and tear, and he showed me one from which he said 1,500 pictures had been printed, the last impression being as good as the first, and the skin ready for further work if necessary.

By this process many of Mr. Nasmyth's lunar pictures have been copied, and while on the premises I saw some work then being executed for Mr. Ruskin, and others known in the world of art and science. Bones, and some descriptions of anatomical specimens, are very easily photographed and printed by this process, which is also well adapted for landscapes and architectural subjects. If it be desired, a glaze is given to the finished prints in a very simple way. A little powdered magnesia is sprinkled over the surface of the print, and it is then placed on a smooth board and rubbed with a pad of flannel. Magnesia belongs to the soapstone family, and when used in this way it very readily gives a surface polish to paper.

WILLIAM H. HARRISON

## *PARIS NEWS*

FOR some time past the Académie des Inscriptions et Belles Lettres has held no regular sitting, almost all the members being refugees in Versailles or elsewhere. A special commission has been given to M. Rénan, one of its most distinguished members, to inspect the ruins of the Parisian monuments which have been destroyed by the Communists. M. Rénan, before publishing his last books on religious matters, had been sent to Mesopotamia to do the same work as Mr. Layard. Private letters received from the distinguished commissioner have been read before a group of members of different academical bodies at Versailles, sitting almost in an official capacity as the Academy for Inscriptions and Belles Lettres. It was stated that the Louvre buildings had escaped, and the bulk of artistic works will be saved from the conflagration. But the private Imperial Library in the old building of the Ministry of State has been destroyed. The value of this collection was chiefly historical, a number of the volumes being of special value from the fact of their having been presented to the several Kings and Emperors of France during the last three centuries. There were also some manuscripts of value, and collections of drawings for the study of art in the Museum. It was intended to open it shortly as a special art library for the use of students at the Louvre. The National Library, formed by Richelieu, was not burned down as has been rumoured; the building has entirely escaped. But it appears that steps had been taken by the insurgents to destroy it like the Serapion was at Alexandria when Omar took possession of the city. The Luxembourg buildings and Museum were saved only by the prompt exertions of the troops, when the insurgents were actually setting fire to them. The Luxembourg holds within its precincts a valuable library, where have been collected parliamentary documents from every nation and of every period. It was said to be the most valuable in the world in this respect. The collection of pictures is the richest in the world for works of living painters belonging to the French school. Courbet, the member of the Commune, had not been admitted to this, esteemed the highest honour by French artists. The Sorbonne is almost entirely saved, the walls only having been pierced by gun-shot or shells. The collections are most valuable, and very serviceable for students. We have no special news from Sainte Geneviève, a library largely used for law purposes on the Place du Panthéon; but it is supposed that the library is safe, as the insurgents were prevented from exploding it, though an immense quantity of powder had been deposited in the cellars, and it was used as an arsenal during the whole of both sieges. The Institute is safe, although it appears steps had been taken for its destruction. The Mazarine Library close to it is most valuable for works of the 17th and 18th century, as well as the library of the Arsenal. But according to every probability this last establishment will be entirely lost, owing to the vicinity of the *Grénier d'abondance*, a place where an immense number of goods were collected, and which was ignited. A commission of the Academy of Sciences will be issued to study the different processes used by the insurgents for burning the Tuileries, Palais Royal, &c. Hay, soaked with petroleum, appears to have been very often resorted to, as well as canisters full of the same substance. In some instances petroleum had been poured from outside into the cellars, and an ignited match thrown into the impregnated air. The stories of firemen throwing petroleum from fire-engines are, we are happy to say, unfounded.

## *DREDGING OF THE GULF STREAM*

WE are much gratified to learn from *Harper's Weekly* that preparations are now being made, under the direction of the Superintendent of the Coast Survey, for a very complete and thorough investigation of the deep-sea bottom, and especially of the channel of the Gulf Stream off the eastern coast of America, with an examination also of the Straits of Magellan and of a part of the Pacific Ocean. A steamer is now being built, which will shortly be launched, with the special object of continuing the deep-sea dredgings which, under the direction of Count Pourtalès, have given the Survey so much reputation.

It is expected that the arrangements will be completed by the end of August, and that the whole matter will be specially in charge of Prof. Agassiz, assisted by Count Pourtalès, whose experience eminently qualifies him for the post.

The plan of operations is, first, to run a line of dredging across the Gulf Stream between New York and Bermuda, and, if necessary, far enough eastward to completely cross the Gulf Stream current. The course will be thence to Trinidad, where a careful examination will be entered into to ascertain whether there is any difference in the deep-sea fauna of the adjacent waters and that of the coast of Florida. The expedition will then probably proceed to San Paulo for the purpose of examining the deepest known portion of the Atlantic, reaching to, at least, five thousand fathoms. From San Paulo it will again cut across the Brazilian current, and after possibly spending some time on the coast between Buenos Ayres and the Straits of Magellan will proceed by a zigzag course to the Falkland Islands, in the neighbourhood of which the expedition will remain for some time, for the purpose of solving certain important problems relating to both the deep-sea fauna and to that of the coast. It is next proposed to spend, at least, a month in the Straits of Magellan during the summer season of that portion of the globe. The work at the Straits being completed, the party expect to pass up along the western coast of Chili, next to the island of Juan Fernandez, and thence across to Callao. From this point the course will be to the Gallapagos, and thence across the Chilian current to some point on the west coast of Mexico—possibly to Mazatlan. The Revillagigedo Islands will next be visited, whence the party will proceed to San Francisco.

The entire exploration will probably occupy ten months, and bids fair to be the most important attempt ever made at determining the character of the fauna of the deep seas. The experience gained in all the former American and foreign expeditions of this kind will be freely used on this occasion; and no pains will be spared in the way of outfit to render the whole undertaking an entire success.

The fact that this expedition is under the direction of the Coast Survey is sufficient guarantee that nothing will be neglected to secure satisfactory results in the way of investigations upon the physics of the ocean, as well as its natural history, as it is intended to make use of the most approved apparatus for the determination of depths, temperatures, specific gravity, and chemical composition of the waters, &c.

## THE SPECTRUM OF URANUS *

IN the paper "On the Spectra of some of the Fixed Stars," † presented conjointly by Dr. Miller and myself to the Royal Society in 1864, we gave the results of our observations of the spectra of the planets Venus, Mars, Jupiter, and Saturn; but we found the light from Uranus and Neptune too faint to be satisfactorily examined with the spectroscope.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in my hands by the Royal Society, I have succeeded in making the observations described in this paper of the remarkable spectrum afforded by the light of the planet Uranus.

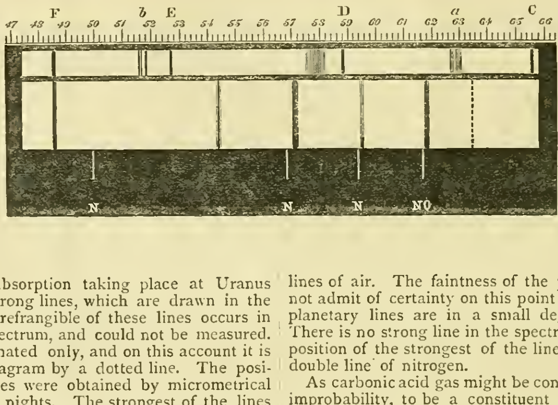
It should be stated that the spectrum of Uranus was observed by Father Secchi in 1869.‡ He says, "Le jaune y fait complétement défaut. Dans le vert et dans le bleu il y a deux raies très larges et très noires." He represents the band in the blue as less refrangible than F, and the one in the green as near E.

The spectrum of Uranus, as it appears in my instrument, is represented in the accompanying diagram. The narrow spectrum placed above that of Uranus gives the relative positions of the principal solar lines, and of the two strongest absorption-bands produced by our atmosphere, namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre.

The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from C to about G.

On account of the small amount of light received from this planet, I was not able to use a slit sufficiently narrow to bring out the Fraunhofer lines. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer and by direct comparison with the spectra of terrestrial substances.

The spectroscope was furnished with one prism of dense flint-glass, having a refracting angle of 60°, an observing telescope magnifying $5\frac{1}{2}$ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum.




The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least refrangible of these lines occurs in a faint part of the spectrum, and could not be measured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave length of about 544 millionths of a millimetre. The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others.

The measures taken of the most refrangible band showed that it was at, or very near, the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark, was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen.

Three of the bands were shown by the micrometer not to differ greatly in position from some of the bright lines of the spectrum of air. A direct comparison was made when the principal bright lines were found to have the positions, relatively to the lines of planetary absorption, which are shown in the diagram. The band which has a wave-length of about 572 millionths of a millimetre is less refrangible than the double line of nitrogen which occurs near it. The two planetary bands at 595 and 618 of the scale appeared very nearly coincident with bright lines of air. The faintness of the planet's spectrum did not admit of certainty on this point; I suspected that the planetary lines are in a small degree less refrangible. There is no strong line in the spectrum of Uranus in the position of the strongest of the lines of air, namely, the double line of nitrogen.

As carbonic acid gas might be considered, without much improbability, to be a constituent of the atmosphere of Uranus, I took measures with the same spectroscope of the principal group of bright lines which present themselves when the induction-spark is passed through this gas. The result was to show that the bands of Uranus cannot be ascribed to the absorption of this gas.

There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere.

WILLIAM HUGGINS

* From the Proceedings of the Royal Society.

† Phil. Trans. 1864, p. 413; and for Mars, Monthly Notices R. Ast. Soc. vol. xxvii., p. 178.

‡ Comptes Rendus, vol. lxviii. p. 761, and "Le Soleil," Paris, 1870, p. 354.

## NOTES

WE understand that the contributors to the next volume of the *Zoological Record* are as follows:—Mammalia, Reptilia, and Pisces, Dr. Albert Günther, F.R.S.; Aves, Mr. H. E. Dresser, F.Z.S., and Mr. R. B. Sharpe, F.L.S.; Mollusca, Molluscoidea, and Crustacea, Dr. Edward von Martens, F.M.Z.S.; Arachnida and Myriapoda, Mr. O. Pickard-Cambridge, C.M.Z.S.; Insecta generally and Coleoptera, Mr. E. C. Rye; Lepidoptera, Mr. W. F. Kirby; Diptera, G. A. Verrall; Neuroptera and Orthoptera, Mr. M'Lachlan; Rhynchota, Mr. John Scott; Vermes, Mr. E. Ray Lankester; Echinodermata, Cœlenterata, and Protozoa, Prof. Traquair. Respecting the work and the association which has been formed to prosecute it, we spoke at some length a few weeks ago, and will only remind our readers that the secretary of the Zoological Record Association is Mr. H. T. Stainton, F.R.S., to whom all applications for further information on the subject should be addressed.

We are glad to see that the higher examination for women at the University of London has this year been successfully taken by one candidate in the department of Natural Philosophy and Chemistry. She is from the Cheltenham Ladies' College.

We have received the Anniversary Address of Sir Roderick Murchison, as retiring President of the Royal Geographical Society, dealing with the history of geographical progress during the past year.

A letter has been received from Dr. Hooker from Mogadore, bringing down the narrative of his journey to April 26. The party had visited Ceuta, Gibraltar, and Casa Blanca, but had found no very striking novelties since the last report.

Mr. Thomas Moore, the Curator of the Chelsea Botanic Gardens, is now delivering in the gardens a course of six lectures on Botany for medical students.

A scientific society has been formed at Middletown, Connecticut, and Dr. John Johnson, of the university, elected president, Prof. Rice, corresponding secretary, and Prof. John M. Van Vleck, treasurer.

Prof. Newton of Cambridge is engaged upon a revised edition of Yarrell's well-known "History of British Birds," which has long been justly regarded as the standard work on the ornithology of these islands. It is now more than thirty years ago since the late Mr. Yarrell began the first edition of this book, and as the literature of the subject has in that time been doubled if not trebled, the editor will have enough to do to bring the work up to the level of the information of the present day. It is announced that the mode of publication will be in monthly numbers, the first of which may be expected in the course of a few days.

At a recent meeting of the Asiatic Society of Bengal, Col. Strachey made a communication to the effect that the Government of India have lately resolved to place four lacs of rupees in deposit, which sum should be available for completing the new Museum building at Calcutta. He regretted the delay which has been caused in the construction of the building, and stated that it was greatly due to the financial difficulty in which the Government of India found themselves a short time ago. Col. Strachey mentioned that the original approximate estimate amounted to about three and a half lacs of rupees. This sum had been sanctioned by Government, and the work for the new building was commenced. Subsequently the regular estimate came up, and it amounted to about seven lacs. After about four lacs had already been spent, a revised estimate was called for, and this rose to about ten lacs. It was, therefore, not surprising that the Government stepped in and inquired into the whole matter carefully, and this caused such a delay that it became impossible to complete the Museum within the appointed time, 23rd March, 1871. However, Col. Strachey hoped that the present action taken by Government in the matter would bring the building to its desired completion at as early a date as possible.

At Plymouth, one of the largest provincial centres of science instruction in connection with the South Kensington department, this year's examinations have just been brought to a close, the arrangements having been carried out under the direction of the School Board for the town, in compliance with the request of the department in their circular of March 7th In accordance with it the School Board appointed their clerk, Mr. Henry Soltan, as the examination secretary, and the assistants were selected principally from the secretaries of the various Science and Art Schools of the town. These gentlemen have been occupied every evening during last month, up to Monday last (22nd), in superintending the conduct of the various examinations which have all been held at the Public Free School, and which have comprised no less than twenty-two subjects in science, besides the four branches of art study, of the second grade, the examinations in which in previous years have been held in March. The details show that a total of 846 papers has been worked, 257 in Art and 589 in Science. The largest numbers of papers in any special subjects were Physical Geography 151, and Freehand Drawing, and Mathematics, stages 1, 2, and 3, each 112.

A considerable portion of modern astronomical literature appears to be designed to bring science into contempt. A paper has been circulated, containing an "extract from a letter addressed to John Hampden, Esq., Chippenham, Wilts," attributing the death of Sir John Herschel to "the chagrin he has for some time evinced at the severe attacks recently made on the astronomical theories with which his name has been so long and prominently associated." It is only the most uncleanly of animals that howl around the bodies of the dead; we should have thought that any "gentleman" receiving such a letter would have consigned it at once to the fire instead of sowing it broadcast over the land.

The Scotch papers report a mirage at the mouth of the Forth on Sunday the 21st inst. The weather was remarkably warm, and in the afternoon there was a dull deceptive haze. The sea presented almost the appearance of a mirror, and the vessels upon it seemed to have a double reflection from the sea and the background beyond. At one time the masts and rigging seemed elongated to four or five times their natural length, and then in the course of a few minutes they were reduced so as to be scarcely visible. At other times the vessels appeared to be sailing double—one ship in sea and one in air. Extraordinary appearances were assumed by the May Island, which rose and fell and changed to all manner of shapes in the course of a few minutes. At one time it appeared a perpendicular wall, rising to the height of several hundred feet, and shortly afterwards it appeared to be flat on the surface of the sea All the other objects which came within the range of the refraction underwent similar changes, and the illusion lasted with varying features for several hours.

Dr. Marshall Hall has contributed to the *Devizes Gazette* a letter, calling attention to the importance, at the present time, of establishing local centres for instruction of farmers in agricultural chemistry and sciences connected therewith. In connection with the circular of Mr. Little's, printed last week, we hope the subject is now at length attracting the attention it deserves.

The *Engineer* of last week contains a drawing of the machinery used in the casting of the Vendôme Column, lately thrown down by the Paris Communists.

At Derajat, in India, a supply of water has been obtained by artesian boring at the depth of 400 feet.

"To the Rt. Hon. Earl Granville, K.G., H.M.'s Secretary of State for Foreign Affairs," &c., &c. Suppose there were addressed to his Lordship a rather pressing letter from the Anthropological Society of Honolulu or Bolivia, requesting him to furnish male and female skulls of each of the tribes of Irish, Welsh, Erse, Lowlanders, Danes, English, &c., inhabiting these islands; a special application made for skulls from St. Giles's, and the Liberties of Dublin; skulls of extinct tribes earnestly sought; and his Lordship expected to supply a consignment of 200 or 300 skulls, besides their flint and other implements, tobacco pipes, pottery, and so forth, ancient and modern. What would be the commotion here, and what the comments in our newspapers! and yet such a request has been made by the zealous Secretary of the Smithsonian Institute to the Government of the United States of Colombia, and he is likely to prefer the same hint here, now that the *Alabama* claims are in happy way of settlement. The Colombian Government has taken the matter coolly, and published the application in the *Gaceta Oficial*, calling to it the attention of the local authorities. How the local authorities will persuade the local Indians to hand over the solicited pair of recent male and female skulls, appears rather doubtful and dangerous, as will be the polite request of the resident magistrate, or other authority, to the tribes of Achill Island or Connemara. Some of the Indian tribes in Colombia are much more likely to place the skulls of the Government or their emissaries in their own local museums.

It is stated from India that a number of experiments have shown that the madar plant, when mixed with opium, is an excellent substitute for ipecacuanha in dysentery.

The Kava or Ava is well known as a favourite intoxicating drink of the South Sea Islanders. The extraordinary and disgusting mode of preparing this beverage, by chewing the root, ejecting the saliva into a bowl and fermenting it, has been the means of giving it a greater amount of publicity than it would have otherwise obtained. Many stories have been told about its uses and effects. It appears that at one time before the intercourse with foreigners the only intoxicating drink known to the natives was the water in which the roots of the kava plant (*Macropiper methysticum*) had been macerated, and this was comparatively little used, except as a medicine, as it was supposed to prevent corpulence. Since the introduction of foreign spirits into the islands the use of kava has much diminished, though intoxication is none the less common, and with many of the natives kava is still much appreciated, and even many of the lower classes of white people are confirmed kava-drinkers. It is said that, if drunk in excessive quantities, it produces numerous cutaneous diseases, but if taken in moderation has no ill effect upon the system. A drink, prepared in a similar filthy manner, is the South American Piwarri, which is produced by first chewing a sufficient quantity of cakes made of cassava meal (*Manihot utilissima*), and then putting the masticated material into a bowl with water, where it is left to ferment for some days, and finally boiled.

So much has been said about the adulteration of butter and the frequent substitute of a compound for that useful article, in which no trace of true butter exists, that the introduction of Australian butter into this country is a matter not only of commercial importance, but must also give general satisfaction to all classes. The butter is of good quality, and arrives after its lengthened voyage in good condition. It is produced in different parts of the Australian colony, so that we may hope to receive large and continuous supplies. It is a fact worth noting that whereas at one time large supplies of butter were exported from Cork into the colonies, one of these same colonies is now sending its produce to us.

The cultivation of the beet in England for the manufacture of sugar seems in a fair way of at last becoming a *fait accompli*. Besides the works already in operation at Lavenham in Suffolk, Buscote in Berkshire, and other places, a new beetroot sugar company has just been formed under good prospects at Sandwich Kent.

## PROF. WYVILLE THOMSON'S INTRODUCTORY LECTURE AT EDINBURGH UNIVERSITY

(*Continued from page* 76)

The distinction between inorganic bodies and organised beings instinct with life seems clear enough. Between the animal and the vegetable kingdoms it is impossible to draw a definite line. It is to solve this difficulty that Ernst Haeckel has proposed his fourth kingdom, and we have now to consider whether or not the solution is satisfactory or legitimate.

Plants have the power of secreting and storing in organs which are specially fitted for its reception, usually the leaves, a substance called "chlorophyll," by means of which, acting probably as a ferment or in some way which is not yet thoroughly understood, the plant can, under the influence of light, absorb carbonic acid from the atmosphere, decompose it, and, while the carbon is in the nascent condition, combine it with the elements of water, or with the elements of water and of ammonia, reduced likewise to the nascent state by the same agency. The plant thus gains from the inorganic world, from the water contained in the soil and the inorganic substances dissolved in it, and from the atmosphere, various elementary substances, chiefly, however, oxygen, hydrogen, carbon, and nitrogen, and these it recombines into ternary, quaternary, and still more complex organic compounds.

The operations which take place within the chlorophyll cell of the plant are briefly these:—

1. Water, $H_2O$, which has been absorbed by the root of the plant from the soil, and pumped up by endosmosis or capillary attraction to the leaf, is decomposed, and its elements are reduced to the nascent condition.

2. Carbonic acid, $CO_2$, which exists as a gas in the atmosphere to the amount of about 1 vol. to 2,500 of air, and which is consequently in contact with the surface of the leaves, is absorbed, and its elements are reduced to the nascent state.

3. Ammonia, $NH_3$, an abundant product of the decomposition of organised bodies, which exists in small quantities in the water of the soil, and much more abundantly as a gas in the atmosphere (in the proportion of about 1 vol. to 1,000,000 of air), is decomposed, and its elements are reduced to the nascent state.

4. The nascent carbon of the carbonic acid is combined with the elements of water in varying proportions, and by the re-combination of the elements of these binary compounds, ternary compounds; for example, cellulose, starch, dextrine, and gum, which have all apparently the same composition, $C_6H_{10}O_5$, or some multiple of these proportions; sucrose or cane sugar $C_{12}H_{22}O_{11}$; dextrose and levulose, grape and fruit sugar, $C_6H_{12}O_6$; the glucosides, such as tannine, $C_{27}H_{22}O_{17}$; the fixed and essential oils, some of which latter, however, as turpentine and its isomers, are binary, $C_{10}H_{16}$; and many like substances, are produced.

5. The nascent nitrogen of the ammonia, sulphur, which is probably derived from a trace of sulphuretted hydrogen in the atmosphere; phosphorus, derived from the decomposition of certain minerals contained in the soil or of vegetable or animal matter, are united to form quarternary, quinary, and more complex compounds, such as vegetable albumen, fibrin, casein, and protoplasm.

| | Albumen. | Casein. | Fibrin. |
|---|---|---|---|
| Carbon | 53·5 | 53·8 | 52·7 |
| Hydrogen | 7·0 | 7·2 | 6·9 |
| Nitrogen | 15·5 | 15·6 | 15·4 |
| Oxygen | 22·0 | 22·5 | 23·5 |
| Sulphur | 1·6 | 0·9 | 1·2 |
| Phosphorus | 0·4 | 0·0 | 0·3 |

and the vegetable alkaloids, for example, nicotine $C_{10}H_{14}N_2$, morphine $C_{17}H_{19}NO_3 + H_2O$, and narcotine $C_{22}H_{23}NO_7$. Under the guidance of the vital property, and through the medium of a peculiar substance called protoplasm, whose exact composition it is difficult to determine, but which seems to be closely related to albumen, which is constantly present where vital actions are going on, and in which alone apparently the peculiar property called life resides, these different substances are adjusted as to their related proportions, and selected and applied each to its destined object in the plant economy, to enlarge the cell or to strengthen the cell-wall, to lay the foundation of a new cell, to be stored up in a special reservoir for future use, to contribute, in short, to the development or maintenance of the special specific form of the organism of which it forms a part.

A plant cannot assimilate pure carbon, or hydrogen, or nitrogen; it seems that it can assimilate no elementary substance except oxygen, unless it be presented to it in the nascent condition. An animal stands in precisely the same relation to the binary compounds, carbonic acid, water, and ammonia. However abundantly, therefore, it might be supplied with these binary compounds which actually contain all the elements necessary for its sustenance, it would surely die of inanition. In order to be capable of affording nourishment to the animal kingdom, these substances must be elaborated to the condition of ternary and quaternary compounds, and this can only be done in the cells of plants. This, then, is the broad and practical distinction between the vegetable and the animal kingdoms. Plants have the power of absorbing, modifying, and organising inorganic substances, while animals are entirely dependent upon the organic substances thus prepared for their support. Taken in this sense, the distinction between the two kingdoms is most marked, and of the highest practical value; but when we set aside this one peculiar property, which is possessed only by some plants, and only by certain parts of those plants at certain periods of their life, and especially when we observe certain minute forms, of low organisation, on the verge of either kingdom, it becomes absolutely impossible to assign any definite distinctive character. The character which is, perhaps, most palpable and universal, is that a mass of vegetable protoplasm is at some time during its existence, inclosed in a cell-wall, which is composed of cellulose, or some very nearly allied ternary compound. Animal protoplasm is rarely, if ever, confined in this way; that is to say, in nucleated cells, with cellulose walls, which are found in all plants, and are not found in the animal kingdom.

The protoplasm of the cells of plants has the power, without developing colour and without the aid of light, of absorbing, decomposing, and assimilating the elements of already prepared organic ternary and quaternary products, whether they be derived from the soil or surface to which the plant is attached, as in the case of fungi; or from sap already elaborated by another part of the same plant, as in the growing root; or even of another plant, as in pale parasites. Most growing points in plants are pale, and the protoplasm in the cells of a pale shoot, or of a colourless plant, has precisely the same vital powers and relations as animal protoplasm. It is only in cells in which protoplasm elaborates and incorporates with itself endochrome, which seems to be a more powerful catalytic agent, capable of disengaging the component atoms of the more stable binary compounds when loosened by the vibrations of light, that the special function of the vegetable cell is performed. The cells of a pale growing point develop the characteristic cellulose wall, but the supply of material is abundant, for the protoplasm of these cells is either confluent, through the porous cell-walls, with the protoplasm of chlorophyll cells, or, as in the seed, it is in connection with a *cache* of preserved food, prepared and stored up by the protoplasm and endochrome of previous leaves.

The difference between the great mass of plants, as represented by their familiar and higher groups, and the higher animals, is very palpable; it is only among the comparatively obscure forms near the limits of either kingdom that a difficulty occurs. The general chemical composition of plants differs markedly from that of animals. A plant consists mainly of ternary compounds, cellulose, dextrine, starch, &c. In an animal, ternary compounds, although some of them—such as oils and fats—are of great importance in its economy, play altogether a subordinate part, and the bulk of the body is made up of substances of the albumen series—albumen, fibrin, and gelatin. Until lately this chemical distinction was regarded as absolute, and still it holds good generally, though glycogen and glucose (animal starch and sugar), and now recognised as being universally present in the tissues of the higher animals whether in health or in disease. A colouring matter apparently undistinguishable from chlorophyll is found in the green bodies of Hydra and Stentor, though there is as yet no evidence that this animal endochrome possesses the power of decomposing carbonic acid; and cellulose, perhaps the most special of the vegetable products, is found in the testa of ascidians. Endochrome is absent in many fungi.

The intimate structure of plants is usually very different from that of animals. Plants and all their parts consist of but one histological element, the cell with a cellulose wall, and its very simple modifications; the texture of plants is therefore to a great degree homogeneous. Animals, on the contrary, consist of many tissues highly differentiated, among which the nucleated vesicle with a definite wall, and tissues simply derived from it, are comparatively rare. The structure test fails also, however, on the borderland, for the most simple animals, such as Amœba and Gromia, are mere minute masses of jelly-like sarcode, which show no structure and no differentiation of tissues, and seem to differ only from the unicellular fungi and algæ in the absence of the cellular wall, while the free amœbiform cell-contents of the myxomycetous fungi are perfectly undistinguishable from such animals without a knowledge of their history.

The general plan of the organs of nutrition is strongly contrasted in the two kingdoms. One of the higher plants absorbs its peculiar food and assimilates it by means of an enormously extended external surface of roots and leaves. An animal, on the other hand, receives its prepared nourishment into the interior of its body by a mouth, and then subjects it to complicated processes of digestion and assimilation in contact with an extended internal absorbing and secreting surface, of which special portions are organised for the performance of the several steps in the process, till at length the unassimilable residue is rejected; but as we descend in the animal series, the digestive cavity becomes more and more simple, and in certain undoubted animal forms, such as the cestoid worms, the gregarinæ, and the foraminifera, it is entirely wanting, and nutrition is effected by absorption through the external surface as in plants.

No special character can be derived from the function of reproduction. Most plants, and many of the lower animals, are multiplied by gemmation and fission, and probably all animals and plants are propagated under certain circumstances by a nearly uniform process of sexual generation. The reproductive elements are produced internally in the higher animals and externally in plants; but even this minor distinction fails very early, for in large groups of the simpler animals ovaries and testes are external.

It is true in the general sense that animals possess organs and functions of relation which are absent in plants. Most plants remain permanently rooted to the ground, and neither the whole plant nor any visible part of it exhibits any spontaneous movements, either voluntary or automatic. It never performs any independent or consequent acts, from which one would deduce the existence of consciousness, intelligence, or will; still, there are certain phenomena even among the higher plants, connected with the habits of climbing plants and with the functions of fertilisation, which it is very difficult to explain without admitting some low form of a general harmonising and regulating function comparable to such an obscure manifestation of reflex nervous action as we have in sponges and in other animals in which a distinct nervous system is absent. The protoplasm in the interior of the vegetable exhibits movements so characteristic and special as almost to be sufficient, were other evidence wanting, to prove its absolute identity with the sarcode of the rhizopods; and when we reach the confines of the two kingdoms, the test of locomotion fails like the others, for the branched and plant-like sponge remains permanently rooted to the ground, while the freed cell-contents of many of the lower plants move actively, either by contractility of the sarcode substances, as in the plasmodia of the myxomycetous fungi, or by circlets or bunches of cilia, as in the zoospores of *Volvox*, *Euglena*, and *Chætophora*.

If we take a water-reed from a pond in summer, and carefully scrape off the slimy matter adhering to it upon a slip of glass and place it under a microscope, we may probably see in the field some minute oval bodies like the small seeds of a plant. These are the bodies of an animal belonging to the genus Gromia, a genus of the Rhizopoda, grouped among Haeckel's Protista, but usually regarded as true animals. If we break up one of them under the microscope, we find it to consist of a little mass of apparently perfectly structureless viscid semi-fluid jelly inclosed in a thin membranous oval shell, with a large opening at one end. This gelatinous mass is under the highest powers destitute of anything which can be called structure. A transparent colourless matrix contains extremely small globules and fine granules scattered through it, and here and there are rounded spaces, which seem to contain a homogeneous liquid. If instead of breaking up the animal we allow it to remain quiet in the water, probably in a few minutes we see a set of very delicate threads protruded from the opening in the test. These threads increase in length, and spread like a branching root in the water. When we examine these closely, we find that they are continuous with the jelly of the animal, that they are, in fact, mere processes of that jelly. When two of the threads touch they flow together, and coalesce, as two streams of treacle might do, showing that they are bounded by no membranous wall; and, when one of the threads comes in contact with an organic particle in the water, the particle sinks into it, and then the thread begins to flow back again into the body of the animal bearing the particle with it, as a stream of treacle might entangle, and carry along a crumb of bread.

The organic particles are introduced into the body, into any part of it, and there they are dissolved and assimilated. I believe that the granules observed in the gelatinous substance are particles of the various products of this assimilation, and that the living matter is perfectly homogeneous and transparent. If the creatures be kept for a few days in water nearly pure, they become less and less granular.

If the thread-like pseudo-podia, as they are called, be rudely touched, they at once contract, and flow rapidly back into their test. The membranous test cannot be truly regarded as a part of the animal, it is a mere excreted defensive covering incapable of any further change, or of manifesting any of the phenomena of life. The body of the animal can be easily squeezed out of it entire, and in that case it shortly begins the excretion of a new shield.

Here, then, we have a homogeneous substance which has the power of inducing and controlling chemical and physical forces, and of moulding into indefinite form the products of the regulated changes taking place within it, which therefore possesses life. The gelatinous matter which in this animal and in the whole sub-kingdom to which it belongs can thus feed and digest without a mouth or stomach, contract without muscles, display irritability without a nervous system—in fact, exhibit all the essential phenomena of living beings without a trace of organisation, is Protoplasm.

If now, laying aside the Gromia, we examine with the microscope the water-plant on which we found it, we find that the whole plant from end to end and in all its parts is honeycombed, that is to say, composed of a congeries of minute chambers separated from one another by well-defined walls, the walls giving the plant its support and consistency.

We place in the field of the microscope a small portion of the growing point of a leaf or stem, and we easily make out that the chambers are minute vesicles each complete in itself, adhering according to a definite arrangement to one another. As these cells have occupied a very prominent position in modern histological and physiological speculation, having been regarded, and being still regarded by many as the units of organisation, the centres and sources of all vital activity, I should wish to sketch distinctly their structure and properties. It is of no consequence whence the cell is selected. All vegetable cells appear to have the same structure at first, during their growth and while their vitality lasts; subsequently most of them undergo great changes, their walls being thicker and their cavities clogged with various secretions. There are some beautiful transparent-beaded hairs at the bottom of the flower cup of the white variety of the Virginian spider-wort. If we place one of these hairs in a drop of water in the field of the microscope, we find that it is simply composed of a row of oval cells attached end to end. The cell is in this case a minute vesicle with an extremely thin transparent wall. This wall consists of cellulose, a substance composed of thirty-six parts of carbon and thirty parts of water. The membrane is perfectly structureless under the highest powers of the microscope, and apparently continuous. It must, however, be minutely perforated, for water and various secretions and excretions pass through it freely. From its composition and structure it is impossible to imagine that vital force should reside in the vegetable cell-wall. We must regard it as an excretion of dead matter moulded as a boundary wall to the cell cavity by some external agent, but incapable of originating any vital action. The cell is full of water or mucous solution, and watching carefully with a proper arrangement of the light, and a moderately high power, we can distinctly trace threads of dense gelatinous matter moving slowly into the inner surface of the cell-wall. These streams commence wider in the region of a nucleus, which was at one time regarded as the heart of the cell, as it were, the centre of its vital activity, and gradually branch and diminish at a distance from it. Under the microscope granules appear in these streams, and with these granules embedded in them, as crumbs are embedded in a stream of treacle, the currents flow round and round the cell, the granules gradually disappearing and being absorbed. The observations of Prof. Max Schultze and of others have, I think, placed it beyond a doubt that this gelatinous substance occurring within the living cell, and forming, at all events, a large proportion of the cell-contents, is identical with the protoplasm which forms the entire substance of such an animal form as *Gromia*.

The necklace-like hair of the spider-wort is, in fact, a chain of cells with dead cellulose walls, and each with a living Gromia body imprisoned within it.

Now, although the power which plants possess of fixing carbon and combining it with the elements of water, is the character which practically distinguishes the Vegetable from the Animal kingdom, I have already shown that we cannot regard this as by any means a universal test. In this respect broomrapes and dodders are animals.

When we pass down by any path we choose, either through animals or plants, we come equally to a great series of very simple forms—mere little masses of protoplasm with a nucleus. Some of these contain peculiarly formed masses of bright colouring matter, green, scarlet, or yellow, and with the possession of such pigment we usually associate the power of decomposing carbonic acid. Many of these bodies have, however, no colouring matter at all, except what is derived from their food. A large number of these simple forms are enclosed in a wall of cellulose, but very many of them are naked or merely covered with a pellicle of firmer protoplasm; while some, such as the plasmodia of the myxogastric fungi are, for some part of their lives, enclosed in a cellulose wall, and for another part, naked. Going still lower, we have Haeckel's Monera, differing from the others merely in the absence of a nucleus and the total want of differentiation of any part. Even these last are sometimes coloured, and from their chemical reactions it seems very likely that they possess some low form of the peculiar vegetable power. Now, the question is, whether all these considerations lead in any way in the direction of establishing a separate kingdom for these simple beings. I think decidedly not, but it seems to me that they prove almost to demonstration that organic nature must be taken as one whole, that the Animal and Vegetable kingdoms are absolutely continuous, and that a tree flinging its green flags into the sunshine and feeding on the winds of heaven, is essentially nothing more than a vast colony of a protozoon, comparable to a gigantic nummulite, only building a cellulose instead of a calcareous shell, and developing a special secretion in special organs for the purpose of enabling it to do so.

## *MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY*

HAVING now for the tenth time the honour of addressing you from this chair on the occasion of your annual gathering, it has been my wish to lay before you a general sketch of the progress making in systematic Biology, the foundation upon which must rest the theoretical and speculative, as well as the practical, branches of the science, to report upon the efforts made further to investigate, establish, and extend that foundation, and to convert the numerous quicksands with which it is beset into solid rock. This subject formed the chief portion of my address of 1862, and again of those of 1866 and 1868; but on the present occasion I have had some difficulties to contend with. Mr. Dallas, to whose kindness I owed the zoological notes I required, has now duties which fully absorb his time, and I have been obliged to apply to foreign correspondents, as well as to my zoological friends at home, for the necessary information. They have one and all responded to my call with a readiness for which I cannot too heartily express my thanks; and if there is some diversity in the extent and nature of the information I have received from different countries, which may prevent any very correct estimate of the comparative progress made in them, it is owing to the questions which I put having been stated too generally, and, though sent in the same words to my different correspondents, they have been differently understood by them. In such a review, however, as I am able to prepare, I propose chiefly to consider the relative progress made by zoologists and botanists in the methods pursued and the results obtained, in the first place as to general works common to all countries, and secondly as to those which are more particularly worked out in or more specially relate to each of the principal states or nations where biological science is pursued, prefacing this review by a few general remarks supplementary to those I laid before you in my first address in 1862.

Since that time systematic biology has to a certain degree been cast into the background by the great impulse given to the more speculative branches of the science by the promulgation of the

Darwinian theories. The great thunderbolt had indeed been launched, but had not yet produced its full effect. We systematists, bred up in the doctrine of the fixed immutability of species within positive limits, who had always thought it one great object to ascertain what those limits were, and by what means species, in their never-ending variations and constant attempts to overstep those limits, were invariably checked and thrown back within their own domain, might at first have been disposed to resist the revolutionary tendency of the new doctrine; but we felt shaken and puzzled. The wide field opened for the exercise of speculative tendencies was soon overrun by numerous aspirants, a cry of contempt was raised against museum zoologists and herbarium botanists, and nothing was allowed to be scientific which was not theoretical or microscopical. But this has been carried, in some instances, too far. If facts without deductions are of little avail, assumptions without facts are worse than useless. Theorists in their disputes must bring forth the evidences they rely upon, and these evidences can only be derived from and tested by sound systematic biology, which must resume and is resuming its proper position in the ranks of science, controlled and guided in its course by the results of those theories, for which it has supplied the basis.* If the absolute immutability of races is no longer to be relied upon, the greater number of them (whether genera, species, or varieties) are at the present or any other geological period, practically circumscribed within more or less definite limits. The ascertaining those limits in every detail of form, structure, habit, and constitution, and the judicious appreciation of the very complicated relations born to each other by the different races so limited, is as necessary as the supplementing the scantiness of data from the depths of Teutonic consciousness by the vivid flashes of Italian imagination, or as the magnifying minute as yet undeveloped organisms, with a precision beyond what is fully justified by our best instruments.

I am, however, far from denying on the one hand how much biological science has of late been raised, since it has been brought to bear through well-developed theories and hypotheses upon the history of our globe, and of the races it has borne; and on the other, how very much the basis upon which it rests has been improved and consolidated by the assiduous use of the microscope and the dissecting knife; but I would insist upon the necessity of equal ability being applied to the intermediate process of method or nomenclature and classification, which forms the connecting link between the labours of the anatomist and the theorist, reducing the observations of the one to forms available for the arguments of the other. All three, the minute observer, the systematist, and the theorist, thus assisting each other, equally contribute to the general advancement of science, and for all practical application, the systematist's share of duty is certainly the most important.

The quicksands to which I have alluded to as besetting thus the foundation of biological science, may be classed as imperfect data and false data, imperfect method and false method. To show what progress is making in removing or consolidating them, it may be useful to consider what these data are, and what are our means of fixing them so as to be readily available for use.

It must, in the first place, be remembered that the races whose relations to each other we study, can only be present to our minds in an abstract form. In treating of a genus, a species, or a variety, it is not enough to have one individual before our eyes, we must combine the properties belonging to the whole race we are considering, abstracted from those peculiar to subordinate races or individuals. We cannot form a correct idea of a species from a single individual, nor of a genus from a single one of its species. We can no more set up a typical species than a typical individual. If we had before us an exact individual representative of the common parent from which all the individuals of a species or all the species of a genus have descended—or if you prefer it an exact copy of the model or type after which the whole species or genus had been created, we should have no possible means of recognising it. I once heard a lecture of a German philosophical naturalist of considerable reputation in his day, in which he thought he proved that the common Clover was the type of Papilionaceæ. His facts were correct enough, but his arguments might have been turned in favour of any other individual species that might have been selected. Suppose two individuals of a species, two species of a genus, two genera of a family, in one of which certain organs are more developed, more differentiated, or more consolidated than in the other, if we agree upon the first question of which is the most perfect, a point upon which naturalists seldom do agree, how are we to determine which represents the common parent or model? whether the perfect one is an improvement upon, or an improved copy, or the imperfect one a degeneracy from or a bad imitation of the other? No direct evidence goes beyond a very few generations, reasoning from analogy is impossible without direct evidence to start from, and the imagining a type without either is the business of the poet, not of the naturalist.

It follows that every such abstract idea of a race must be derived from the observation, by ourselves or by others, of as large a number of the constituent individuals as possible. However fixed a race may be, if fixed at all in Nature, that is not the case with our abstract idea of it, no species or genus we establish can be considered as absolutely fixed, it will ever have to be completed, corrected, or modified, as more and more individuals come to be correctly observed. Hence it is, that a species described from a single specimen, and even a genus established on a single species, always excites more or less of suspicion unless supported by strong reasoning from analogy or confirmed by repeated observation.

Our means of observing and methodising biological facts, of establishing and classifying those abstract ideas we call varieties, species, genera, families &c., consist in the study (1) of living individual organisms; (2) of preserved specimens; (3) of pictorial delineations; and (4) of written descriptions. Each of these sources of information has its special advantages, but each is attended by some special deficiencies to be supplied by one or more of the others.

1. The study of living individuals in their natural state is without doubt the most satisfactory, but very few such individuals can be simultaneously observed for the purpose of comparison, and no one individual, at any one moment, can supply the whole of the data required relating even to that individual. Some additional facilities in these respects are given by the maintenance of collections of living animals and plants, particularly useful in affording the means of continuous observation during the various phases of the life of one and the same individual, and sometimes through successive generations, or in facilitating the internal examination of organisms immediately after death, when the great physiological changes consequent upon death have only commenced. But there are drawbacks and difficulties to be overcome, as well as a few special sources of error to be guarded against, and in this respect, as well as in the progress recently made in their application to science, there is a marked difference between zoological and botanical living collections, or so-called gardens.

The great drawback to living collections, especially zoological, is their necessary incompleteness. At the best it is individuals only, not species, and in a few cases genera that are exposed to observation; genera, indeed, can always be better represented than species, for a few species bear a much larger proportion to the total number contained in a genus, than a few individuals to the total number which a species contains. Whole classes are entirely wanting in zoological gardens, which are usually limited to vertebrata. Of late years means have been found to include a few aquatic animals of the lower orders, but insects, for instance, those animals which exercise the greatest influence on the general economy of nature, the observation of whose life and transformations is every day acquiring greater importance, are wholly unrepresented in zoological gardens. The shortness of duration of their individual lives, their enormous power of propagation, the different mediums in which they pass the different stages of their existence, will long be obstacles to the formation of living entomological collections on anything like a satisfactory scale. The cost also of the formation and maintenance of living collections is very much greater in the case of animals than of plants; but on the other hand zoologists have the advantage of the attractiveness of their menageries to the general unscientific but paying public, and, under judicious management, some sacrifices to popular tastes are far outweighed by the additional funds obtained towards rendering their collections useful to science.

The false data or errors to be guarded against in the observation of living zoological collections are chiefly owing to the unnatural conditions in which the animals are placed. Ungenial climate, unaccustomed food, want of exercise, &c., act upon their temper, habits, and constitution, and confinement materially

* The great importance of morphology and classification, the elements of systematic biology, has been forcibly illustrated by Prof. Flower in his last year's introductory lecture at the Royal College of Surgeons.

modifies circumstances connected with their propagation. Such errors or false data are, no doubt, as yet very few and unimportant compared to those which have arisen from the reliance on garden plants for botanical observations, but, as zoological gardens multiply and extend, they will have to be more and more kept in view.

In my younger days there were already a number of small collections of living animals, but almost all either travelling or local menageries exhibited for money by private individuals, or small collections kept up as a matter of curiosity for the benefit of the public, such as those of the Pfauen-Insel at Potsdam, the park at Portici, or our own Tower menagerie. At Paris alone, at the Jardin des Plantes, in the flourishing days of the Jussieus and Cuviers, was the living zoological collection rendered essentially subservient to the purposes of science. Since then, however, matters have much changed: the Jardin des Plantes, which so long reigned supreme, has, by remaining stationary, sunk into a second rank. She may indeed be as justly as ever proud of her Milne-Edwards, her Brongniart, her Decaisne, and many others, but long out of favour with the Government and the paying public, who transferred their patronage to the high-sounding Jardin d'Acclimatation, now no more, she has been almost abandoned to the resources of pure science, always of the most restricted in a pecuniary point of view. We in the mean time, and after our example several continental states or cities, have made great advances. The formation of our Zoological Society and Gardens opened a new era in the cultivation of the science. After various vicissitudes, the Society had the good fortune to secure the services of one who combined in the highest degree zoological eminence with administrative ability, and thus our great living zoological collection is now raised to the proud relative position which the Jardin des Plantes once held, and which there seems every reason to hope it will long maintain. With an annual income of about 23,000*l.* the Zoological Society is enabled to maintain a living collection of about a thousand species of Vertebrata, and although some portion of the surplus funds is necessarily applied for the sole gratification of the paying public, yet a fair share is devoted to the real promotion of that science for which all the fellows are supposed to subscribe, the accurate observation of the animals maintained, the dissection of those that die, and the publication of the results. Physiological experiments are either actually made in the garden, or promoted and liberally assisted, such, for instance, as those on the transfusion of blood, the effects or non-effects of which were recently laid before the Royal Society by Mr. F. Galton. A very rich zoological library has been formed, and last year's accounts show a sum of about 1,800*l.* expended in the Society's scientific publications.

Zoological gardens, after the example of the London one, have been established not only in several of our provincial towns, but in various continental cities, amongst which the more important ones, as I am informed, are those of Amsterdam, Antwerp, Hamburgh, Cologne, Frankfort, Berlin, Rotterdam, and Dresden; the receipts of the one at Hamburgh, for instance, amounting annually, according to the published reports, to between 8,000*l.* and 9,000*l.* There are also so-called gardens of acclimatisation; but these have not much of a scientific character; their professed object indeed is not so much the observation of the physiology and constitution of animals as their modification for practical purposes, and practically they are chiefly known as places of recreation, and are not always very successful. The great one in the Bois de Boulogne, now destroyed, out of an expenditure in 1868 of about 7,200*l.* showed a deficit of about 1,600*l.*; a smaller one at the Hague is enabled to pay an annual dividend to its shareholders.

Living collections of plants have great advantages over these of animals, they can be so much more extensively maintained at a comparatively small cost. In several botanical gardens several thousand species have been readily cultivated at a comparatively small cost, and species can be represented by a considerable number of individuals, a great gain especially where instruction is the immediate object, the lives of many can be watched through several successive generations, and great facilities are afforded for physiological experiments and microscopical observations on plants and their organs whilst still retaining more or less of life. On the other hand the false data recorded from observations made in botanical gardens have been lamentably numerous and important. A plant in the course of its life so alters its outer aspect that each one cannot be individualised by the keeper of a large collection, and at one period, that of the seed in the ground, it is wholly withdrawn from his observation. He is therefore obliged to trust to labels, these are often mismatched by accident or by the carelessness of the workmen employed, or again, one seed has been sown and another has come up in its place, or a perennial has perished and made room for a sucker or seedling from an adjoining species. The misnomers arising from these and other causes have become perpetuated and sanctioned by directors who, for want of adequate libraries or herbaria, or sometimes for want of experience or ability, have been unable to detect them. Plants have also been so disguised or essentially altered by cultivation that it has become difficult to recognise their identity, and new varieties or hybrids, which, if left to themselves, would have succumbed to some of the innumerable causes of destruction they are constantly exposed to in a wild state, have been preserved and propagated through the protective care of the cultivator, and pronounced at once to be new species. If, moreover, a misplaced label indicates that the seed has been received from a country where no plants of a similar type are known to grow, the director readily notes it as a new genus, and, proud of the discovery, gives it a name and appends a so-called diagnosis to his next seed-catalogue, adding one more to the numerous puzzles with which the science is encumbered. So far, indeed, had this nuisance been carried in several Continental gardens in the earlier portion of the present century, that, excepting perhaps Fischer and Meyer's and a few other first-rate indexes, the great majority, perhaps nine-tenths, of the new species published in these catalogues have proved untenable, and, from my own experience, I am now obliged, *a priori*, to set down as doubtful every species established on a garden plant without confirmation from wild specimens. Fortunately the custom is now abating, and directors of botanic gardens are beginning to perceive that they do not add to their reputation by having their names appended to those of bad species.

Living collections of plants, or botanical gardens, are of much older date than zoological ones, and since the sixteenth century have been attached to the principal universities which have medical schools, that of Padua, dating from 1525, that of Pisa, from 1544, and of Montpellier, from 1597. The Jardin des Plantes of Paris, which in botany, even more than in zoology, so long reigned supreme, was established in 1610, our own first one at Oxford in 1632. These university gardens having been generally more or less under the control of eminent resident botanists, have contributed very largely to the means of studying the structure and affinities of plants, especially in those Continental cities where a milder or more steady climate has facilitated the maintenance of large collections in the open air or with little protection. Continental gardens have also been long and are still made largely available for the purpose of instruction as well as of scientific experiments, of which the recent labours of Naudin and Decaisne are an excellent illustration. For these scientific purposes the arrangement in large and small square compartments is peculiarly suitable, and I confess that I have frequently had greater pleasure in witnessing the facilities afforded to zealous students in following up, book in hand, the straight rows of scientifically-arranged plants in these formal university gardens, than in watching the gay crowds that flock to the more ornamentally laid out public botanic gardens.

I do not think that generally much advance has been made of late years in Continental botanical gardens. Those that I first visited in 1830 appeared to me to be but little improved when I again went over them in 1869. Some have acquired additional space, others have paid more attention to ornament, but most have remained nearly stationary, and a few have even fallen back. In our own country we have made great progress. Kew Gardens had indeed, in former days, rendered assistance to the investigations of Robert Brown and a few other favoured individuals, but they were the Sovereign's private property, and were kept very close, with little encouragement to science at large. But thirty years' unceasing exertions on the part of its distinguished directors, the two Hookers, father and son, have raised them to a point of scientific usefulness far beyond any other establishment of the kind at home or abroad. Of the large sums annually voted for it by Parliament, a portion has indeed to be applied to mere ornament and to the gratification of visitors, but yet, with all the drawbacks of our climate and consequent expenditure in houses, the largest named collection of species ever brought together in one spot, representatives of all parts of the globe, are there maintained, freely exhibited to the public, and submitted to the examination of scientific botanists.

(*To be continued*)

## SOCIETIES AND ACADEMIES

### London

Royal Society, May 25.—"Some Remarks on the Mechanism of Respiration." By F. Le Gros Clark.

The author commences his paper by narrating some experiments on recently slaughtered animals, in the course of which the remarkable tension of the diaphragm was noticed; and the varying condition of that muscle, and of the lungs and pleura, with their mutual relations, are commented on.

The importance of this passive tension of the diaphragm is indicated and exemplified both physiologically and pathologically. It is essential in retaining the supplemental air within the lungs, in restoring the equilibrium of repose, in economising active muscular power, and in maintaining the pericardial space, &c.

The action of the diaphragm in relation to the walls of the chest and to other muscles is next discussed; and the influence of the diaphragm in drawing in the chest-walls, under certain circumstances, is pointed out, and illustrated by cases of injury to the spinal cord.

The action of the intercostal muscles, as necessary adjuncts to the diaphragm and as muscles of inspiration, is insisted on and illustrated by diagrams; and a summary of their action is given.

The agency of the serratus magnus is then discussed; and reasons are advanced, supported by observation and experiment, to show that it is only under special conditions and to a limited extent that it can be regarded as taking any part in the act of inspiration.

The mobility of the different costal regions and of the sternum is exemplified by observation and experiment.

Lastly, the question of abdominal and thoracic breathing, severally in the male and female, is considered; and reasons are adduced for concluding that the received opinions on this subject are erroneous.

"Spectrum of Comet I." By Dr. William Huggins, F.R.S.—On April 7 a faint comet was discovered by Dr. Winnecke. I observed the comet on April 1 and May 2. On both days the comet was exceedingly faint, and on May 2 it was rendered more difficult to observe by the light of the moon and a faint haze in the atmosphere. It presented the appearance of a small faint coma, with an extension in the direction of the sun. When observed in the spectroscope, I could detect the light of the coma to consist almost entirely of three bright bands. A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. I was not able to do more than estimate roughly the position of the less refrangible band. The result gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangiele side. It would appear that this comet is similar in constitution to the comets which I examined in 1868.*

"Researches on the Hydrocarbons of the Series $C_nH_{2n+2}$."—By C. Schorlemmer.

In a former communication I have shown that the paraffins, the constitution of which is known, may be arranged in four groups. The first group, which I called *normal paraffins*, contain the carbon atoms linked together in a single chain. Of these I have obtained some new ones, which I shall describe more fully in a further communication. The normal paraffins which I have so far studied are given, together with their boiling points, in the following table:—

| | From petroleum. | From the acids of the series $C_nH_{2n-2}O_4$ | So-called alcohol radicals. | |
|---|---|---|---|---|
| $C_5H_{12}$ | 37° – 39° | —— | —— | |
| | | | Dipropyl. | From Mannite. |
| $C_6H_{14}$ | 69° – 70° | 69°·5 | 69° – 70° | 71·5° |
| $C_7H_{16}$ | 98° – 99° | 100°·5 | —— | —— |
| | | | Dibutyl. | From methylhexylcarbinol. |
| $C_8H_{16}$ | 123° – 124° | 123° – 124° | 123° – 124° | 124° |

That these paraffins have really the constitution which I have ascribed to them follows partly from their mode of formation; thus dipropyl was obtained from the normal propyl iodide, and dibutyl from normal butyl iodide. The constitution of the others was determined by converting them into alcohols and studying the oxidation products of the latter; thus the hexyl hydride from petroleum, as well as that obtained from mannite, was transformed into secondary hexyl alcohol, which on oxidation yielded acetic acid and *normal* butyric acid.

In the communication above referred to, I placed the hydrocarbon $C_8H_{18}$ from methyl-hexyl carbinol amongst another group; but I have found now that this body is identical with dibutyl and also with the hydrocarbon, which Zinke obtained from primary octyl alcohol. This chemist prepared also dioctyl, $C_{16}H_{34}$, which consequently is a normal paraffin; and it appears probable that dibexyl, which Brazier and Goslett obtained by the electrolysis of œnanthylic acid, belongs to this group too.

We are now acquainted with the following normal paraffins:—

| | Boiling-points. Found (mean). | Calculated. | Difference. |
|---|---|---|---|
| $C\ H_4$ | —— | —— | |
| $C_2H_6$ | —— | —— | |
| $C_3H_8$ | —— | —— | |
| $C_4H_{10}$ | 1° | 1° | |
| $C_5H_{12}$ | 38° | 38° | 37° |
| $C_6H_{14}$ | 70° | 71° | 33° |
| $C_7H_{16}$ | 99° | 100° | 29° |
| $C_8H_{18}$ | 124° | 125° | 25° |
| $C_{12}H_{26}$ | 202° | 207° | 4 × 19° |
| $C_{16}H_{34}$ | 278° | 278° | 4 × 19° |

From this it appears that the boiling-point is not raised 31° for each addition of $CH_2$, as I formerly assumed, but that, as the calculated numbers show, the difference between the boiling-points of the lower members decreases regularly by four until it becomes the well-known difference of 19°.

Chemical Society, May 18.—Prof. Frankland, F.R.S., president, in the chair. Messrs. T. Greenish and J. E. Mayall were elected Fellows. The following papers were read:—"On a new double salt of thallium," by R. T. Friswell. The author wishing to prepare thallic platino-cyanide, mixed hot solutions of thallic carbonate and potassic platino-cyanide, and obtained on leaving the mixture to cool masses of splendid crystals, which appeared by transmitted light of a magnificent crimson red, whilst their reflected colour was a bronzy green of strong metallic lustre. Analysis showed that they are a compound of thallic carbonate with thallic platino-cyanide, $Fl_2\ Pt\ Cy_4$, $CO\ Fl\ O_2$. On treating this salt with acids carbonic acid is set free, and a pale pink residue left, which on examination was found to be thallic platino-cyanide.—The next paper read was "On the action of nitric acid on dichloro-phenolsulphuric acid," by Dr. Armstrong.

Geologists' Association.—The excursion of this Society to Oxford took place on the 12th and 13th inst. On the first day the numbers assembled at the beautiful new University museum at noon, and were introduced to Professor Phillips, who commenced a descriptive lecture on the museum, its arrangement and contents. There is a peculiar double arrangement of the palæontological collection by which the student may with equal ease make himself acquainted with the organisms derived from any one geological formation or devote himself to the study of the fossil remains of a single class or order of the animal or vegetable kingdoms. The museum is not crowded, but contains good specimens of those species which are most typical or characteristic. These, too, with the fossil remains of saurians and mammals are the complete skeletons of analogous living genera, an arrangement most advantageous to the student. The speciality of the Oxford museum is the unique collection of the remains of *Cetiosaurus*. A most interesting description of the enormous bones of this genus was given by Prof. Phillips, who, by means of corresponding crocodilian bones, gave a clear idea of the vast size to which these huge creatures attained. The estimate made by the Professor was that the Cetiosaurus was 40 feet long and 12 feet in height, the femur being fully 60 inches long, while the femur of a crocodile, with which it was contrasted, being no more than nine inches in length. In the afternoon the party proceeded to Shotover Hill, examining by the way the excavations in the Oxford clay near the city, and the exposures of the coral rag and the Kimmeridge clay on the side of the hill. Near the top of Shotover, Portland sands and a thin band of Portland rock are seen, and above these beds and forming the summit of the hill are the "Iron Sands," which have been the subject of much dispute. These highly ferruginous beds were considered to be lower greensand, but the finding of a considerable number of fresh-water species of Mollusca has induced Prof. Phillips to conclude the "Iron Sands" to be of Wealden age. From these sands at the summit of the hill ochre is obtained in large quantities. In the evening a soirée was given by Mr. James Parker, of Oxford, who most hospitably entertained the members of the Association. The magnificent collection

* Phil. Trans. 1868, p. 555; and Proc. Roy. Soc. vol. xvi. p. 386.

of reptilian remains and other fossils from the neighbourhood of Oxford was shown to the visitors, and described by Prof. Phillips and Prof. Morris.—Saturday's proceedings were commenced by an early visit to Merton College, for the purpose of inspecting the very fine collection of fossils which Mr. Earwaker of that college has brought together. Afterwards the party, with Profs. Phillips and Morris, started by carriage for Islip, Enslow Bridge, and Kidlington. At Islip a very fine section of the Forest Marble and Cornbrash is exposed, and the usual fossils of these formations are here found. The village of Islip is, however, interesting to geologists on other grounds, for here lies Buckland. Around the tomb of the great geologist with his distinguished successor at their head the party assembled. The tomb is of polished Aberdeen granite, and the inscription briefly records the fact that there lie the remains of Dr. Buckland, Rector of Islip, Dean of Westminster, and First Reader in Geology in the University of Oxford. The quarries at Enslow Bridge, which have yielded a large number of the Saurian bones in the University Museum, was then visited, and here the visitors were highly gratified to find that during the morning a very fine *Teleosaurus* had been found, and the head, taken out of the bed in which it had lain for untold ages, was exposed to view. This quarry is in the great Oolite, the lower and uppermost strata of which in Oxfordshire yield remains of *Megalosaurus*, while in the middle beds we find *Teleosaurus*. A very remarkable bed of about twelve inches thick occurs a little above the Teleosaurian cave, crowded with *Terebratula maxillata* to the exclusion of every other species. Several other sections of the Great Oolite, Forest Marble, and Cornbrash were examined, and the weather being very fine the drive through the beautiful country was much enjoyed, and the return to Oxford effected in time to allow of the party taking their departure for London by the evening train.

DUBLIN

Royal Dublin Society, May 13.—Dr. J. Emerson Reynolds, analyst to the Society, delivered the concluding lecture for the session 1870-71. The subject of the lecture was the "Chemistry of Milk." The lecturer, referring to cow's milk more particularly, described the constituents of the fluid at considerable length, and showed the precise quantities of butter, casein, sugar, and salts obtained from a known amount of milk of good quality. A number of new facts bearing on the chemical constitution of the different substances present in milk were then stated, and the relations of casein and sugar to the several parts of the animal organism were pointed out. It was proved by a large number of analyses of milk taken from cows fed in various ways in different parts of the country, that milk is naturally subject to very wide variations in the proportions of its constituents, and hence that it is extremely difficult, if not impossible, to state with precision that a given sample of milk had been adulterated with a certain amount of water. Under these circumstances the lecturer suggested that milk should in future be judged according as it might reach or fall below a certain standard quality, fair alike to the vendor and the purchaser, but that milk falling below the standard should not necessarily be stigmatised as adulterated, but simply have a lower commercial value attached to it. Dr. Reynolds stated, as the result of his experience, that milk sold at the present price per quart may fairly be expected to have the following composition in one hundred parts:—

| | |
|---|---|
| Water | 87·0 |
| Butter | 3·5 |
| Casein | 4·0 |
| Sugar | 5·0 |
| Salts | ·5 |

The proportion of fatty matter can be easily ascertained by the rapid methods of Sir Joseph Banks or Dr. Minchin, and the sugar determined in a few minutes by the aid of the polariscope; but it has been hitherto impossible to speedily measure the proportion of the valuable casein of milk without recourse to elaborate chemical analysis. By means of a very simple contrivance, which was exhibited at the lecture, the proportion of casein can, however, now be speedily ascertained; we are, therefore, for the first time in a position to form quickly a sufficiently precise estimate of the nutritive value of a given sample of milk. The lecturer concluded by expressing a hope that the public would now use the means placed in its hands for guarding against imposition on one side, or the hasty condemnation of the honest trader on the other.

May 22.—Prof. Dyer in the chair. Prof. R. Ball read notes on Kater's Pendulum and on a new Hydraulic Press.—Dr. J. Emerson Reynolds read notes of Experiments on the flow of Liquids through Capillary Tubes.—Mr. W. F. Kirby communicated a list of the species of *Papilionidæ* or Swallow-tailed Butterflies in the collection of the Society, and exhibited specimens in illustration. Among these were a long series of *P. cymochles* Gray and *P. idalion* Felder, which Mr. Kirby believed to be sexes of one species. He also called attention to a remarkable variety (?) of *P. polymnestor* Cram., in which the blue colouring of the hind wings was reduced to a band.—Prof. Dyer read a paper on Bud scales, in the course of which he objected to the word mimicry being used for the resemblance borne by a plant belonging to one natural family to a plant belonging to a different natural family, except in such cases as where the plants were found living side by side.—Mr. A. G. More exhibited for Dr. Cartt a number of additions to the museum, and told some of the more remarkable stories known about each species. Among the more interesting specimens exhibited, one of the American Goshawk shot in Tipperary and one of *Cygnus bewickii* may be mentioned.

## DIARY

*THURSDAY*, JUNE 1.

LINNEAN SOCIETY, at 8.—On Some Plants from Northern China: Dr. Hance.—On South American *Hippocrateaceæ*: Mr. Miers.
CHEMICAL SOCIETY, at 8.—On Ozone: Dr. Debus, F.R.S.
ROYAL INSTITUTION, at 3.—On Sound: Prof. Tyndall, F.R.S.

*FRIDAY*, JUNE 2.

GEOLOGISTS' ASSOCIATION, at 8.—On Flint: M. Hawkins Johnson, F.G.S.
ROYAL INSTITUTION, at 9.—Gaseous and Liquid States of Matter: Prof. Andrews.

*SATURDAY*, JUNE 3.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.
ROYAL INSTITUTION, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

*MONDAY*, JUNE 5.

ENTOMOLOGICAL SOCIETY, at 7.
ROYAL INSTITUTION, at 2.—General Monthly Meeting.

*TUESDAY*, JUNE 6.

ZOOLOGICAL SOCIETY, at 9.—On Dinornis (Part XVII). Containing a description of the sternum and pelvis, with an attempted restoration of *Aptornis defosser*, Ow.: Prof. Owen, F.R.S.—On a Seal new to the British Fauna: Prof. Flower.—On Risso's Dolphin: Prof. Flower.
SOCIETY OF BIBLICAL ARCHÆOLOGY.—On the Early History of Assyria and of Babylonia, from Contemporary Inscriptions (part 1): G. Smith.—On the Date of the Nativity: J. W. Bosanquet.
ROYAL INSTITUTION, at 3.—Least Action in Nature: Rev. Prof. Haughton.

*WEDNESDAY*, JUNE 7.

GEOLOGICAL SOCIETY, at 8.—Notes on the Geology of part of the County of Donegal: A. H. Green, M.A., F.G.S.—On the Persistence in the Deep-seas of the present day of *Caryophyllia cylindracea*, Reuss, a cretaceous coral: Prof. P. Martin Duncan, F.R.S.—Note on an *Ichthyosaurus* (*I. enthikiodon*), from Kimmeridge Bay, Dorset: J. W. Hulke, F.R.S.—Note on a Fragment of a Teleosaurian Snout, from Kimmeridge Bay, Dorset: J. W. Hulke, Esq.
ROYAL MICROSCOPICAL SOCIETY, at 8.
LONDON INSTITUTION, at 2.—Distribution of Prizes and Certificates by Mr. T. Baring, M.P., President.

*THURSDAY*, JUNE 8.

SOCIETY OF ANTIQUARIES, at 8.30.
MATHEMATICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.

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THURSDAY, JUNE 8, 1871

## THE GENERAL OCEANIC CIRCULATION

AMONG the results of the *Porcupine* Expeditions of 1869 and 1870, there are perhaps none more important than those relating to the Temperature of the Deep Sea. For it is only to such accurate determinations of ocean temperatures as have now been made for the first time, not only at the surface and the bottom, but also at intermediate depths, that a really scientific theory can be framed of that great Oceanic Circulation, which, while it eludes all ordinary means of direct observation, seems to produce a far more important effect, both on terrestrial climate and on the distribution of the marine fauna, than that of the entire aggregate of the surface-currents which are more patent to sight. The latter usually have winds for their prime motors, and their direction is mainly determined by the configuration of the land; so that their course and action will change with any superficial alteration which either opens out a new passage or blocks up an old one. The former, on the other hand, depending solely on difference of temperature, will (to use Sir J. Herschel's apposite language) have its movements, direction, and channels of concentration mainly determined by the configuration of the sea-bottom; and vast elevations and subsidences may take place in this, without producing any change that is discernible at the surface.

The history of the doctrine of the general oceanic circulation has been recently given in the Anniversary Address of the President of the Geological Society, with a completeness which (so far as we are aware) had never been previously paralleled. But this doctrine has hitherto rested on the very insecure foundation of observations which were alike inadequate and inaccurate; and it has consequently been discredited, both by physicists and by physical geographers. It is now impossible to assign a precise value to the older observations upon deep-sea temperatures. For it was shown by the careful experiments which were made by Mr. Casella two years ago, under the direction of the late Prof. W. A. Miller, Dr. Carpenter, and Captain Davis of the Admiralty, that the pressure of sea-water at great depths on the bulb of the thermometer—a pressure amounting to about a ton per square inch for every 800 fathoms—exerts so great an influence on even the very best instruments of the ordinary construction, as to cause a rise of eight or ten degrees under an amount equivalent to that which would be exerted at from 2,000 to 2,500 fathoms' depth;* and the error of many thermometers under the same pressure was two or three times that amount. There is reason to believe that some of the thermometers formerly employed, especially in the French scientific expeditions, were protected against that influence; but no such protection appears to have been applied to the thermometers supplied to Sir James Ross's Antarctic Expedition; and the observations by which he supposed himself to have established the existence of a uniform deep-sea temperature of about 39°, now seem to have been altogether fallacious. So again, Captain Spratt's observations in the Mediterranean, though made with great care, were seriously vitiated by this source of error.

It appears from Mr. Prestwich's exhaustive summary, that as long ago as 1812 Humboldt had maintained that such a low temperature exists at great depths in tropical seas, as can only be accounted for by the hypothesis of under currents from the Poles to the Equator. And this view was adopted by D'Aubuisson, Lenz, and Pouillet; the latter of whom considered it certain "that there is generally an upper current carrying the warm tropical waters towards the Polar seas, and an under current carrying the cold waters of the Arctic regions from the Poles to the Equator." Our Arctic navigators had met with temperatures in the Polar seas as low as 29° at 1,000 fathoms; and these observations have been more recently confirmed by those of M. Charles Martins and others in the neighbourhood of Spitzbergen. Several instances are recorded, on the other hand, in which temperatures of from 38° to 35° were observed at great depths nearly under the Equator; and this alike in the Atlantic, Pacific, and Indian Oceans.

The Temperature-soundings taken in the *Lightning* and *Porcupine* Expeditions, with trustworthy instruments, have shown:—(1) That in the channel of from 600 to 700 fathoms' depth which lies between the North of Scotland, the Orkney and Shetland Islands, and the Faroes, there is an upper stratum of which the temperature is considerably higher than the normal of the latitude; whilst there is stratum occupying the lower half of this channel, of which the temperature ranges as low as from 32° to 29°·5; and a "stratum of intermixture" lying between these two, in which the temperature rapidly falls—as much as 15° in 100 fathoms. (2.) That off the coast of Portugal, beneath the surface-stratum, which (like that of the Mediterranean) is super-heated during the summer by direct solar radiation, there is a nearly uniform temperature down to about 800 fathoms; but that there is a "stratum of intermixture" about 200 fathoms thick, in which the thermometer sinks 9°; and that below 1,000 fathoms the temperature ranges from 39° down to about 36°·5. (3.) That in the Mediterranean the temperature beneath the super-heated surface-stratum is uniform to any depth; being at 1,500 or 1,700 fathoms whatever it is at 100 fathoms, namely from 56° to 54°, according to the locality. To these may be added (4) the observations recently made by Commander Chimmo, with the like trustworthy thermometers, which, in lat. 3° 18½′ S., and long. 95° 39′ E., gave 35°·2 as the bottom temperature at 1,806 fathoms and 33°·6 at 2,306 fathoms. These seem to be the lowest temperatures yet observed in any part of the deep ocean basins outside the Polar area.

It is clear, therefore, that very strong evidence now exists, that instead of a uniform deep-sea temperature of 39°, which, on the authority of Sir James Ross, by whom the doctrine was first promulgated, and of Sir J. Herschel, by whom it was accepted and fathered, had come to be generally accepted in this country at the time when the recent deep-sea explorations commenced, not only is the temperature of the deeper parts of the Arctic basin below the freezing-point of fresh water, but the temperature of the deepest parts of the great oceanic basins, even under the Equator,

* Mr. Prestwich cites Dr. Carpenter as estimating the error from pressure "at 2° or 3° or even more." The error is said by Dr. Carpenter to have been from 2° to 3° on the depths of from 500 to 700 fathoms first explored; but would have been from 8° to 10° at the depths subsequently reached.

is not far above that point. And it seems impossible to account for the latter of these facts in any other mode, than by assuming that Polar water is continually finding its way from the depths of the Polar basins along the floor of the great oceanic areas, so as to reach or even to cross the Equator. And as no such deep efflux could continue to take place without a corresponding in-draught to replace it, a general circulation must be assumed to take place between the Polar and Equatorial areas, as was long since predicated by Pouillet.

Such a vertical circulation, it was affirmed by Prof. Buff, would be necessarily caused by the opposition of temperature between the Equatorial and the Polar seas; and this view was adopted by Dr. Carpenter, in his *Porcupine* Report of 1869, as harmonising with the temperature-phenomena which had been determined in the expedition of that year. It has been since contested, however, not only by Mr. Croll and Dr. Petermann, but also by Dr. Carpenter's colleague, Prof. Wyville Thomson, all of whom agree in regarding the amelioration of the temperature of the Arctic Sea as entirely due to an extension of the Gulf Stream, the underflow of Polar water being merely its complement. And the authority of Sir John Herschel was invoked against the idea that any general oceanic circulation could be maintained by difference of temperature alone; though his statements, when carefully examined, only go to prove that no such difference could produce *sensible currents*.

Such was the state of the question when the *Porcupine* Expedition of last year concluded its work; and the results obtained, whilst confirmatory of previous observations, suggested to Dr. Carpenter a definite Physical Theory, which now comes before us with the express approval of the great philosopher who had been said to be opposed to it.

Having ascertained, as our readers have learned from his report, the existence of an outward under-current in the Strait of Gibraltar, which carries back into the Atlantic the water of the Mediterranean that has undergone concentration by the excess of evaporation in its basin, Dr. Carpenter applied himself to the consideration of the forces by which the superficial in-current and the deep out-current are sustained; and came to the conclusion that, as had been previously urged by Captain Maury, a *vera causa* for both is to be found in excess of evaporation, which at the same time lowers the level and increases the density of the Mediterranean column as compared with a corresponding column of Atlantic water. This conclusion, when scientifically worked out, was found to be applicable, *mutatis mutandis*, to the converse case of the Baltic Sound; in which, as was long ago experimentally shown (with a result that has recently been confirmed by Dr. Forchhammer), a deep current of salt water flows inwards from the North Sea, whilst a strong current of brackish water sets outwards from the Baltic, the amount of fresh water that drains into which is greatly in excess of the evaporation from its surface.

Comparing, then, the Polar and Equatorial areas, it is shown by Dr. Carpenter that there will not only be a continual tendency in the former to a lowering of level and increase of density, which will place it in the same relation to the latter as the Mediterranean bears to the Atlantic; but that the influence of Polar cold will be to produce a *continual descent* of the water within its area; thus constituting the *primum mobile* of the General Oceanic Circulation, of which no adequate account had previously been given. This conclusion, as our readers will have seen, has been most explicitly accepted by Sir John Herschel.

Our limits do not admit of our following Dr. Carpenter through his discussion of the relative shares of the Gulf Stream and of the General Oceanic Circulation in that amelioration of the temperature of the Polar area, of which the industry of Dr. Petermann has collected a vast body of indisputable evidence; and for this discussion we would refer such of our readers as are specially interested in the question to the last part of the "Proceedings of the Royal Geographical Society." But as Dr. Carpenter has now shown a capacity to deal not merely with Physiological but with Physical questions, in a manner which has obtained the approval of some of the ablest physicists of our time, we hope that he will not again be accused (as he was by some of those who opposed his views on their first promulgation) of venturing beyond his depth when he began to reason on these subjects, and of advancing doctrines which his own observations refuted. The exclusive doctrine of the thermal action of the Gulf Stream advocated by Mr. Croll, rests, as Dr. Carpenter has shown, upon so insecure a basis, that a very large body of careful observations must be collected before any reliable data can be obtained as to the heat it actually carries forth from the Gulf of Mexico. And how much of this heat is dissipated by evaporation, as well as by radiation, before one-half of the Stream reaches the banks of Newfoundland (the other half having turned round the Azores to re-enter the Equatorial current), is a question which there are as yet no adequate data for determining. On the other hand, in his conclusion that a great body of Ocean water slowly moving northwards, so as to carry with it a considerable excess of temperature even to the depth of 500 or 600 fathoms, must exert a much greater heating power than the thinned-out edge of the Gulf Stream, Dr. Carpenter seems to us to have both scientific probability and common sense on his side.

## SCIENCE IN ITALY

*Atti dell' Accademia Pontificia de' Nuovi Lincei.*

*Reale Istituto Lombardo di Scienze e Lettere.* Rendiconti.

*Annali di Chimica Applicata alla Medicina.* Compilati dal Dottor Giovanni Polli.

ITALY has become a nation. It is no longer enslaved by the barbarous despotism of a single city, nor divided into mutual throat-cutting republics, nor diplomatically parcelled into heir-looms for royal families. It has at last become the country of its own people. The moral and intellectual laws of Natural Selection are now freely operating, and they will soon show what manner of people these Italians really are.

There are many ways of gauging the civilisation of a community. The consumption of soap has been suggested, and has the advantage, being numerically definite. Thus, let $s$ represent the quantity of soap used, $p$ the population, and $x$ the state of civilisation, we obtain the following equation—

$$x = \frac{s}{p}$$

and thus moral philosophy is brought within the reach of the mathematician. But there is another test which is, I think, even safer than soap, that is, the current literature of the nation, and more especially its periodical literature. The readers of *Reynolds's Weekly Newspaper* and the *Sporting Life* diverge so widely from the subscribers to NATURE, that their differences almost come within the reach of ethnological specification, and I believe that the direction of the growth of Young Italy may best be determined by watching the progress of its periodical and general literature.

It is a very interesting and promising fact that during the last two years the proportion of purely scientific works to those of light literature has been greater in Italy than in either England, France, or Germany. This refers only to original Italian works, and does not include translations. The periodicals named at the head of this article are good examples of the progress of the highest kind of intellectual culture in Italy. The first is published at Rome, the second and third at Milan. "The Transactions of the Pontifical Academy of the New Lynxes" does not belong to Young Italy, as the fasciculi before me are for the twenty-second year of the Academy, and the new Lynceans claim Pontifical patronage, and announce it in their name, which of course is derived from the old classical society of the Lynx-eyed Philosophers of the middle ages; a title which, in spite of its fanciful character, presented one of the earliest formal recognitions of the paramount importance of laborious observation, as opposed to the speculative and disputatious spirit of contemporary philosophy.*

The papers of Father Secchi and Prof. Respighi, on their spectroscopic and telescopic observations on the sun and stars, occupy a considerable portion of these Transactions. Father Secchi's observations are tolerably well known to English readers, but I doubt whether such is the case with Respighi's spectroscopic researches on "Stellar scintillation," the results of which were stated in papers read at the Lyncean meetings of May 10, 1868, and February 14, 1869. The second paper supplemented the first and embraces 720 observations, extending from October 4 to February 12. The primary results obtained by Prof. Respighi were that in the spectrum of a star near the horizon, dark lines and bright bands travel along the spectrum, most frequently from the red to the violet, but sometimes in the contrary direction, and occasionally oscillate from one colour to the other. The nearer to the horizon, the more distinct and definite are these moving lines and bands, and the more slow and regular their movements. The bright moving bands are more rare and less regular than the dark lines, and are only seen when the star is very near the horizon. Further observations brought out the law that in the normal condition of the atmosphere their motion is from the red to the violet in the spectrum of all stars on the west, and from the violet to the red when the star is in the east; while near the meridian, whether on the north or south horizon, the motion is generally an oscillation from one colour to the other; and sometimes the lines appear stationary, or traverse only a portion of the spectrum. In abnormal states of the atmosphere the moving lines are weaker and more irregular in form and motion; this is especially the case during strong winds, which sometimes reduce the movements on the spectrum to mere variations of brilliancy, even in stars very near to the horizon.

Prof. Respighi observes that these appearances are not due to an oscillation of the characteristic spectrum of the star, and that there is no superposition of its colours due to any sensible movement of the image of the star in the normal states of the atmosphere, though he does not deny that there may be such sensible oscillation and superposition, under conditions of abnormal atmospheric disturbance. He has observed that when the moving lines are regular in form and motion, there is usually a continuance of fine weather, and that the phenomena of scintillation are most marked and decided on those nights when there is much humidity in the atmosphere. He believes that by careful study of these phenomena the spectroscope may become an important meteorological instrument.

It would occupy too much space to follow Prof. Respighi through his theoretical reflections on these phenomena, which he attributes to irregularities in the temperature, and distribution of aqueous vapour, &c., in the earth's atmosphere, and the effects of its rotatory movement with the earth, which carries it across the direction of the radiation of the light from the stars.

Besides the above-named, there are some very interesting papers on electro-static induction, and electrical induction or influence in rarefied gases, by Prof. Volpicelli.

The four first numbers of the present year of the Reports of the Royal Institution of Lombardy are remarkably rich in interesting papers, extending over a wide range of subjects. Buccellati on Military Punishment, Lambrosi on the Italian Races, and Ciavarini on the Laws of Human Progress, are the chief essays in the Department of the Moral and Political Sciences. In the Department of the "Mathematical and Natural Sciences" the subjects of the papers are more varied, including, Rovida on the Pulse of the Veins, Barbieri on the Utility of Statistics of Hernia in Italy, Experimental Researches on the Origin of Fibrin, and a new Theory of the Cause of the Coagulation of the Blood, by Mantegazza, and Serpieri on the Probable Relation between the Luminous Cones (*pennacchi*) of the Solar Corona, and the Positions of the Planets.

The author of this paper supposes that the corona is an electrical phenomenon identical with that of our terrestrial aurora borealis, that the sun and all the planets mutually act and react upon each other inductively; that the cones of rays which stretch out from the corona are electrical streamers pointing towards one of the planets; that the curved lines and rays which have been observed

* The original *Academia Lincea* was founded in Rome by Federigo Cesi, the Marchese di Monticelli, in 1603. Cesi was the first president, and Baptista Porta the Neapolitan vice-president. Galileo became a member in 1611. It preceded our Royal Society and the French Academy by about half a century, and combined the functions of these societies with a scheme of philosophical brotherhood and affiliated lodges, somewhat resembling the masonic organisation. Its members were required to be "philosophers eager for real koowledge, who will give themselves to the study of nature, and especially to mathematics." They were to waste no time in "recitations and declamatory assemblies," and to "pass over in silence all political controversies and quarrels of every kind, and wordy disputes, especially gratuitous ones." They are commended to "Let the first fruits of wisdom be love; and sn let the Lynceans love each other as if united by the strictest ties, nor suffer any interruption of this sincere bond of love and faith, emanating from the source of virtue and philosophy."

are other streamers, pointing obliquely to planets or comets; that the zodiacal light is the general stream of electrical influence emanating from the sun and embracing all his planetary children; and that the sun-spots and the hydrogen prominences are due to electrical outbursts. We have had similar hypotheses put forth in England, but not so well argued as by Prof. Serpieri. Like the rest, he fails, however, to supply us with any explanation of the source of such tremendous electric energy. We may have the cylinder, the prime conductors, the insulators, the Leyden jars, and all the apparatus of a fine electrical machine; but we shall get no sparks unless somebody turns the handle. We obtain no electrical force without an expenditure of equivalent mechanical power. In the battery we must oxidise an equivalent of zinc for each equivalent of electricity produced. We know something about the laws of electrical excitation; and those who assume the existence of such huge electrical forces without indicating their origin in accordance with these known laws, only carry the problem of the source of solar energy one stage further back without advancing a single step towards its solution. Among the other papers are Lannetti on Etruscan Crania, and some on Pathology and pure Mathematics.

The Annals of Chemistry, compiled by Dr. Polli, is a carefully collected and valuable record of the progress of Chemistry, in which the subjects are classified under the heads of Pharmacy, Hygiene, Dietetics, Physiology, Toxicology, Pathology, Therapeutics, and Miscellaneous. It is published monthly in octavo fasciculi of sixty-four pages, each containing abstracts of papers from native and foreign journals. It sells at rather less than one shilling. As our *Philosophical Magazine*, with eighty pages of the same size, scarcely pays expenses at 2*s*. 6*d*., we may infer that the *Annali di Chimica*, of Milan, has a better circulation than its old-established scientific contemporary of "London, Edinburgh, and Dublin."

W. MATTIEU WILLIAMS

## SEELEY ON THE ORNITHOSAURIA

*Index to the Fossil Remains of Ornithosauria, Aves, and Reptilia from the Secondary Strata, arranged in the Woodwardian Museum, Cambridge.* By Harry Govier Seeley. 8vo. pp. 144. (1869. Cambridge: Deighton.)

*The Ornithosauria, an Elementary Study of the Bones of Pterodactyles.* By Harry Govier Seeley. With 12 plates. 8vo. pp. 136. (1870. Cambridge: Deighton.)

THANKS to the activity of the "Coprolite" workings in the Upper Greensand around Cambridge, the Woodwardian Museum possesses particularly rich series of interesting reptilian remains, especially those belonging to the *Ornithosauria* or Pterodactyles (Flying Lizards) of the Secondary rocks; to which the second work with its twelve plates is entirely devoted, as is also a large part of the Catalogue first published.

The "Index to the Fossil Remains" is introduced to the attention of the student and anatomist by a prefatory notice from the Rev. Adam Sedgwick, Woodwardian Professor, who, although in his eighty-fifth year, evinces still considerable remains of his wonted fire, when taking up his pen to write of the treasures contained in the Museum of his Alma Mater. The cost of preparing these works has been borne by Prof. Sedgwick, but the printing of both books has been defrayed out of the funds of the Syndics of the University Press.

1. The first book is intended to serve as a guide to the student in the examination of the remains of the extinct birds and reptiles preserved in the Woodwardian Museum, each case, shelf, and bone being numbered so as to correspond with the catalogue in which it is described. The list of specimens from the Cambridge coprolite bed occupies about half the book.

Many new forms are here announced by the author for the first time, as *Enaliornis* (a new bird) several new *Ichthyosauri*, a new Crocodile, 3 species of *Stereosaurus*, 2 new Chelonians; so that we have altogether 70 species from the Cambridge Greensand. There are also Chalk (8 sp.), Gault (2), Wealden (12), Purbeck (7), Potton beds (18), Portland (1), Kimmeridgian (10), Coral rag (3), Oxford clay (8), Oolites (4), Lias (20), foreign reptiles (24), making a total of 187 species.

2. In the second work, that on the Ornithosauria, the author enumerates the materials at his disposal, namely, 500 bones in one collection, and 400 in another, probably representing not fewer than 150 individuals, which well displays the richness of the area.

The bones from the coprolite diggings are much broken, but they retain sufficient character to be readily determined by the comparative anatomist.

Probably, no group of animals have caused more contention between Naturalists than the Ornithosaurians. They have been regarded as bats (Sömmering), as intermediate between birds and reptiles (Goldfuss), amphibians (Wagner), and so on. Herman von Meyer, who has paid more attention to them than any other anatomist, concludes them to be reptiles, though with strong avian affinities. Prof. Owen maintains that they are Saurians.

Mr. Seeley combats these views, and contends that the Pterodactyles were more nearly allied to birds than reptiles, and he refers them all to a new genus, *Ornithocheirus*.

He contends against the cold-blooded view taken of them by Prof. Owen, and asserts that they were warm-blooded, chiefly founding his opinion on the form of the brain. There is a very strong objection to be made against the retention of the terms "cold-blooded," and "warm-blooded," for it seems to us that the heat developed by the animal's body is in direct proportion to the work to be performed. Thus, in aërial locomotion, the efforts of the pectoral muscles to sustain the body in the air, necessitate also a correspondingly more rapid action of the heart and lungs, producing, therefore, more rapid circulation, and an increased bodily temperature. We are therefore inclined to agree with Mr. Seeley on the grounds that, in proportion to the rapidity and the sustained action of the great motor muscles of the body (whether of legs or wing) so will be the rapidity of the action of the heart and lungs, and consequently the acceleration of the temperature of the whole body.

The bones from the Cambridgeshire Greensand are very often so fragmentary that their determination requires the most exact anatomical skill, and we think the plates would have been more useful if in a few instances (perhaps in all) the missing parts and processes had been indicated in outline, so as to help to the better understanding thereof by the student.

H. WOODWARD

## OUR BOOK SHELF

*Handbuch der Systematischen Anatomie des Menschen.* Von Dr. J. Henle, 1 Band, 1 Abtheilung, Knochenlehre, 3 Auflage, pp. 310. (Braunschweig, 1871. London: Williams & Norgate.)

It is unnecessary to commend the work of Prof. Henle, which is on the whole the most full and exact yet published. It shares the richness and accuracy of its illustrations with the last edition (the fourth) of Cruveilhier's great work, and shares with it the serious disadvantage of being incomplete. Indeed, while in the latter the part relating to "Angéiologie" which includes the description of the heart, blood-vessels and absorbents, was published in 1867, preceding the completion of the second volume on visceral anatomy in the following year, the third volume of the German work, with the whole of the nervous system, has not yet appeared. In this respect the only English work on descriptive anatomy which can rival Henle's has a great advantage; each edition of what was originally Dr. Quain's Anatomy has been published complete, and on this ground, as well as that of conciseness, the last edition of this work may, with the help of Prof. Sharpey's masterly introduction on general anatomy, take rank with those of France and Germany.

The department of osteology is not that which Prof. Henle has done best. In minute accuracy of detail it is decidedly inferior to Mr. Ward's treatise, which at least equals the best efforts of the French School of Anatomy. And there is a want of attention to broad views of morphology almost as conspicuous as in M. Cruveilhier's work. Thus the comparison between the upper and lower extremities (pp. 226—229) is very insufficient, giving no account of the important and opposing views which have been maintained on this subject, and admitting the demonstrably false position that the radius answers to the fibula, and the ulna with the olecranon to the tibia with the patella. The difficult subject of the homologies of the cranial and facial bones is also entirely omitted, an omission rendered necessary by the absence of any account of their fœtal development. The rigid specialisation of human osteology so as to exclude all reference to embryology and comparative anatomy on the one hand, and on the other to the mechanism of the skeleton, makes what ought to be the most interesting part of anatomy the most arid and forbidding. In the last edition of "Quain's Anatomy" we have within a shorter compass a good account of the antecedent development, as well as the mere ossification of the several bones, with illustrative diagrams, and a sufficient account of its homologies to awaken interest in this attractive study. On the other hand, there is nowhere to be found so complete an account of Abnormalities as in Prof. Henle's work, a subject of which the importance is only beginning to be recognised in England. The references to observations in this branch of the subject are very full, and include many only lately published. On this, as on other points, the author has added many fresh facts in the present edition. On the whole, however, it differs but little from the first issue in 1855, and the number of woodcuts remains the same. Among the more important additions may be mentioned one on the differences in the skull of the two sexes (p. 216). No mention is made of the little tympano-hyal bone described by Prof. Flower, and even the ordinary variations of the styloid process, which throw so much light on its homology, are scarcely alluded to.

In conclusion, every anatomist will acknowledge the industry and care with which even small advances in knowledge are added in this edition, but will also hope that nothing may delay the appearance of the volume which is to complete the whole treatise, and no doubt complete it worthily of its distinguished author, and of what he has already published.

H. P.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Science Lectures for the People

Of the justice of your remarks on "Science Lectures for the People" there can be no doubt whatever. The lectures in question are perfect models of what lectures should be, and while reading them I pictured to myself the rich feast that had been prepared for the people who were fortunate enough to hear them—especially for those who had some previous acquaintance with the subjects of which they treat. They are couched in simple language, so that those who run may read. They are strictly to the point, well calculated to excite further inquiry, and in every way adapted for the purpose for which they were intended. It may be, however, doubted, whether lectures on scientific subjects before the general public, however delivered, do that amount of good which they certainly ought to do. A lecture to be thoroughly and lastingly effective presupposes a certain acquaintance with the subject already. To listen even to the most brilliant and never so simply worded address on Spectrum Analysis or Coral Reefs, has a very transient effect, I take it, upon those who have rarely or never heard of such things. However praiseworthy, therefore, every effort to scatter scientific knowledge among our population may be—and it certainly deserves every commendation—my decided opinion, arrived at after large experience with the people in towns and country, especially the latter—is that it will fail, unless we begin with the young. People in masses may be compared with fuel laid in the grate. If you ignite it from the top, a considerable time will elapse before it reaches the whole mass; but if the fire be applied from below, the course is more rapid, and the fuel sooner feels the effect. So with science teaching, or any teaching, we must begin in our schools. Every school, from the primary to the highest, must be opened to its influence. Teachers, I am sure, would welcome the innovation, for it would dispel many a weary hour both for teacher and taught. The everlasting monotony of reading, writing, arithmetic, and scripture, would be enlivened by simple explanations of the human body, plants, &c., and thus children would be taught to take an interest in all matters connected with their future welfare even from their infancy. The same remarks, slightly modified, would apply to many of our middle class and upper schools; for scientific matters, in far too many cases, have still to find a place even here—parents being themselves quite as much, in many instances far more, to blame than the regulations of the school.

It is precisely owing to this want of early training, and consequently to an utter ignorance of the subject, that the lectures on divinity, science, &c., in our universities are of such little real value, and of such little interest to the students. They attend them, it is true, not from any genuine love, but simply because they must attend some for certificates or otherwise. No fault whatever attaches to the lectures themselves; on the contrary, they are of the greatest possible value, and had the students themselves been trained properly and gradatim when at school, the attendance would be vastly increased, a genuine love for the lectures would be engendered, and incalculable results would be the consequence. Or take another instance—our farmers' clubs. With laudable zeal these have been formed all over the kingdom. Lectures on scientific subjects connected with agriculture are delivered from time to time. All very admirable no doubt in its way. The attendance generally is good, but from the vacant stare, the nodding head, and subsequent remarks, nothing can be clearer than that nine-tenths of the lecturer's address on the abstruse niceties of chemical analysis, &c., have been utterly thrown away. What subjects can be more valuable to a farmer than a knowledge of the constituents of the air, the origin of soils, the inner life of plants, the wonderful dependence of animals and plants upon each other, the means of judging artificial manures, &c.; and yet, except among the upper favoured few, utter ignorance of these matters almost universally prevails. It is not from indifference to the subject, far from it, but, as in the former case, from a want of early training in this particular line of thought. The farmer acts just as his father acted before him. He is of all people the most backward in leaving the old routine, and considers such subjects as geology and botany altogether beside the purpose, and a waste of time for his children to learn, though he will praise them in the same breath.

There is nothing more trying for a master's patience—and I speak from experience—than this persistent and short-sighted adherence to what has gone before, just as if the world (the agricultural world particularly) had to jog on to the end of time in the self-same fashion.

Whatever united action, therefore, may be taken by our leaders in science for bringing about a more healthy feeling on this subject, for scattering science and a love for it in every household, depend upon it the readiest and surest way will be to urge on Government to introduce, nay, force, the subject freely and universally into all schools, so that it may grow up with the rising generation, and become a part of their very existence. The task is Herculean, no doubt. An enormous amount of prejudice will have to be overcome, but

Sedit, qui timuit ne non succederet : esto :
Quid? qui pervenit fecitne viriliter?

Lectures on science will thus be not merely listened to as now, but understood and appreciated. Superstition, the child of Ignorance, will be dispelled, and a nation of reasoning and thinking men and women inaugurated as the glorious and inevitable consequence. THOMAS FAWCETT

Blencowe School

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### Preponderance of West Winds

I HOPE you will publish this reply to Mr. Laughton's letter in NATURE of May 4, on the Prevalence of West Winds.

He maintains from statistical evidence that west winds occupy a greater portion of the earth's surface than east winds; that their force is greater; and that in the upper regions of the atmosphere the preponderance of west winds is still more decided than at the earth's surface; so that on the whole the atmosphere moves round the earth from west to east.

It is in my opinion certain that this is on the whole proved. I do not question Mr. Laughton's facts but his inferences from them. He thinks this rotation points to some force acting from without—some cosmical cause of a nature quite unlike the sun's heat. I maintain, on the contrary, that all the phenomena of the great atmospheric currents, of which the trade-winds are a part, are to be accounted for by the heat of the sun as the motive power, combined with the rotation of the earth as a modifying influence.

In discussing the question of whether the phenomena point to such a cause as that suggested by Mr. Laughton, the motion of the upper strata of the atmosphere is quite unimportant. It is only the currents at the surface of the earth that can in however infinitesimal a degree increase or diminish the velocity of the earth's rotation; and if the circulation of the atmosphere is due to the sun's heat as its motive power, it cannot have the slightest effect on the earth's rotation; while if it is due to any mechanical force acting from without, as Mr. Laughton thinks—if the Cartesian theory is true, and the circulation of our atmosphere is part of a cosmical vortex—the earth's rotation must be accelerated by its friction. This follows from the simplest dynamical principles. It is true that the acceleration which could be produced in such a way would at the greatest be far too small for us to detect; but it is quite possible for us to ascertain whether or not the currents of air that sweep over the surface of the earth are by their united action capable of affecting its rotation; or, to state the problem more definitely, whether or not the effect of west winds in accelerating the rotation is balanced by the effect of east winds in retarding it. I maintain that such evidence as we have tends to the conclusion that the effects of the two are so balanced.

The separate effect of any wind covering a given area on the earth's rotation = the east and west component of its force × the radius of the parallel of latitude. The latter factor gives leverage. An east wind near the equator has more effect in retarding the rotation of the earth than a west wind of equal extent and force at a higher latitude has in accelerating it, just as a weight at the end of the long arm of a lever outweighs an equal weight at the end of the short arm. Now, the east winds, under the name of trade-winds, are chiefly to be found in the lower latitudes, and for the reason just given they are able to balance the west winds, which are certainly more forcible, and according to Mr. Laughton, occupy a greater area, but being at higher latitudes act at a disadvantage. If it can be shown—and the facts certainly point to it—that the total mechanical effect of the winds is not such as to produce any effect on the earth's rotation, this goes very far to prove that they have no motive power except the sun's heat.

But how is the motion of the upper strata of the atmosphere from west to east to be accounted for? The answer to this will involve the entire theory of the great currents of atmospheric circulation. There is always a current of air towards a heated place along the earth's surface, like the draft towards a fire, and a compensating current of air away from it in the upper regions of the atmosphere. The equatorial latitudes being the hottest, there are currents to them from the higher latitudes, which bring with them the smaller velocity due to the rotation of the higher latitudes, and thus move less rapidly than the earth in those lower latitudes to which they flow. Moving with a less velocity than the earth is the same as moving from the east, and thus are the trade-winds constituted; they are from north-east in the northern hemisphere and from south-east in the southern. This is generally understood and believed; what follows is less generally understood, though I claim no originality for it.

The air rises up over the equatorial regions like a column of smoke over a fire, and flows off towards the poles. Coming from the latitudes where the velocity of the earth's rotation is greatest, it carries that greater velocity with it, and spends the energy of its motion in the form of the west winds of the higher latitudes. The reason, then, that the upper strata of the atmosphere (in all latitudes except on the equator) have a motion round the earth from west to east, is simply that they are at the same time moving from latitudes where the velocity of the earth's rotation is greater to latitudes where it is less. JOSEPH JOHN MURPHY

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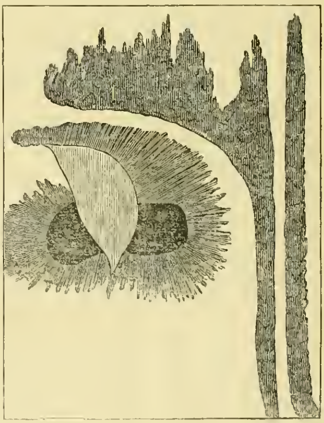
### Remarkable Sun-spots

THE accompanying sketch shows in a rough way the umbræ and a small portion of the penumbra of a sun spot that I observed on the 6th and 7th of this month, and which was made remarkable by the presence of a reddish-brown object like a cloud, that seemed to hang over the nucleus of the principal umbra, apparently dividing it in two. Could this object be seen without the intervention of the dark glass, it would doubtless show a bright red instead of a reddish-brown colour; and from its fog-like aspect, though it was well defined in outline and acuminated at both ends, the impression was inevitable that it hung at a certain altitude above the spot. However, it evi-



dently had no motion distinct from the latter, as on the 7th it occupied the same position as on the day before, but it was much reduced in size. On the 8th it was seen no longer, and the nucleus was now in one, seeming to show pretty clearly that its previous apparent division in two was really caused by the intervention of the brown cloud suspended over it, and that the phenomenon did not consist of two distinct neuclei with the brown object lying between them. I am not aware that anything like this was observed before. J. BIRMINGHAM

Millbrook, Tuam, May 18

## ANNUAL VISITATION OF THE ROYAL OBSERVATORY

ON Saturday last the Board of Visitors of the Royal Observatory made their annual visitation to our National Observatory to examine into the work done, and to receive the Report of the Astronomer Royal. We have not space for the report in full, but this is not to be regretted, for it contains a quantity of minute detail about trivial matters which the ordinary run of mankind would not think worth the printing.

There are, however, several points of great scientific interest in the Report, the new Water Telescope and the instruments for use in the approaching observations of the Transit of Venus having been the lions at Greenwich.

The correction for level-error in the transit instrument having become inconveniently large, owing apparently to a gradual subsidence of the eastern support since the erection of the instrument, about a ton weight of stone was placed on the western pier. Not the slightest change, however, could be traced as due to this; the level-error maintaining its usual value. This plan having failed, the stones were removed, and a sheet of very thin paper, $\frac{1}{270}$ inch in thickness, was placed under the eastern Y, which was raised from its bed for the purpose. The collimators having been observed just before this operation, no difficulty was experienced in adjusting the instrument so as to have very nearly the same error of Azimuth as before. The mean annual value of the level-error appears to be now sensibly zero. This will give some idea of the delicate arrangements necessary for this ponderous instrument.

The usual course of Astronomical observations now carried out for so many years has been adhered to. The list of fundamental stars used for determination of clock-error has been increased to 210. Besides these, Nautical Almanac, circumpolar, and moon-culminating stars have been observed; also refraction stars, stars with large proper-motion, and stars which are required for any special investigation. A few of Bradley's stars which had been inadvertently omitted have been observed in the past year. The Sun, Moon, and large planets have been observed as usual. As the siege and war operations in Paris seriously interfered with the observations of small planets made at the Paris Observatory, observations of them were continued at Greenwich throughout each entire lunation during the investment of the city.

The observations of $\gamma$ Draconis, the star which passes very near the Zenith of Greenwich, with the Water Telescope, made in the spring of the present year, are completely reduced, with the exception of a small correction for the positions of the micrometer-wires, to be determined shortly. As the astronomical latitude of the place of observation is not known (it is not many yards from the transit circle), the bearing of these observations on the question of aberration cannot be certainly pronounced until the autumn observations shall have been made; but, supposing the geodetic latitude to be accordant with the astronomical latitude, the result for aberration appears to be sensibly the same as with ordinary telescopes.

With regard to the Magnetic instruments, the Astronomer Royal states that a plan was arranged last year for photographic impression of hour-lines upon the photographic sheets carrying the records of the three Magnetometers and of the Earth-current Galvanometers: an arrangement already for some time carried out in the new instruments by Dr. Balfour Stewart. The beam of light, constantly directed through a cylindrical lens to fall upon the sheet, emanates from one of the existing lamps, or (in one instrument), from a flame specially mounted for it; it is, however, obstructed by a shade till $2\frac{1}{2}$ minutes before each hour, and acts till $2\frac{1}{2}$ minutes after each hour. The connections of the shades were so arranged that all could be opened and closed by a single wire.

The following numerical results of the magnetic elements for 1870 may be interesting:—

| | |
|---|---|
| Mean westerly declination . | 19° 54′ nearly. |
| Mean horizontal force . . . | 3·865 (in English units).<br>1·782 (in Metrical units). |
| Mean dip . . . . . | 67° 51′ 9″ (by 9-inch needles).<br>67 52 25 (by 6-inch needles).<br>67 53 41 (by 3-inch needles). |

The observations of dip at different hours appear to show a diminution from 9h. A.M. to 3h. P.M.

A small Appendix of great interest is attached to the report. Those who have given attention to the history of Terrestrial Magnetism are aware that Halley's Magnetic Chart is very frequently cited; but the Astronomer Royal could not learn that any person, at least in modern times, had ever seen it. Inquiries were made of nearly all the principal scientific bodies in Europe, and in several of the best continental libraries in vain. At last, by the assistance of Mr. Winter Jones, Principal Librarian of the British Museum, a copy was discovered in the library of the Museum. 600 copies have been taken in a reduced size, for insertion, as an Appendix, in the Magnetical and Meteorological volume for 1869.

On the subject of Chronometers it is remarked, "The performance of chronometers, as depending on their mechanical construction, is very admirable; I have remarked but one point on which I could desire change, namely, that the balance should be struck more lightly, at a greater distance from its axis; the late Mr. Charles Frodsham, at my suggestion, had made experiments on this point, which promised to be successful. The principal errors of even moderately good chronometers are, however, produced by defective compensation, which the most skilful makers cannot perfectly manage. I have long been of opinion that the final adjustment for compensation ought to be made by some more delicate operation than that which suffices for approximate compensation; but the able chronometer-makers whom I have consulted have not yet devised a satisfactory plan."

With reference to Time Signals, we read that a proposal has been made to have a time-ball dropped at Queenstown, and that the report of the Westminster Clock shows that 55 per cent. of its errors are under $1^s$, and 94 per cent. under $3^s$.

In December two attempts were made to determine the longitude of Gibraltar, at the request of Professor Newcomb, but without success, the cable connecting Falmouth and Gibraltar being out of order.

During the year the time of the Astronomer Royal has been partly occupied in preparations for the Transit of Venus, 1874. Measures have been taken for equipping each of five stations with a Transit, an Altazimuth, and an Equatoreal. Some other instruments mounted in temporary observatories were inspected by the visitors. Of Transits there are five new, all mounted on stone piers. Of clocks to accompany them, there are two from the Royal Observatory, three new. Of Altazimuths, one from the Royal Observatory, four new. Of Equatoreals, 6 inches in aperture, and carried by clock-work, there are five, purchased or new. Of clocks of an inferior class, to accompany the two last classes of instruments, one can be supplied, nine must be procured. Fifteen portable observatories must be prepared, of which specimens were exhibited to the visitors. The Royal Observatory can supply three 4-inch detached telescopes, and two more will be desirable.

The report goes on to say:—"My preparations have respect only to eye-observation of contact of limbs. With all the liabilities and defects to which it is subject, this method possesses the inestimable advantage of placing no reliance on instrumental scales. I hope that the error of observation may not exceed four seconds of time, corresponding to about $0''\cdot13$ of arc. I shall be very glad to see, in a detailed form, a plan for making the proper measures by heliometric or photographic apparatus; and should take great interest in combining these with the eye-observations, if my selected stations can be made available. But my present impression is one of doubt on the certainty of equality of parts in the scale employed. An error depending on this cause could not be diminished by any repetition of observations. As, in the event of any national enterprise being promoted in the direction of photographic record, it is probable that the Astronomer Royal may ministerially take an important part, I venture to submit to the Board of Visitors that suggestions on the value and plan of such observations fall entirely within their competence."

All the American observers of the Solar Eclipse, as well as M. Janssen, have visited the Observatory during the past year.

The current reductions of observations, it is remarked, are in a healthy state. Regular reductions give, "in general, great facility for the most advanced inferences; the star-catalogues, and solar, lunar, and planetary errors, lend themselves immediately to investigations of a physical character; the magnetic reductions distinctly, though tacitly, exhibit some of those results (for instance, annual inequalities) which in various observatories have been the subject of special memoirs.

"But from time to time it becomes desirable to unite some of those annual or nearly annual results in groups, so as to exhibit the results justly derivable from masses of observations extending over long periods of years. These operations require new organisations; and, what is worse, they require additional grants of money. I have usually refrained from asking for these, without the distinct approval of the Visitors. I would now submit for their judgment the following subjects:—

"The vigorous prosecution of the Meteorological Reductions (exhibiting the results deducible from the photographic registers) already begun.

"The combination of the results of Magnetic Observations on disturbed days, from the year 1864.

"The discussion of Magnetic Storms, from the year 1858.

"Perhaps, also, the discussion of observations in groups depending on Lunar Declination, or other phases."

The report concludes as follows:—"There is another consideration which very often presents itself to my mind: the waste of labour in the repetition of observations at different observatories. The actual Greenwich system was established when there was little to compete with. Other observatories have since arisen, equipped with and principally using the same classes of instruments, and devoting themselves in great measure to the same subjects of observation (except the unrelenting pursuit of the moon, and perhaps the fundamental elements of the ecliptic). Ought this Observatory to retire from the competition? I think not; believing that there is greater security here than anywhere else for the unbroken continuity of system which gives the principal value to series of observations. Still, I remark that much labour is wasted, and that, on one side or another, that consideration ought not to be put out of sight in planning the courses of different observatories."

This is a very broad hint for some English as well as Foreign Observatories, and it will be well for the cause of Science if the directors of those observatories will take it.

## *THE SCIENTIFIC VALUE OF CHEESE-FACTORIES*

THE American system of cheese-factories was established nearly twenty years ago, and in its present condition of maturity it retains all the essential features which were characteristic of its infancy.* The test of twenty years' experience in a country where apparent improvements are eagerly submitted to a fair trial is amply sufficient to prove the success of the system. Recently the question of its adaptability to English dairy districts has acquired considerable prominence in agricultural circles, and is now passing from the stage of discussion to that of experiment. The two great merits which are claimed for it are, economy in the labour of production, and superiority of quality in the produce. It is evident that if a dozen farmers convey their milk to one building (a factory) to be made into cheese or butter, fewer hands are required to perform the work than if the process were carried on at a dozen different places by as many sets of people. The factory can be furnished with better labour-saving machinery than the farm-dairy, and the former establishment requires no more supervision than the latter. The process of cheese-making, also, occupies practically the same length of time, whether the quantity of milk under treatment be large or small, so that two or three persons whose energies are concentrated at one place will produce as great an economic result as a dozen or more who are necessarily employed at as many different points, each one going through the same routine independently of the other.

The superiority in the quality of the manufactured article may be more difficult of explanation, for the best farm-dairies produce as good cheese as any factory. The reason why the establishment of factories has improved the *average* make of cheese is because fewer first-rate cheese-makers are required under the factory system. But when Mr. Jesse Williams established the first factory twenty years ago, the great bulk of American cheese was extremely poor, and for many years after it was almost unsaleable in the English market. At the present day, on the contrary, it can compete on even terms with all but the very choicest English makes, notwithstanding that it has to undergo the ordeal of a long sea-voyage. The factory-system, therefore, has not only improved the *average* quality of American cheese, but it has very considerably raised the standard of the choicest brands.

Students of nature are perfectly well aware that the most sure and rapid progress is made by means of association and co-operation. The same phenomena are observed from different points of view by workers in the same field; a comparison of their notes leads to the grouping of kindred facts; the apparent exceptions are seen to be the product of attendant variations in the methods or circumstances of observation; and by a process of induction an explanatory theory is arrived at, to be confirmed or rejected by future investigations. In this manner the cheese-factory system has gone far towards the establishment in America of a science of cheese-making. Each factory has been the theatre of exact observations, which have been duly recorded. The results of comparisons of these records have been embodied in papers read before the American Dairymen's Association; and the conclusions of the authors have been frequently put to the crucial test of experiment.

The American Dairymen's Association is only a child of the Factory-system. It is organised on a plan similar to that of the British Association for the Advancement of Science, and like that institution, holds an Annual "Convention," at which papers are read and lectures are delivered. These contributions to the literature of dairying, and the discussions thereon, are published in an annual "Report," which also contains detailed reports from numerous cheese and butter factories, giving the dates of commencing and finishing work, the number of cows supplying the factory, the quantity of milk received, the quantity of cheese made, the percentage of cheese to milk at different periods of the year, and as compared with

* For detailed descriptions of this system, *vide* Journal Royal Agricultural Society, 2nd Series, vol. vi. p. 173, and vol. vii. p. 1.

previous years, as well as other data, including peculiarities in modes of manufacture, which may be useful for comparison with the methods pursued and the results obtained at other factories. There can be no doubt that these efforts must sooner or later result in the formation of a dairy science, and in the establishment of sound theories of dairy management.

But the functions of the American Dairymen's Association are not confined to observation and experiment at home. Already the inquiries of its officers have enabled its members to improve their cheese-making practice by adopting some features of our Cheddar system; and in the last volume of the Report of the Association is an able paper by Prof. Caldwell,* showing some features common to the numerous cheese-making processes followed in Holland, Switzerland, France, and Italy. One of the most interesting points brought out is the intimate connection that exists between the ripening of cheese and the development and growth of *Micrococcus* and other forms of mould. As a matter of commerce it is important to the farmer to ripen his cheese as soon as possible. This is done in various ways, all having for their object the introduction of large numbers of germs of the appropriate fungus. The ripening of Stracchino cheese is thus induced by the introduction of layers of old curd; that of Roquefort by an admixture of mouldy bread, containing germs of *Penicillium*, and that of Brie by packing the thin cheeses between layers of musty hay. Another observation of interest is, that the presence of free ammonia in the curing-room hastens the ripening of the cheese, a fact which may have some bearing on the well-known property of American cheese (which is always packed in boxes) to ripen more rapidly than English makes.

These evidences of a process of scientific investigation induce us, therefore, to regard the factories, or associated dairies, as they are termed, as possessing a scientific value, both as educational establishments and as laboratories. But, it may be asked, why is this not true also of the farm-dairy? Our answer is, that while the manager of a factory makes cheese-making his sole business, his success in which depends entirely on his skill and knowledge, the English dairy-farmer has little or nothing to do with cheese-making, but occupies himself with the management of his farm. With the production of the milk his supervision ceases, and the manufacturing process is either carried on by his wife, who has household cares to occupy her time and thoughts, or by a dairymaid, who has no interest in the matter, and who knows that her services are at a premium.

Thus, with the exception of the additions to our knowledge of the *rationale* of cheese-making, for which we are chiefly indebted to Dr. Voelcker's chemical researches,† the manufacture of dairy products in England can hardly be said to have advanced during the last half century, while it has made enormous strides in America during the last ten years. Let us hope that the establishment of cheese-factories in England, commenced last year at the risk of some liberal-minded Derbyshire landlords,‡ may also be the dawn of an English era of progress in this most important agricultural industry.

* Sixth Annual Report, Syracuse, N.Y., 1871, p. 25.
† Vide Journ. Royal Agric. Soc., vol. xxii. p. 29, and vol. xxiii. p. 170.
‡ Ibid, 2nd series, vol. xii. p. 42.

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## *HYDRAULIC BUFFER FOR CHECKING THE RECOIL OF HEAVY GUNS*

THE ingenious instrument, the name of which stands at the head of this paper, deserves some notice, not only on account of its utility for its purpose, but as an interesting method of meeting and overcoming those violent efforts of nature to which she is provoked by explosion. In the recoil of a heavy gun, we have an example of the greatest force which man attempts to control. The inventions of Captain Moncrieff, which no long ago formed the subject of an article, seek to utilise this force; other gun carriages lead it to expend itself as harmlessly as possible.

The Hydraulic Buffer accomplishes this latter object in a manner very ingenious, and affording some interesting illustrations of Nature's laws; it also possesses several advantages over other methods which have been and are still used. For it the public service is indebted to Colonel Clerk, R.A., F.R.S., Superintendent of the Royal Carriage Department in Woolwich Arsenal. Before the introduction of the Hydraulic Buffer into the English service, and in those cases where it is not yet applied, the method employed to overcome the recoil was the friction of iron plates. To the bottom of the gun-carriage several plates are fixed, which pass between long plates placed along the middle of the slide or platform on which the carriage runs; and the friction of their surfaces in contact overcomes the force of the recoil, and brings the gun and carriage to a standstill. The amount of the friction can be regulated by the compression given to these plates, and requires to be altered for the various charges used. The compression must be taken off to allow the gun to be run forward to the firing position, and must be again set up to meet the recoil.

The Hydraulic Buffer, on the other hand, is always ready for use, and never needs any adjustment. This is one of its advantages, and one which is of special importance in the heat and excitement of action. It consists of a cylinder (A B in figure) placed in the platform, and lying along its length. In the cylinder is a piston pierced with four holes, and the extremity of the piston-rod is attached to the carriage. When the gun and carriage are run out for firing, the piston is moved to the lower end of the cylinder (A), which is filled with water, except a small air-space exceeding slightly the cubic content of the piston-rod, so as to allow for the displacement of the water when the piston is driven to the other end of the cylinder. When the gun is fired, and with its carriage begins to recoil, the piston is driven back into the cylinder. The first effect of this is to compress the air in the cylinder very violently, then the water begins to run through the four holes in the piston, this motion soon attains a very great velocity, and in imparting this to the water, the force of the recoil is soon exhausted. It is spent in transferring the water with very great rapidity through these orifices from one side of the piston to the other.

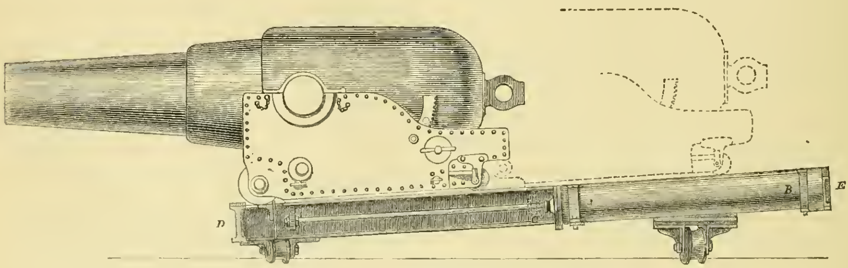
This rapidity depends on the ratio of the area of the piston to the area of the four holes in it. A very small diminution in the area of these orifices would cause the recoil to be checked very much sooner; a correspondingly slight increase would allow the piston to strike with violence against the end of the cylinder. It was found in an experiment with a 20-pounder gun, that when the holes were 0·562 of an inch in diameter, the recoil extended the whole length of the cylinder, 2ft. 9in., and struck violently the end of it; when a piston was used with holes 0·437 in. in diameter, the recoil was only 1ft. 11in., and ended quietly, the same charge being used. In another experiment with a 12-pounder gun in a boat carriage the holes in the piston were five-eighths of an inch in diameter, the recoil was 2ft. 2in.; when the diameter of the holes was increased by one-sixteenth of an inch the recoil was 3ft. 2in.* The proper ratio of the area of the holes to the area of the piston is evidently that which will allow the recoil to expend its force in nearly, but not quite, the whole length of the cylinder. When once this ratio is fixed, it is very remarkable that the amount of the charge, or the slope at which the platform is placed, whether up or down or

* The reason of this is evident from a little consideration: first, every addition to the area of the holes diminishes the area of the piston, which acts on the water; secondly the difference of the work done by the recoil is proportional to the difference of the *squares of the velocities* given to the water in passing through the orifices in the two cases.

horizontal, makes comparatively little difference in the length of the recoil. With a 12-ton (300 pounder) gun a service charge of 30 lbs. of powder gave a recoil of 4 ft. 5 in.; with a battering charge of 43 lbs. the recoil increased only to 5 ft. 1 in.. If the charge is heavy, or if the slope favours the recoil, the carriage will not go much further back than if these conditions are reversed. But it will do so more rapidly. The space travelled over is not much greater with the violent recoil, but it is done in a shorter time. It is also worthy of notice that quick burning powder, such as the rifled large grain, does not give so long a recoil as the slow burning ones, such as the pebble and pellet powders, although it acts much more violently on the gun; the reason is that the recoil is more rapid. Few machines give so striking an illustration of how important an element is time in work to be done, and how much force is to be increased if anything is done more rapidly. The strength of one man is quite sufficient to push in or pull forward the piston of the Hydraulic Buffer, because he does it quietly, "takes his time to do it." The force of a 25-ton gun, recoiling from the discharge of 70 lbs. of powder, and a 600 lb. shot exhausted itself in doing the same, because it does it so quickly.

In fact, the ease with which the hydraulic buffer permits slow motion is one of its disadvantages, and prevents its application to sea service carriages, as it would not keep the carriages from moving as the ship rolled. A modification to obviate this difficulty has been proposed. It consists of a solid piston (without holes), and the back and front ends of the cylinder are connected by a pipe through which the water is driven by the recoil. The motion of the water can be stopped altogether by the stop-cock till the gun is fired, and the area of the orifice through which the water is to pass can also be regulated by it.

The resistance of the water, and consequently the pressure on the cylinder from the recoil, is not uniform. It becomes greatest at the moment when the air receives its maximum compression, before the water attains its highest velocity in passing through the holes in the piston. At this point the force of the recoil is felt as a severe strain upon the cylinder and the platform which holds it. This destructive action of the recoil of heavy guns not only upon platforms, pivots, and racers, but also upon the foundations on which they rest, is one of the great difficulties with which modern military engineering has to grapple. To remedy this disadvantage by causing the recoil to meet with a gradually increasing resistance,




A B cylinder; C end of piston-rod attached to carriage; C′ end of piston-rod after recoil; D E slide or platform. The dotted lines show the position of the carriage and gun after recoil.

so that its force may be felt as a continuous pressure, and not at any point as a shock or blow, the following very ingenious arrangement was proposed by Mr. H. Butter, Chief Constructor in the Royal Carriage Department. It consists in placing along the length of the cylinder and through the holes in the piston four tapering rods, the largest extremities of them being at the rear end of the cylinder, and being of such a size as there to fill completely the piston holes. These orifices and also the whole cylinder must be larger than where the rods are not employed. The effect in this case is that, as the area for the water to flow through the piston is continually diminishing as the holes get further along the rods, the force of the recoil has to impart a continually increasing velocity to the water, and is at no point felt as a shock or blow. The resistance, slight at first, gradually increases throughout the recoil, and so exhausts its force not at any one point, but throughout the whole of its course.

It has been suggested, and it is a consummation most devoutly to be wished, that the Hydraulic Buffer might be applied to railway trains so as to take away the destructive effects of a collision. A train of carriages separated by Hydraulic Buffers would, if suddenly stopped at a high speed, simply close up, the piston being driven in, and the force of the collision would exhaust itself in the motion given to the water in the cylinders. Some practical difficulties stand in the use of this application of the invention; principally, that the length of the piston rods would inconveniently increase the length of the train. But there are none which might not be overcome by a little ingenuity; and the great importance of the object to be gained makes the neglect of any promising means to attain it highly culpable. However, slowness in taking up new ideas (especially if they do not immediately add to dividends), is not altogether a peculiarity of Government departments.

A very interesting pamphlet on this subject has been published by Colonel Clerk, in which Mr. Butter shows the work done in the Hydraulic Buffer, by comparing it with the moment of a similar weight of water falling through such a height as to give it the same velocity as that with which it passes through the holes in the piston. By this ingenious comparison he ascertained that a locomotive engine, weighing 50 tons, and moving at the rate of 30 miles an hour, would be brought to rest in the space of six feet by two Hydraulic Buffers of 12in. diameter. "There are," Colonel Clerk remarks, "two important problems to be worked out by the railway authorities:—(1) to have no railway collisions; (2) if they must sometimes occur, to render them as harmless as possible;" and it is with the second that he deals. The plan which has been so successful in meeting the violence of exploding gunpowder, should, at least, have a trial in a case of far greater importance—security to life in railway collisions. To refuse this, on account of a few difficulties or inconveniences, seems a sin against Nature herself.

## NOTES

IT will be welcome news to astronomers throughout the world to hear that the Board of Visitors of the Royal Observatory have determined to ask the Government to grant the sum of 5,000*l.* to enable *photographic* observations to be made of the approaching Transit of Venus. It is a matter of wonder that now, when the labours of De la Rue and Rutherford have brought this most perfect means of astronomical record to a pitch of perfection which it is scarcely needful to surpass, it is still ignored in official observatories. In the matter of the Transit of Venus, it will more than double the chances of success, and Mr. Rutherford has shown that in other inquiries it enables an only moderately skilled person to do in a month what a Bessel would require years to compass by the old method. There is no doubt that the appeal to Government will be successful. Would that we had a Physical Observatory and a Board of Visitors to look after other phenomena which we are now neglecting, the observation of which is even of more importance in the present state of science than that of any number of Transits of Venus!

AT the conclusion of a recent lecture at the Royal Institution Dr. Carpenter "expressed the earnest hope that the liberal assistance of Her Majesty's Government, which has already enabled British Naturalists to *obtain the lead* in this inquiry, would be so continued as *to enable them to keep it for the future.* In particular he called attention to the suggestion lately thrown out by M. Alex. Agassiz that an arrangement might be made by our own Admiralty with the naval authorities of the United States, by which a thorough survey, Physical and Biological, of the North Atlantic should be made between the two countries, so that British and American explorers, prosecuting in a spirit of generous rivalry labours most important to the science of the future, might meet and shake hands on the mid-ocean." We fear that if we do not bestir ourselves the credit which has been won by British scientific enterprise will pass elsewhere. The United States Government is not only preparing the Deep-sea Exploring Expedition, of which we gave an account in our last issue, but is also fitting out a similar expedition to the North Pacific. The German Government is about to send a like expedition to explore the depths of the Atlantic to the west of Portugal, where the *Porcupine* Expedition of last year met with its greatest biological successes; and the Swedish Government has already despatched two ships expressly fitted for deep-sea exploration, to Baffin's Bay; the Natural History part of the work being under the charge of Mr. Lindape, who acted as assistant-naturalist in the last *Porcupine* Expedition. What are our Scientific men as a body, what is our Government doing? We grieve to say that up to the present time we have heard of no general appeal to the Government for the required help. And thus, having shown other nations the way to the treasures of knowledge which lie hid in the recesses of the ocean, we are falling from the van into the rear, and leaving our rivals to gather everything up. Is this creditable to the Power which claims to be mistress of the seas? Is it fair to the eminent men who have freely given their best services to the nation, and obtained for it a glorious scientific victory? If their success is regarded by other countries as so distinguished that they are vieing with each other for a participation in it, surely we ought at least to *hold our own.*

A PARAGRAPH has appeared in several papers to the effect that the lime light is to be displayed on the great clock tower at Westminster during the sitting of Parliament. We are glad to announce that the light in question will not be the lime light, but a much more brilliant one—the magneto-electric. Such a light has now for some time past shone from the Capitol at Washington, and was under the consideration of Lord John Manners some few years ago. Mr. Ayrton, we learn, has expressed great interest in the matter, an estimate has been called for, and as there is already steam power available, we may hope that, under Dr. Tyndall's direction, the new light will soon brightly shine.

THE Educational Lectures delivered at the London Institution during the past session by Prof. Huxley, Dr. Odling, and Mr. R. A. Proctor, were followed by examinations, and on Wednesday last the prizes and certificates obtained by the students were distributed by Mr. Thomas Baring, M.P., F.R.S., the president of the Institution. In Chemistry, the first prize was awarded to Frederick Garrett, and the second prize to A. J. Richardson. In Biology, Miss Dora Harris gained the first prize, while A. Percy Lloyd and Miss F. L. Tolmé obtained second prizes. In Astronomy, the first prize was gained by A. J. Wallis, and second prizes fell to Miss Annie Piper and Edward Garrett.

W. T. THISTLETON DYER, Esq., commenced last Monday a course of six lectures on the Natural History of a Flowering Plant, at the Royal College of Science for Ireland, St. Stephen's Green. The lectures will be continued on the following Monday and Thursday evenings.

THE Society of Arts has this year conferred its Albert Gold Medal upon Mr. Cole, C.B. It seems agreed on all hands that without Mr. Cole we should have had neither the South Kensington Museum nor any of those Science and Art classes which are now either in full work or are springing up throughout the country, and are doing an incalculable amount of good. If this be so, then certainly Mr. Cole has done more for the spreading of Science and Art in this country than any other man of his time; and our scientific and artistic bodies should join with the Society of Arts in acknowledging his services.

THE Royal Commission on Scientific Instruction and the Advancement of Science recommenced their sittings on Tuesday last, and will meet again to-morrow.

PROF. HUMPHRY, of Cambridge, will hold classes for instruction in Practical Anatomy on Tuesdays, Thursdays, and Saturdays at half-past twelve during the Long Vacation, commencing July 4. There will also be classes for instruction in Practical Histology on Wednesdays and Fridays at half-past twelve, commencing July 5. This, together with a course of instruction in the Physiological Laboratory, under the direction of Dr. Michael Foster, will constitute a course of Practical Physiology. Gentlemen who have entered to the Anatomical Lectures will be at liberty to attend these classes without additional fee.

THERE will be an election to a Science Fellowship at Corpus Christi College at the beginning of Michaelmas Term. Candidates must have passed all the examinations required by the University for the degree of B.A., and must not be in possession of any benefice or property which would disqualify for retaining a Fellowship. This examination will commence on Monday, October 9, and will be specially in chemistry. Candidates are requested to communicate with the president, either personally or by letter, at their convenience, before the end of Act Term.

THE Sheepshanks Astronomical Exhibition at Trinity College, Cambridge, has been awarded to Horace Lamb, Scholar of the College. The Exhibition is open to all members of the University, the only conditions being that the person elected shall become a member of Trinity College.

AT a recent Court of Governors of St. Bartholomew's Hospital, Mr. Paget, F.R.S., was appointed consulting surgeon to the hospital. At a Court held last Thursday, Mr. Callender was elected surgeon. There is now, therefore, a vacancy for the office of assistant-surgeon to the hospital.

WE greatly regret to hear of the alarming illness of Mr. Grote, the Vice-Chancellor of the University of London. His long and useful life has been devoted to the cause of higher education in this country, and since the foundation of the University he has been one of its staunchest and most unwearied friends.

We observe that Miss Esther Greatbatch, who has just passed the second (special) examination for women at the University of London in French and in Harmony and Counterpoint, also took the second prize at the examination which followed Prof. Guthrie's Lectures on Physics at the London Institute, in February, 1870, and out of seventy-four candidates, the first prize for the examination in Physical Geography, which followed Prof. Huxley's lectures on that subject in 1869. In 1868 Miss Greatbatch passed as a Junior with first class honours, and gained a prize for Mathematics at the Cambridge Local Examination. In December, 1870, she passed as a Senior, with first class honours, gaining the Mill-Taylor Scholarship and a prize for Political Economy. Miss Greatbatch did not take up the Mill-Taylor Scholarship, which can only be held at Cambridge. She is a pupil of the North London Collegiate School for Girls, where she has received the whole of her education. The lady to whom we alluded last week is not the first who has gained a special distinction of proficiency in Natural Philosophy and Chemistry at the examinations of the University of London, that honour having been conferred on Miss Orme in 1870.

Prof. Huxley's "Elementary Lessons in Physiology" are about to be translated into Hindostanee.

The Paris correspondent of the *Daily News* states that the chateau of the Marquis Laplace at Arcueil Cachan, which escaped the Prussians, has been plundered by a band of housebreakers from the Mouffetard district. The manuscripts of the celebrated astronomer were thrown into the Bièvre, from which the original of "The Mechanism of the Heavens," in the author's handwriting, has subsequently been fished out. The library, which was rich in rare books, souvenirs, and works of art, has been looted and devastated.

The medal given by the Royal Geographical Society to Dr. Keith Johnston is in acknowledgment of the services of the devotion of more than forty years of an unusually active life to purely geographical pursuits. He has done more for popularising geography than almost any other living author. The publication of his "Physical Atlas" in 1847 gave an unexampled impetus to the study of Physical Geography in Britain, and his publications since then, of great utility and importance, have been very numerous.

We learn from the *Journal of Botany* that the well-known cryptogamist Fries accompanies the Swedish Arctic expedition as botanist, and MM. Lindahl and Nauckhoff as zoologist and geologist. The expedition intends to visit Baffin's Bay, and to return in October.

A new botanical magazine has just appeared at Lund in Sweden, "Botaniska Notiser," edited by Otto Nordstedt. It will be specially devoted to Scandinavian botany, and to a review of all botanical papers published in Sweden, Norway, Denmark, and Finland.

The yearly part, just issued, of the Natural History Transactions of Northumberland and Durham contains, as usual, many valuable contributions to scientific literature. The first paper is a Revision of the Catalogue of Coleoptera of Northumberland and Durham, by Mr. T. J. Bold, including 1,520 species, about one half the number hitherto found in Great Britain. Dr. W. C. M'Intosh contributes a short Report on the Collection of Annelids dredged off Northumberland and Durham, and Mr. George Hodge a Catalogue of the Echinodermata of the same counties, forty-three in number, accompanied by interesting remarks on each species, and illustrated by four plates. From Messrs. W. Kirby and J. Duff we have some elaborate Notes on the Geology of part of South Durham, the lower coal measures in the neighbourhood of the village of Etherley, embellished by diagrams showing the position of the coal-seams. Then follow five papers on remarkable fossils of the carboniferous series by Mr. Albany Hancock, in conjunction with Messrs. T. Atthey and R. Howse; viz., On the Labyrinthodont Amphibian, *Loxomma Allmanni*, being its first occurrence in the neighbourhood; On a new generic form of the same order, to which the name *Batrachiderpeton lineatum* is given; On another new form from the magnesian limestone, *Lepidotosaurus Duffii*; A description of a specimen from the marl-slate of the oldest known reptile, *Proterosaurus Speneri*, and of a new species, *P. Huxleyi*; and A description of four specimens from the same formation of *Dorypterus Hoffmanni*; these papers are illustrated with six well-executed plates. The last paper is on *Saccamina Carteri*, a new foraminifer from the carboniferous limestone of Northumberland (with one plate), by Mr. H. B. Brady; and the volume closes with an admirable address from the president of the Tyneside Naturalists' Field Club, Mr. G. S. Brady, in which he gives a sketch of the work of the society during the past year, and of the general additions to natural history literature during the same period, especially in the departments of "Spontaneous Generation" and the "Origin of Species." The president for the present year is Mr. G. C. Atkinson.

The last number of the "Bücher-verzeichniss," published by Friedländer and Son, of Berlin, is a valuable and copious catalogue of astronomical literature in English, French, German, Latin, and the other European languages.

A third edition of Prof. Brünnow's "Lehrbuch der Sphärischen Astronomie" has just been published in Berlin.

We are favoured by Mr. Login with a photograph representing the produce of one grain of wheat grown on the Egyptian system in India, which gave off 160 shoots, and has produced 105 ears of corn; another, $4\frac{1}{2}$ ft. high, which produced 45 ears from $4\frac{1}{2}$ in. to 5 in. in length; and another $3\frac{1}{2}$ ft. high, with about 50 ears. These are represented as about average results from this system, and are contrasted with a single plant grown on the native broad-cast system, assisted by irrigation, producing seven ears from each grain, and another without irrigation, producing from five to fifteen ears, and also represented as average results. We congratulate the Indian authorities upon having a man of Mr. Login's wide grasp in their service. This is by no means the first time we have referred to his labours.

With reference to the alleged disappearance of Aurora Island, one of the New Hebrides group, to which we alluded some weeks since on the authority of a paper read before the Academy of Natural Sciences of Philadelphia, a correspondent of the *Shipping and Mercantile Gazette* affirms that the whole story is a fable. The original statement rested on a notice by Captain Plock, of the French ship *Adolphe*, bound from Iquique to London, that he passed over the position of the Iles de l'Aurore, as marked on his French chart of the South Atlantic, and saw nothing of them, from which he concluded that they had disappeared. It appears, however, that the Iles de l'Aurore (Aurora Islands) never existed. They were formerly placed between lat. 52° 38′ and 53° 15′ S., and between long. 47° 43′ and 47° 57′ W. of Greenwich. The first reporters of the islands probably saw icebergs in the given locality, and mistook their character. Aurora Island, in the New Hebrides group, has been confounded with the Aurora Island in the Paumotu, Tuamotu, or Low Archipelago. Aurora, Makatea, or Metia Island, lat. 15° 50′ S., long. 148° 13′ W., one of the Low Archipelago, has not been visited for some time, but its elevation would lead to the inference that it could not disappear suddenly; it is fertile and inhabited. This is the island visited by Wilkes, and on which the unique specimens of mollusca were found. It is upwards of 2,500 miles eastward of the New Hebrides.

## *AMERICAN NOTES* *

THE official report of the geological explorations prosecuted during the past summer by Prof. F. V. Hayden, under the authority of the Department of the Interior, has just been published by the Government in a well-printed volume of over five hundred pages, containing a full account of the geology and natural history of the region traversed. It embraces an article by Prof. Hayden upon the physical character and local geology of the different sections of his route, which extended from Cheyenne, by way of Fort Fetterman, South Pass, Fort Bridger, the Uinta Mountains, to Green River, and back again, *viâ* Bridger's Pass, to Cheyenne. This is followed by an account of the Geology of the Missouri Valley from Omaha to Salt Lake Valley, with observations on the mines, ores, coals, and salts. An appendix contains an article by Prof. Cyrus Thomas upon the agricultural possibilities of the country, with a list of the orthopterous insects, including a number of new species, followed by a number of special reports—as one by Prof. Meek, on the invertebrate fossils; on the Tertiary coals of the West, by Prof. Hodge; on the ancient lakes of Western America, by Prof. Newberry; on the vertebrate fossils of the Tertiary formation, by Prof. Leidy; on the fossil plants of the Cretaceous and Tertiary formations of Kansas and Nebraska, by Mr. Lesquereux; on the fossil reptiles and fishes of the Cretaceous rocks of Kansas, the fossil fishes of the Green River group, and the recent reptiles and fishes, by Prof. Cope; and on the industrial resources of Western Kansas and Eastern Colorado, by Mr. Elliott. Lists of the mammals, molluscs, and birds, of the coleoptera, hemiptera, and plants, are also included, together with an account of the general meteorology of the expedition. A large number of new species of different kinds are described, and the whole work forms a very important addition to our information relative to the geology, geography, and natural history of the West.

The second and third annual reports of the Peabody Academy of Science of Salem (for 1869 and 1870) have just been published, giving a gratifying account of the activity of that young and energetic society, which, although only in the third year of its existence, already ranks among the best establishments of the kind in the country, and which, in the number of excellent working naturalists associated with it, is rapidly making its mark. The donations to the museum of the academy during 1870 alone amounted to 195, received from 148 different persons. The identification of the specimens presented has been accomplished by the officers of the academy, aided by specialists in other parts of the country. The reports embrace references to several exploring expeditions instituted in the interests of the academy in different parts of the United States, as well as in Central America. The second number of the first volume of the Memoirs of the academy has also just appeared, and closely resembles typographically, as well as in size and other features, the well-known Memoirs of the Boston Society of Natural History, and of the Museum of Comparative Zoology. This part is occupied entirely by a paper upon the embryology of certain neuropterous and other insects, by Dr. A. S. Packard, jun., the Secretary of the Council.—Attention is called by the Panama papers to the extraordinary meteorological conditions that have lately prevailed throughout Central and South America, especially in the falling of large quantities of rain where previously such an occurrence was almost unknown. This unusual amount of precipitation is understood to have first occurred on the Isthmus of Panama, and to have resulted in disastrous floods at Aspinwall and elsewhere, of which an account has already been given. The climatic change seems to have travelled southwardly from that region, and to have involved successively a large portion of the chain of the Andes in its operations. The latest advices from Peru show that in localities hitherto perfectly rainless torrents have fallen to such an extent as to produce very great disasters. These have occurred at Payta, San José, Lambayeque, &c. The villages on the western slope of the Andes in Chili and Peru are not prepared for such an occurrence (of which many of the inhabitants had never had any practical experience), the sites and material of the buildings being alike unsuited to resist storms. The town of Lambayeque, containing seven or eight thousand inhabitants, is reported to have been entirely destroyed by the rain. The most southerly point reached by the rain at last dates seems to be the valley of Canete, which was inundated to the great damage of the sugar and other plantations. Much land has been totally ruined by the washing out of its soil, leaving behind a mere collection of gravel and stones. Vessels passing along the western coast at a distance of hundred miles and more experienced heavy rains where previously nothing but fog had been met with. The electric phenomenon visible around Mount Tacora, to which we recently referred, seems to have been a part of this same system of atmospheric disturbance, and connected with it was a widely extended arrangement of the telegraph lines in Chili, an event of extreme rarity.

* Communicated by the Editor of *Harper's Weekly*.

## *SCIENCE IN VICTORIA*

ONE or two interesting subjects were discussed at a recent meeting ot the Royal Society of Victoria, and we are favoured by a correspondent with the following particulars:—Notes on the working of the great Melbourne telescope, which some time ago was inconsiderately pronounced to be a failure, which were read by Mr. Faric MacGeorge, who has had charge of the instrument since Mr. Le Sueur left. It was stated that the speculum polished by Mr. Le Sueur had worked very satisfactorily, and some fresh discoveries with regard to Sirius and the star *d* were thus described by Mr. MacGeorge:—"On 9th Dec. 1870—indifferent evening—I noted all the faint stars near Sirius for future identification. On the 18th Jan. 1871, for the first time, I chanced upon Lassell's observations of Sirius in the 'Memoirs of the Royal Astronomical Society,' 1867. Mr. Martin there mentions having suddenly found a very faint star in the neighbourhood of Sirius which had, until then, escaped keen observers like Struve, Lassell, and himself, in the exquisite 4ft. equatorial at Malta. On comparing the position of this faint star—now called Lassell's Companion—with the faint stars noted by me on 9th December, it evidently corresponded with one noted on that date, so that with our great equatorial my eye, unbiassed by previous knowledge, detected at the first inspection on an indifferent evening an object which had long escaped these careful and experienced observers in the great Malta equatorial, an instrument of acknowledged excellence and equal aperture to our own. Several still fainter stars have since been seen near Sirius, two of them between Lassell's Companion, the star *d*, and Sirius. So far as I have yet seen, any want of definition is evidently due to atmospheric defects, not instrumental ones, the power of definition being at all times in direct ratio with the goodness of the evening." Prof. Wilson made a suggestion to the society respecting an expedition to Cape York, in a steamer, to witness the Total Eclipse of the Sun on the 12th December next, the eclipse being visible along a portion of the northern coast of Australia. The proposal was favourably entertained, and an understanding arrived at that it should receive fuller consideration at the next meeting. The annual meeting of the Acclimatisation Society of Victoria was held on the 10th March, Dr. Black, the President, occupying the chair. In their customary report to the subscribers, the council, while regretting the smallness of their numbers, stated that under the management of the new secretary Mr. A. C. Le Sœur, the society bade fair to again become extensively useful. It was mentioned that four ostriches which had been received from South Africa had been taken charge of by Mr. Samuel Wilson, of Longerenong, and had now increased to sixteen, and there was every reason to suppose that their numbers would be considerably augmented in the course of this season. So far the experiment had been a marked success. Ostrich farming was a profitable occupation at the Cape colony, and it was hoped it wou d ultimately become so here. The climate of the Wimmera district, it was remarked, appeared to be well adapted to their habits; as a proof of which, the young Australian birds were now taller than the parent stock. It was stated, amongst other subjects dealt with in the report, that the society had done and was doing all in its power to encourage sericulture in the colony, and to this end had, in conjunction with Dr. Von Mueller, sent white mulberry cuttings and plants to all parts of Victoria. Some months ago a box of silkworm eggs was sent by the Governor of India to his Excellency the Governor, who kindly handed them to the society for d's ribution, and lately a supply of very superior Japanese eggs, such as were seldom sold to foreigners, had been forwarded by Dr. Bennett, the hon. secretary of the Acclimatisation Society of New South Wales. The Silk Supply Association of London, it was mentioned, in one of their reports recently published, recognised no less than 36,000 square miles of country in Victoria as well suited to the growth of silk; and when the numerous young plantations came into bearing a great stimulus would be given to this industry, which in all probability would, before many years, add materially to the wealth of the colony.

## *MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY*

(*Continued from page* 94)

PRESERVED specimens have the great advantage over living ones, that they can be collected in infinitely greater numbers, maintained in juxtaposition, and compared, however distant the times and places at which they had been found. They are often the only materials from which we can obtain a knowledge of the races they represent; although still consisting of individuals only, they can, by their numbers, give better ideas of species and other abstract groups than the almost isolated living ones; and their careful preservation supplies the means of verifying or correcting descriptions or delineations which have excited suspicion. Their great drawback is their incompleteness, and the impossibility of deriving from them all the data required for the knowledge of a race or even of an individual. It is owing to the frequency with which characters supplied by preserved specimens, although of the most limited and unimportant a nature, have been treated as sufficient to establish affinities and other general conclusions which have proved fallacious, that the outcry I have alluded to has been raised against museums and herbaria by those very theorists whose speculations would fall to the ground if all the data supplied by preserved specimens were removed from their foundation.

In respect of these deficiencies, as well as in the means of supplying them, there is a great difference between zoological and botanical museums. Generally speaking, zoological specimens show external forms only; botanical specimens give the means of ascertaining internal structure;* and as a rule the characters most prominently or most frequently brought under the observer's notice acquire in his eyes an undue importance. Hence it is that external form was for so long almost exclusively relied upon for the classification of animals, whilst the minutiæ of internal structure were at a comparatively early period taken account of by botanists, while palæontologists are still led to give absolute weight to the most uncertain of all characters—outline and external markings of deciduous organs. External form is, however, really of far greater importance in animals than in plants; the number, form, size, and proportions of limbs, the shape and colour of excrescences, horns, beaks, feathers, hair, &c., in animals may be reckoned almost absolute in species when compared with the same characters in the roots, branches, and foliage, and, to a certain extent, even in the flowers of plants. In plants, local circumstances, food, meteorological conditions, act readily in modifying the individual, and producing more or less permanent races of the lowest degree (varieties); whilst animals in these respects are comparatively little affected, except through those slow or occult processes by which the higher races, species, or genera in all organisms are altered in successive ages or geological periods. Even relative position of external parts, so constant in animals, is less so in plants. Animals being thus definite in outline, and a very large proportion of them manageable as to size, their preserved specimens, carcases, or skins can be brought together under the observer's eye in considerable numbers, exhibiting at once characters sufficient for the fixation of species; whilst, with a few rare exceptions, a whole plant in its natural shape can never be preserved in a botanical museum. And, although good botanical specimens have a general facies, often sufficient to establish the species if the genus is known, yet the most experienced botanists have often erred in their determinations where they have been satisfied with external comparison without internal examination.

Identification of species is, however, but a small portion of the business of systematic biology, and for higher purposes the classification of species, and the study of their affinities, the pre-eminence of ordinary zoological over botanical specimens soon fails, those characters distinguished by Prof. Flower as adaptive are proportionately more prominent, and the essential ones derived from internal structure are absent; and not only do the former thus acquire undue importance in the student's eyes, but arguments in support of a favourite theory have not unfrequently been founded on distortions really the result of bad preparation, although supposed to be established on the authority of actual specimens, and therefore very difficult to refute. Mounted skins of vertebrata, showy insects in their perfect stage, shells of malacozoa, corals, and sponges, necessarily form the chief portion of a museum for public exhibition; but science and instruction require a great deal more; museum collections really useful to them should exhibit the animal as far as possible in all its parts and in all the phases of its life. This necessity has been felt in modern times, and has resulted in the establishment of Museums of Comparative Anatomy, amongst which that of our own College of Surgeons has certainly now taken the lead. But I have nowhere seen, except on a very small scale, the two museums satisfactorily combined. The idea, however, is not a new one; several zoologists have expressed their opinions on the desirableness of such an arrangement, which it is to be hoped will be duly considered in the formation of the new National Zoological Museums about to be erected at South Kensington, for the double purpose of exhibition and science. The requirements of the gazing public are sure to be well provided for, and there is every reason to believe that the exertions of scientific zoologists will not have proved useless, that we shall in the portion devoted to science and instruction see the skins of vertebrata preserved without the artist's distortions, accompanied, as far as practicable, by corresponding skeletons and anatomical preparations, as well as by the nests and eggs of the oviparous classes; insects with their eggs, larvæ, and pupæ; shells with the animals which produce them, &c., always with the addition, as far as possible, of the collector's memoranda as to station, habits, &c., in the same manner as herbarium specimens are now frequently most carefully completed by detached fruits, seeds, young plants in germination, gums, and other products.

Here, however, will arise another source of false data to be carefully guarded against—the mismatching of specimens, which in botany has probably produced more false genera and species than the misplacing of garden labels. The most careful collectors have in good faith transmitted flowers and fruits belonging to different plants as those of one species—the fruits perhaps picked up from under a tree from which they were believed to have fallen, or two trees in the same forest with similar leaves, the one in flower the other in fruit, supposed to be identical, but in fact not even congeners, and the mismatching at the various stages of drying, sorting, distributing, and finally laying in the specimens, have been lamentably frequent. Collectors' memoranda, if not immediately attached to the specimens or identified by attached numbers, have often led the naturalist astray, for collectors are but too apt, instead of noting down any particulars at the time of gathering, to trust to their memory when finally packing up their specimens. And so long as reasoning by analogy was never allowed to prevail over a hasty glance at a specimen and the memoranda attached to it, false genera and species arising from these errors were considered indisputable. *Magallana* of Cavanilles was, till recently, allowed materially to invalidate the character of Tropæoleæ, overlooking the strong internal evidence that it was founded upon the fruit of one natural order carefully attached to a poor flowering specimen of another.

Zoological museums and botanical herbaria differ very widely in the resources at their disposal for formation, maintenance, and extension of their collections. Zoological museums are by far the most expensive, but on the other hand as exhibitions they can draw largely on the general public, whilst herbaria must rely mainly upon science alone, which is always poor; both, however, may claim national assistance on the plea of instruction as well as of pure science, and for practical or economic purposes the herbarium is even more necessary than the museum. The planning the new museums so as best to answer these several purposes for which they are required, has, we understand, engaged the attention of the Royal Commission on Scientific Instruction and the Advancement of Science, and our most eminent zoologists have been consulted; any further observations on my part would therefore be superfluous. If our Government fail in their arrangements for the promotion of science, it will not be for want of having its requirements laid before them.

I am unable to say what progress has been made of late years in zoological museums, my notes on Continental ones were chiefly taken between the years 1830 and 1847, and would therefore be now out of date. It would, however, be most useful if some competent authority would undertake a tour of inspection of the more important ones, as in the great variety of their internal arrangements many a useful practical hint might be obtained, and we much want a general sketch of the principal zoological and botanical collections accessible to science, showing in what branch each one is specially rich, and where the more important

* By *internal structure* is here meant the morphology of internal organs or parts, usually included in the comparative anatomy of animals, not the microscopical structure of tissues, which is more especially designated as vegetable anatomy.

typical series are now respectively deposited. In herbaria a few changes have recently taken place which it may be useful to record. Paris, I mean of course the brilliant Paris of a twelvemonth back, had lost considerably. Of the many important private herbaria I had been familiar with in earlier days, two only, those of Jussieu and of A. de St. Hilaire, had been secured for the national collection, Webb's had gone to Florence, J. Gay's, which would have been of special value at the Jardin, was allowed to be purchased by Hooker and presented by him to Kew. The celebrated herbarium of Delessert is removed to Geneva, whilst his botanical library, one of the richest in existence, is locked up within the walls of the Institut. These are but partially replaced by M. Cosson's herbarium, which has much increased of late years, and to which he added last spring the late Schultz Bipontinus's collection rich in Compositæ. The national herbarium of the Jardin des Plantes is still one of the richest, but no longer *the* richest of all. The limited funds at the disposal of the Administration have allowed of their making but few acquisitions; their staff is so small and so limited in the hours of attendance, that the increase of the last twenty years remains for the most part unarranged, and their library is most scanty. Science has been out of favour with their governments of display. It would be out of place for me here to dwell upon the painful feelings excited in my mind by the dreadful ordeal through which a country I have been so intimately associated with for more than half a century is now passing, feelings rendered so acute by the remembrance of the uniform kindness I have received from private friends as well as from men of science, from Antoine Laurent de Jussieu and his colleagues to the eminent professors of the Jardin, who have now passed through the siege; but I may be allowed to express an anxious hope that when the crisis is passed, and the elasticity of French resources will have restored the wonted prosperity, the new Government may at length perceive that, even politically speaking, the demands of science require as much attention as popular clamour.

The Delesserian herbarium has been well received at Geneva, where it has been adequately deposited in a building in the Botanic Garden, very near to the Natural History Museum now erecting. At Paris it had been for some time comparatively useless, owing to the attempt to class it according to Sprengel's Linnæus, but now an active amateur committee, Messrs. J. Mueller, Reuter, Rapin, and others, under the presidency of Dr. Fauconnet, have already made great progress in distributing the specimens under their natural orders; and Geneva, already containing the important typical collection of De Candolle, and Boissier's stores rich especially in Mediteranean and Oriental plants, has become one of the great centres where real botanical work can be satisfactorily carried on; and as she has had the good sense to level her fortifications, she may accumulate national treasures with more confidence in the future. Munich has lost much of the prospect she had; the Bavarian Government failed to come to terms with the family of the late von Martius, his botanical library has been dispersed, and his herbarium removed to Brussels, where it is to form the nucleus of a national Belgian collection. At Vienna the Imperial herbarium is now admirably housed in the Botanical Garden, and is in good order, with the advantage of a rich botanical library in the same rooms. At Berlin, where the Royal Herbarium, like the Zoological Museum, has always been kept in excellent order, want of space is greatly complained of since it has been transported to the buildings of the University. At Florence, as we learn from the *Giornale Botanico Italiano*, the difficulties with regard to the funds left by Mr. Webb for the maintenance of his herbarium have been overcome, and it is to be hoped that the liberal intentions of the testator who made this splendid bequest for the benefit of science will no longer remain so shamefully unfulfilled. To the above six may be added Leyden, Petersburgh, Stockholm, Upsala, and Copenhagen, as towns possessing national herbaria sufficiently important for the pursuit of systematic botany; but when I visited them, now many years since, they were all, more or less, in arrear in arrangement. I know not how far they may have since improved. In the United States of America, the herbarium of Asa Gray, recently secured to the Harvard University, now occupies a first rank. That of Melbourne in Australia, founded by Ferdinand Mueller, has, through his indefatigable exertions, attained very large proportions; and that of the Botanical Garden of Calcutta, under the successive administration of Dr. Thomson and the late Dr. T. Anderson, had recovered in a great measure its proper position, which, I trust, it will henceforth maintain. Our own great national herbarium and library at Kew is now far ahead of all others in extent, value, and practical utility; originally created, maintained, and extended by the two Hookers, father and son, their unremitting and disinterested exertions have succeeded in obtaining for it that Government support without which no such establishment can be really efficient, whilst their liberal and judicious management has secured for it the countenance and approbation of the numerous scientific foreigners who have visited or corresponded with it. Of the valuable botanical materials accumulated in the British Museum during the last century I say nothing now, for the Natural History portion of that establishment is in a state of transition, and my own views as regards Botany have been elsewhere expressed. I have only to add that we have also herbaria of considerable extent at the Universities of Oxford, Cambridge, and Edinburgh, and at Trinity College, Dublin, and to express a hope that the necessity of maintaining and extending them will be duly felt by these great educational bodies, if they desire to secure for their professorial chairs botanists of eminence.

3. Pictorial representations or drawings have the advantage over museum specimens, that they can be in many respects more complete, they can represent objects and portions of objects which it has been impossible to preserve, they can give colour and other characters lost in the course of desiccation, they preserve anatomical and microscopical details in a form in which the observer can have recourse to them again and again without repeating his dissections, and although, like a museum specimen, each drawing represents usually an individual, not a species, yet that individual can by exact copies be multiplied to any extent for the simultaneous use of any number of naturalists, whilst specimens of the same species in different museums are corresponding only, not identical, and imperfect comparison and determination of specimens supposed to be authentic (*i.e.*, exactly corresponding to the one originally described) have led into numerous errors. Drawings, moreover, of diagrams and other devices can represent more or less perfectly the abstract ideas of genera and species, they can exhibit the generic or specific character more or less divested of specific or individual peculiarities.

Drawings on the other hand are, much more than specimens, liable to imperfections and falsifications arising from defective observation of the model and want of skill in the artist, and errors thus once established are much more difficult of correction than even those conveyed by writing. A pictorial representation conveys an idea much more rapidly, and impresses it much more strongly on the mind, than any detailed accompanying description by which it may be modified or corrected, and is but too frequently the only evidence looked into by the more theoretical naturalist. This is especially the case with microscopical and anatomical details of the smaller animals and plants, the representations of which, if very elaborate and difficult to verify, usually inspire absolute confidence. Drawings are also costly, often beyond the means of unaided science, who here again, as in the case of gardens and museums, is obliged to have recourse to the paying public; the public in return require to have their tastes gratified, artistic effect is necessarily considered, thus increasing the cost and removing the pictures still further from the reach of the working biologist. It appears to me, however, that collections of drawings systematically arranged have not generally met with that attention which they require from directors of museums, and that their multiplication in an effective and cheap form ought to be a great object on the part of Governments, Scientific Associations, and others who contribute pecuniarily to the advancement of science.

To be effective, the first requisites in a zoological or botanical drawing are accuracy and completeness; it is a faithful representation not a picture that is wanted. Many a splendid portrait of an animal or plant, especially if grouped with others in one picture, has been rendered almost useless to science by a graceful attitude or an elegant curve which the artist has sought to give to a limb or to a branch, and those analytical details which are of paramount importance to the biologist are neglected, because they spoil the general effect. We next require from an illustration, as from a description, that it should be representative, or to a certain degree abstract, and this requires that the artist, if not himself the naturalist, should work under the naturalist's eye, so as to understand what he delineates. Great care should be taken, in the selection for the model of an individual in a normal state, as to health, size, &c., and in the selection and arrangement of the anatomical details, so as to represent the race rather than the individual, all of which requires a thorough acquaintance with the questions to be attended to. It is true that the artist working independently and copying mechanically may serve as a check on the naturalist, who in minute microscopic examinations may be apt to see too much in conformity to preconceived theories; but that is not often the case, the most satisfactory analytical drawings I have always found to be those made by the naturalist's own hand, and I have long felt how much my own inability to draw has detracted from the value of botanical papers I have published. And thirdly, when we consider that the great advantage of an illustration over a description is, that the one gives us at a glance the information which we can only obtain from the other by study, we require that each drawing or plate should be as comprehensive as is consistent with clearness and precision. Outline drawings or portraits without structural details often omit the essential characters we are in search of; where details are unaccompanied by a general outline, we miss a great means of fixing their bearing on our minds. Structural details may also equally err in being too numerous or too few, or too large or on too small a scale. If the plate is crowded with details of little importance, or which may be readily taken from the general outline, they draw off the attention from those which it is essential should be at once fixed on the mind, and if enlarged beyond what is necessary for clearness, they require so much the more effort to comprehend them, unless indeed they be destined to be hung up on the walls of a lecture-room. I believe it to be the case with some drawings of the muscles of vertebrata, or of the internal structure of insects, as I know it to be with those of ovules and other minute parts of flowers of the late Dr. Griffith and others, that with their very high scientific value, their practical utility is much interfered with by the large scale on which they are drawn. A great deal depends also on the arrangement in the plate, always keeping in mind that the object is not to please the eye, but to convey at one view as much as possible of comparative information without producing confusion.

Biological illustrations in general have much improved in our time. It is true that some of the representations of animals and plants dating from the middle of last century will enter into competition with any modern ones as to the general outlines and facies, but analytical details were almost universally neglected, and colouring when attempted was gaudy and unfaithful. At present I believe we excel in this country in the general artistic effect, as unfortunately also for the naturalist in the costliness, of our best zoological and botanical plates; the French are remarkable for the selection, arrangement, and execution of the scientific details, and as a model I may refer to some of the publications of the Paris Museum, such as the Malpighiaceæ of Adrien de Jussieu, and also for the excellent woodcuts illustrating their general and popular works; the Germans and some Northern states for the admirable neatness of microscopic and other minutiæ executed at a comparatively small cost, owing partially at least to the use of engravings on lithographic stone.

4. Written Descriptions are what we must chiefly rely upon to convey to the general or to the practical naturalist the results of our studies of animals and plants; but descriptions are of two kinds—individual descriptions and descriptions of species, genera, or other races. The former are like preserved specimens or delineations, materials for study, like them they require in their preparation little more than artistical skill guided by a general knowledge of the subject; but abstract descriptions, whether specific or relating to races of a higher degree, require that study of the mutual relations of individuals and races and their consequent classification which constitute the science of systematic biology, and this distinction should be constantly kept in view for the just appreciation of all descriptive works. Any tyro can with care write a long description of a specimen unimpeachable as to accuracy, but it requires a thorough knowledge of the subject and a keen appreciation of the bearings of the points noticed to prepare a good description of a species. For the latter to be serviceable it must be accurate, it must be full without redundancy, it must be concise without sacrificing clearness, it must be abstractive not individual, and lastly, the most difficult qualification of all and that which constitutes the main point of the science, the abstraction must be judicious and true to Nature.

The paramount importance of accuracy is too evident to need dwelling upon. We are all liable to errors of observation. Imperfect vision or instruments, optical deceptions, accidentally abnormal conditions of the specimen examined, hasty appreciation of what we see from preconceived theories, are so many of the causes which have occasionally led into error the most eminent of naturalists, and require to be specially guarded against by repeated observation of different specimens and constant testing at every step by reasonings from analogy. Errors once established on apparently good authority are exceedingly difficult to correct, and have been the source of many a false theory. Where loose examination and hasty conclusion have been frequently detected, we can at once renounce all confidence in an author's descriptions—in his genera and species—unless confirmed from other sources, but an accidental oversight on the part of a naturalist of established reputation is the most difficult to remedy, notwithstanding the eagerness with which some beginners devote themselves to hunting them out. No botanist was, I believe, ever more careful in verifying his observations over and over again, and in submitting them to the tests supplied by the extraordinary methodising powers of his mind, than Robert Brown, no one has ever committed fewer of what we call blunders, or established his systematic theories on safer ground, yet even he has been detected in a few minor oversights, eagerly seized upon by a set of modern speculative botanists, lovers of paradoxes, as justifying them in devoting their time and energies to the disputal of several of his most important discoveries and conclusions.

The value of a description as to fulness and conciseness is practical only, but in that point of view important. A description, however accurate, is absolutely useless if the essential points are omitted, and very nearly so if those essential points are drowned in a sea of useless details; the difficulty is to ascertain what are the essential points; and hence one of the causes of the superiority of monographs and floras over isolated descriptions, such as those of Zoologies and Botanies of exploring expeditions, which I insisted on in my address of 1862; in the former the author must equally examine and classify all the allied races, and thus ascertain the essential points; in the latter case he is too easily led to trust to what he believes to be essential. My own long experience in the using, as well as in the making, of botanical descriptions, has proved to me how difficult it is to prepare a really good one, how impossible it is to do it satisfactorily from a first observation of a single specimen. However carefully you may have noted every point that occurs to you, you will find that, after having comparatively examined other specimens and allied forms, you will have many an error to correct, many a blank to fill up, and much to eliminate. I have more than once had to verify the same species in two authors, the one giving you a character in a few lines which satisfies you at once, the other obliging you to labour through two or three quarto pages of minute details, from which some of the essential points are omitted.

But the great problem to be solved at every stage in systematic or descriptive biology, and that which gives it so high a scientific importance, is the due detection and appreciation of affinities and mutual relations, and in this respect the science has made immense progress within my own recollection, and especially during the last few years the gradual supplanting of artificial by natural classifications has been too often commented upon to need repetition. It is now, I believe, universally admitted that a species consists of individuals connected together by certain resemblances or affinities the result of a common descent. It is also acknowledged that for scientific purposes these species should be arranged in groups according to resemblances or affinities more remote than in the case of species, although here commences the great difference of opinion as to the meaning of these remote affinities, whether they also are the result of a common descent, or of that supposed imitation of a type which I have above alluded to. For those, however, who have once connected affinity with consanguinity, it is difficult to recede from so ready an explanation of those mysterious resemblances and differences, the study of which must be the ruling principle to guide us in our classifications. All this has now been fully explained by more able pens than mine; my only object in repeating it is to point out clearly the need of treating all systematic groups from the order down to the genus, species, or variety, as races of a similar nature, collections of individuals more nearly related to each other than to the individuals comprising any other race of the same grade, and of abolishing the use of the expression *type* of a genus, or other group, in any other than a purely historical sense, as a question of nomenclature.* If a genus has to be

* For the purposes of instruction some one species is often named as *a* type of a genus, that is to say, as fairly representing the most prevalent

divided, our laws of nomenclature require the original name to be retained for that section which includes the species which the founder of the genus had more specially observed in framing his character, and therefore, and for that reason only, it becomes necessary to inquire which was or which were the so-called typical species—the biologist's or as it were the artist's, not Nature's type.

I need not repeat what I said in 1862 of the comparative value of monographs and faunas or floras over miscellaneous descriptions, observing only that the immense progress made in the accumulation of known species henceforth diminishes still more the relative importance to science of the addition of new forms when compared to the due collocation and correct appreciation of those already known. Much has been done of late years in the latter respect, but yet some branches of biology, and perhaps entomology more than any other, are very much in arrear as to supplying us with available data for investigating the history of species and their genealogy ; their origin, progress, migration, mutual relations, their struggle, decay, and final extinction. It is to be feared that in insects as in plants, but too large a proportion of the innumerable genera and sub-genera have been founded rather on the sortings of a collector than on the investigation of affinities ; and, indeed, that must in a great measure be the case so long as a large proportion are only known from their outward form at one period only of their varied phases of existence.

The days of a Systema Naturæ, or single work containing a synopsis of the genera and species of organised beings, are long since passed away. Even a Species Plantarum, now that their number at the lowest estimate exceeds 100,000, has become almost hopeless. The last attempt, De Candolle's Prodromus, has been nearly forty years in progress, the first portion has become quite out of date, and all we can hope for is that it may be shortly completed for one of the three great classes of plants. Animals might have been more manageable were it not for the insects. Mammalia estimated at between 2,000 and 3,000 living species, birds at about 10,000, reptiles and amphibia under 2,000, fishes at about 10,000, crustacea and arachnida rather above 10,000, malacozoa about 20,000, vermes, actinozoa, and amorphozoa under 6,000, would each by themselves not impose too heavy a tax on the naturalist experienced in that special branch who should undertake a scientific classification and diagnosis of all known species. In one important branch, indeed, the fishes, this work has been most satisfactorily carried out in Dr. Günther's admirable Genera and Species of all known fishes published under the misleading title of "Catalogue of the Fishes in the British Museum," and recently completed by the issue of the seventh volume. The sound philosophical views expressed in his preface to that volume (which, by some strange inversion, bears a signature not his own) can be appreciated by us all, and zoologists are all agreed as to the care with which they have been worked out in the text. Insects are, however, the great stumbling-block of zoologists. The number of described species is estimated by Gerstaecker at above 160,000, viz. : Coleoptera, 90,000 ; Hymenoptera, 25,000 ; Diptera, 24,000 ; Lepidoptera, 22,000 to 24,000. Mr. Bates thinks that, for the Coleoptera at least, this estimate is too high by one-third, but even with that deduction the number would exceed that of plants, and it is probable that the number of as yet undiscovered species in proportion to that of the described ones is far greater in the case of insects than of plants. We can therefore no longer hope for a Genera and Species of insects, the work of a single hand, or indeed guided by a single mind. The great division of labour, however, now prevalent among entomologists may procure it for us in detail, with one drawback only, that the smaller the portion of the great natural class of Arthropoda to which the entomologist confines his attention, the less he will be able to appreciate the significance of distinctive characters, and the more prone he will be to multiply small genera—that is to enhance beyond their due value the races of the lowest grades—to the great inconvenience of the general naturalist who has to make use of the results of his labour.

A Genera Plantarum is still within the capabilities of a single botanist, although he must, of course, trust much to the observations of others, and it therefore cannot be so satisfactory as if he had examined every species himself. The last complete one was Endlicher's, the result of several years' assiduous labour, but now thirty years old. Dr. Hooker and myself commenced a new one, of which the first part was published in 1862, and which might have been brought nearly to a close by this time had we not both of us had so many other works on hand to deter us, although the researches necessary for these other works have proved of great assistance to the Genera. As it is, the part now nearly ready for press carries the work down to the end of Compositæ, or about half through the Phænogamous plants. In regard to works of a still more general description, the exposition of the families or orders of plants, we have nothing of importance since Lindley's "Vegetable Kingdom," dated 1845, but republished with some additions and corrections in 1853, and Le Maout and Decaisne's "Traité Générale," mentioned in my address of 1868, and of which Mrs. Hooker is now preparing an English translation, under the supervision of Dr. Hooker. Dr. Baillon has also commenced a "Histoire des Plantes," containing a considerable number of useful original observations, and illustrated by excellent woodcuts, but as a general work, one portion is of too popular a character, and in some cases too diffuse to be of much use to science, and the generic character too technical for a popular work without any contrasted synopsis, and its great bulk in proportion to the information conveyed will always be a drawback. I cannot believe that the author can have been a party to the unblushing announcement of the French publisher, that it is to be completed in about eight volumes. If carried out on the plan of the first one, it must extend to four or five times that number. In Zoology, Bronn's most valuable "Klassen und Ordnungen der Thierreichs," continued after his death by Keferstein and others, which I mentioned in my address of 1866, has advanced but slowly. The Amorphozoa, Actinozoa, and Malacozoa, forming the first two volumes, were then completed, and Gerstaecker has since been proceeding with the Arthropoda, commencing with the Crustacea for the third volume, of which only the general matter and the Cirripedia and Copepoda are as yet published, and three or four parts of a sixth volume for birds have been issued by Selenka, treating the anatomical and other matters in great detail. Another general work of merit, although on a smaller scale, has been proceeding as slowly. Of Carus and Gerstaecker's "Handbuch der Zoologie," the second volume, containing the Arthropoda, Malacozoa, and lower animals, had been already published in 1861, and to this was added in 1868 the first half of the Vertebrata for the first volume, with a promise that the remainder should appear in the autumn, but which promise has not yet been fulfilled. Among the other recently published systematic zoological handbooks of which I have memoranda as published in various Continental states, the most important are said to be Harting's, published at Kiel, in the Netherlands, of which up to 1870 only three volumes had appeared, containing the Crustacea, Vermes, Malacozoa, and lower animals ; A. E. Holmgren's "Swedish Handbook ;" Zoology, of which Mammalia were published in 1865, and Birds in 1868 to 1871 ; and Claus's "Grundzüge," and Troschel's "Handbook" (7th edition) for University Teaching in Germany.

In a comparative sketch of the more partial monographs, faunas, and floras, I had wished to direct my attention more especially to the means afforded us of comparing the plants and animals of different countries ; and with this view one of the questions I addressed to foreign zoologists was—"What works or papers are there in which the animals (of any of the principal classes) of your country are compared with those of other countries?" The answers to this query have not been generally satisfactory. Where the zoology has been well investigated, we have popular handbooks, elaborate memoirs, and works of high scientific value, or splendidly illustrated. But short synoptical faunas, so useful to the general naturalist and corresponding to the floras we now possess of so many different countries, are very few ; the statement of the general geographical range of each species, so prominent a feature in many modern floras, is still less thought of, and indications of allied or representative races in distant countries are equally rare. We have indeed several excellent essays on the geographical distribution of animals ; I had occasion to allude to several of them in my address of 1869, but they are in general chiefly devoted to discussion, with statements of such facts only as bear upon the author's conclusions, not records of facts which may be useful to the geographical or general biologist. These must be collected from a great variety of separate works and papers, of which I have received long lists from Denmark, Sweden, Germany, Switzerland, Italy, France, and the United

character ; but to prevent any confusion with *the* imaginary type, it would surely be better to call it an "example," as, indeed, is often done. In geographical biology the word "type" is used again in another sense, which, however, does not lead to any misunderstanding.

States. As yet I only have had time to refer to a few which appeared to bear more immediately on the objects I had in view, but I hope on some future occasion to return to the subject. In the meantime I must content myself with glancing rapidly over the different countries, taking them in the order adopted in my former addresses, and endeavouring to show the progress making in supplying our deficiencies. Towards these deficiencies I would particularly call the attention of entomologists and terrestrial malacologists, for insects and land shells are of all others the animals whose life and local stations are the most closely dependent on vegetation. In the following notes I am further precluded from entering into details as to the zoological works or memoirs mentioned, by the consideration that they would be superseded by the analysis given in the annual reviews inserted in *Wiegmann's Archiv*, and more especially in our own admirably conducted *Zoological Record*, which so strongly claims the support of everyone interested in the promotion of Zoological Science.

(*To be continued.*)

## *ZOOLOGY*

### Note on Transversely Striated Muscular Fibre among the Gasteropoda.*

IN studying the radula of a species of *Acmæa* (probably *A. Borneensis* Rye), obtained by Prof. A. S. Bickmore at Amboyna, I noticed, on placing the structure under a power of 100 diameters, that certain of the muscular fibres which adhered to it, when torn from the buccal mass, had a different appearance from the others. On increasing the power to some 800 diameters, it was at once evident that the different aspect of these fasciculi was caused by fine, but clearly defined, transverse striation. Suspecting that it was an optical delusion, caused by a very regular arrangement of the nuclei of the fibres, I subjected the muscle to various tests and to still higher magnifying powers. I also introduced under the same glass some of the voluntary dorsal muscles of a small crustacean for comparison. The structure of the ultimate fibres in both appeared to be similar. These seemed to be composed of a homogeneous tube or cylindrical band of translucent matter, with nuclei interspersed at irregular intervals. In neither was there any appearance of separation into transverse discs, as is seen in the striated muscles of vertebrates. That the striated appearance was not due to contraction and folding of the muscle, was evident upon taking a side view of one of the fibres, when the striæ on each side, as well as the intervening elevations, were seen to correspond exactly to each other. The only perceptible differences between the muscles of the crustacean and the striated muscles of the mollusk, appeared to be that the latter were much more finely striate; the striæ being six to eight times as numerous as in the former in the same space. No difference between the striated and nonstriated muscles of the *Acmæa* could be observed, except in the fact of the striation. In both the nuclei were irregularly distributed. The appearance of the striated fibre reminded one of a string of rhombic beads, which bore no relation to the position of the true nuclei. The striated fibres appeared, after a careful dissection of the parts in a number of specimens, to be the retractors of the radula; they were longer and in narrower bands than the nonstriated fibres, and comparatively much fewer in number. The striation was most evident toward the middle of the fibres, and became evanescent toward their extremities.

Lebert and Robin (Müller's Arch. f. Anat. and Phys., 1846, p. 126) state that the primitive muscular fasciculi of invertebrates often have the nuclei and intervening clear spaces "arranged in such regular order that they might, at the first glance, be mistaken for transversely striated muscular fibres. The latter, however, are actually found in one acephalous mollusk, *Pecten* (and probably in *Lima* also), and some annelids," and are constantly present in the voluntary muscles of *Crustacea* and *Insecta*. In the further researches of M. Lebert (Annales Sci. Nat., t. xiii. 1850, p. 161), he observes that there is nothing extraordinary in the discovery of transversely striated muscular fibre in *Polyzoa* (*Eschara*) by Milne-Edwards, and in *Actinia* by Erdl, since "the further we have pursued the study of the comparative histology of muscular fibre, the more convinced we have become that transversely striated muscular fibre is to be found in a large number of animals of very inferior organisation, without regard to their more or less advanced position in the animal kingdom."

Striated muscular fibre has lately been shown to exist in the "tail" or appendix of *Appendicularia* by Moss (Trans. Lin. Soc., vol. xxvii. p. 300). It was already known to exist in *Salpa*, (Eschricht, ov. Salperne), in the articulated brachiopoda (Hancock, Tr. Roy. Soc., 1857, p. 805), and in *Pecten* (Lebert, Annales Sci. Nat. 1850, 3rd ser. t. xiii. p. 166; and Wagner, Lehrb. d. vergleich. Anat., t. ii. p. 470, 1847), as well as in *Eschara* (Milne-Edwards, Annales Sci. Nat., series ii. t. iv. p. 3). I believe, however, that this is the first instance in which it has been shown to exist in the class *Gasteropoda*; and this, as well as the rarity of such cases among the lower invertebrates, is a sufficient apology for bringing forward such an isolated fact. Other duties have not yet permitted me to determine whether this phenomenon is constant throughout the genus, or whether it does or does not occur among allied genera. W. H. DALL

* Communicated by the author, from the "American Journal of Science and Arts," vol. i., Feb. 1871.

## *SCIENTIFIC SERIALS*

IN the first paper in the *American Naturalist* for May, Prof. C. F. Hartt opens out quite a new field for investigation in the rock-inscriptions of Brazil, and illustrates it with nine plates of very great interest. The inscriptions occur on the rocks in various districts, and are many of them very rude, representing human and other figures, the sun, moon, and stars, and others very difficult to decipher. Prof. Hartt mentions as a curious circumstance that the hands and feet are always represented by radiating lines, usually only three digits being drawn for each hand and foot; the number rarely reaches four, and never five. This, he thinks may be explained by the fact that many tribes of Brazil are unable to count beyond three or four. The antiquity of these rock paintings and sculptures is undoubted, being mentioned by many ancient writers, as well as by Humboldt and others in more recent times. There can be no doubt that they ante-date the civilisation of the Amazons, and there is a strong probability that some of them, at least, were drawn before the European discovery of America. A short paper, by Dr. F. R. Hoy, on Dr. Koch's *Missourium tetracaulodon*, made by Prof. Owen into a Mastodon, points out several particulars in which Dr. Koch's account of the discovery of the fossil is not to be relied on, especially the inference of the great antiquity of man deduced from it. Mr. J. H. Emerton gives an account of the so-called "Flying Spiders," which are merely blown about by the wind. Among the "Miscellany" is an interesting note by Mr. A. Garrett, on the Distribution of Animals in the South Seas, especially in the Viti Islands. The number is altogether one of unusual interest.

*Archiv für Anthropologie*, 1870, Heft 3. An essay on "Theories of Sexual Generation," by Prof. His, of Basel, is rather historical than speculative, tracing the two principal lines of opinion represented in early science by Hippocrates and Aristotle, as to the respective functions of the two parents, and the mode of transmission of their bodily characteristics to the offspring. Among modern writers Prof. His dwells especially on Harvey's views. A paper by Dr. Welcker, "On the compressed feet of Chinese ladies," contains careful drawings, showing the shoe, the foot, and the abnormal position of the bones. As complete an account is given as the subject needs from an anatomical point of view. Dr. Jensen, occupied in studying the proportions of the brain in the insane, arranges for this purpose, a "stereoscopic-geometrical drawing apparatus," by the aid of which to produce geometrical drawings on which measurements can be made. Dr. Schaaf hausen's dissertation on "Cannibalism and Human Sacrifice," is a valuable, though somewhat undigested contribution to the subject. Among the motives assigned for cannibalism, the principal are hunger, revenge, superstition, such as induces savages to devour a brave warrior to obtain his courage, and lastly, the gluttonous longing for a kind of flesh which is described as appetising. Human sacrifice may sometimes be a relic of early cannibalism, an offering to deities who devour human flesh, or it may be an act of propitiation. There is evidence of the ancient or modern existence of cannibalism in most countries of the world, Great Britain being distinctly included. Even in modern times it occasionally breaks out in the civilised world, but on the whole its frequency among savages, and its general disappearance under improved social conditions, enable the writer, who argues in favour of a steady progression in the civilisation, to put it fairly into his argument.

## SOCIETIES AND ACADEMIES

### London

Geological Society, May 24.—Prof. John Morris, Vice-President, in the chair. Messrs. Mosley, Colvin, Noble, F.R.A.S., and Davey, were elected Fellows of the Society. The following communications were read:—(1) "On the principal Features of the Stratigraphical Distribution of the British Fossil Lamellibranchiata." By Mr. J. Logan Lobley, F.G.S. In this paper the author showed, by means of diagrammatic tables, what appears to be the present state of our knowledge of the general stratigraphical distribution of the fossil Lamellibranchiata in Britain. As a class, the Lamellibranchs are sparingly represented in the Lower, and more numerously in the Upper Silurian group, and fall off again in the Devonian; they greatly increase in number in the Carboniferous, become scanty in the Permian and Trias, and attain their maximum development in the Jurassic rocks. They are also largely represented in the Cretaceous and Tertiary series. The stratigraphical distribution of the two great subordinate groups, the Siphonida and the Asiphonida, corresponds generally with that of the class; the Siphonida predominate over the Asiphonida in Tertiary formations, whilst the reverse is the case from the Cretaceous series downwards. Nearly all the families of Lamellibranchs are represented in the Jurassic and Carboniferous rocks, and in the former very largely. The author remarked especially on the great development of the Aviculidæ in Carboniferous times. Mr. Etheridge, after noticing the importance of the paper, remarked that possibly the great difference observed in the proportions of Lamellibranchiata in different formations might to some extent be due to our want of knowledge. Of late years, in the Caradoc and Lower Silurian series, the number of species had been nearly doubled, principally through the persevering industry of one single observer, Lieut. Edgell. The same was to some extent the case in the Carboniferous rocks, owing to the collections of Mr. Carrington. Much was also being done for the Oolitic series, in connection with which the names of Mr. C. Moore, Mr. Sharp, and Dr. Bowerbank ought to be mentioned. Mr. Griffiths and the Rev. Mr. Wiltshire were doing the same work for the Gault. What the late Mr. S. P. Woodward had done as to the distribution of the different species of molluscs through time, Mr. Lobley was doing on a larger and more extended scale. Prof. Ramsay was glad to find that Mr. Lobley was, to some extent, doing the same for the Lamellibranchiata as Mr. Davidson had done for the Brachiopoda. He did not know how the case might be with the Silurian and Devonian formations, but in the Carboniferous strata the Lamellibranchiata were obtaining a preponderance over the Brachiopoda. He accounted for their comparative absence in formations of other ages, especially between the Upper Silurian and Rhætic beds, by the best known areas of those periods having been mainly continental, or containing principally freshwater or inland sea remains, so that the true marine fauna was absent. In Carboniferous times possibly the true relative proportions of the two forms had been preserved in the deposits. Mr. Judd was doubtful as to the safety of placing too great reliance upon figures. He questioned whether some of the conclusions as to the great increase of Lamellibranchiates between the Carboniferous and Jurassic periods could be substantiated. Much depended on the amount of the rocks present in different countries, and the study bestowed on each. The conditions also for the preservation of the fossils might be more favourable at one time than another. Mr. Carruthers considered the tables as of the greatest value, as indicating the present state of our knowledge. He called attention to the difference of conditions under which deposits had accumulated, which must have to some extent affected the proportion of Lamellibranchiates preserved in the different formations. Mr. Charlesworth remarked on the occurrence of *Trigonia* in the Australian seas, and on there being varieties of form among specimens of existing species so great that if they were found fossil they might be regarded as of several species. Mr. Hughes considered that the data were too incomplete to justify the generalisations of some of the previous speakers. It had been pointed out that whenever the tables showed a very large number of Lamellibranchs from any formation, that formation had been carefully worked out by local observers; and therefore he would like to know in each case the proportion the Lamellibranchiata bore to the total number of fossils found. It had been shown also that a larger proportion of Brachiopoda had been found in the older rocks, and of Lamellibranchiata in the newer. But in the older rocks whole genera of Lamellibranchs are confined to horizons and localities which are not cut off by stratigraphical breaks, such as would allow us to think it at all probable that they can be characterised by peculiar genera. He thought the scarceness and irregular occurrence of Lamellibranchs in the older rocks could be best explained on the supposition that those portions of the older deposits which were least favourable to Lamellibranchs happened to be those now chiefly exposed to our search, and that those few portions are only in part worked out. Mr. Jenkins observed that in thick deposits there was a far greater likelihood of numerous forms being present than in thin, for thickness meant time, and time meant variation. Prof. Morris dissented from this view, as in thin littoral deposits an enormous number of shells might be present, while in beds formed of deep sea they might be almost entirely absent.—2. "Geological Observations on British Guiana," by Mr. James G. Sawkins, F.G.S. In this paper the author gave a general account of his explorations of the Geology of British Guiana when engaged in making the geological survey of that colony. He described the rocks met with during excursions in the Pomeroon district, along the course of the Cuyuni and Mazuruni rivers, on the Demerara river, on the Essequibo and its tributaries, on the Rupununi river, and among the southern mountains. The rocks exposed consist of granites and metamorphic rocks, overlain by a sandstone, which forms high mountains in the middle part of the colony, and is regarded by the author as probably identical, or nearly identical, with the sandstone stretching through Venezuela and Brazil, and observed by Mr. Darwin in Patagonia. Prof. Ramsay remarked upon the barrenness, from a geological point of view, of the district investigated by Mr. Sawkins, and especially called attention to the absence of fossils in the stratified rocks. He referred briefly to Mr. Sawkins's labours in Trinidad and Jamaica, and to his discovery of metamorphosed Miocene rocks in the latter colony exactly analogous to the metamorphic Eocene rocks of the Alps. He was glad to see that the author had brought forward examples of cross-bedding in metamorphic rocks, and considered that the results adduced were favourable to those views of the metamorphic origin of granite which he had himself so long upheld. Mr. D. Forbes, on the contrary, considered that the facts brought forward by Mr. Sawkins were confirmatory of the eruptive nature of the granites observed. He added that cross-bedding was common in igneous rocks and even in lavas. Mr. Tate remarked that in the country to the north of the district described in the paper metamorphic rocks abound. He considered that the series of metamorphosed Jurassic rocks extends across the whole north of South America, and perhaps into California. Similar sandstones to those described occur in the basin of the Orinoco, and contain fossils which show them to be of Miocene age. Mr. Tate did not consider these sandstones as the equivalent of the Patagonian sandstones, as from the shells contained in the latter they would appear to be Pliocene or Pleistocene. Mr. Sawkins, in reply to a question from Mr. Tate, stated that the only gold found in the country had probably been carried down from the well-known gold district of Upata. He also entered into a few additional details connected with the chief points in his paper, dwelling especially upon the physical features of the country, in illustration of which several landscape drawings were exhibited.

Royal Institution of Great Britain, June 5.—Sir Frederick Pollock, Bart., M.A., vice-president, in the chair. Silas Kemball Cook, Miss Elinor Martin, Dr. Charles Bland Radcliffe, and Mrs. Radcliffe were elected members of the Royal Institution. The special thanks of the members were returned for the following donation to "The Fund for the Promotion of Experimental Researches":—Sir Henry Holland, Bart. (thirteenth annual donation), 40*l*.

Anthropological Institute, May 29.—Prof. Busk, F.R.S., vice-president, in the chair. George Latimer of Puerto Rico was elected a member. Mr. F. G. H. Price read a paper "On the Quissama Tribe of Angola," inhabiting that portion of Angola situated on the south bank of the Quanza river. The country had lately been visited by Mr. Charles Hamilton, well known for his travels among the Kaffirs. The Quissama bear the reputation of being cannibals, but cannibalism, although undoubtedly practised by them to some extent, does not largely prevail. The men are well formed, and average about five feet eight inches in height, they are copper-coloured, have long, coarse, and in some instances, frizzled, hair; their heads are mostly well developed, and the Roman nose is not unfrequently met with.

Their weapons are spears, bows and arrows, and occasionally guns, the latter being rude copies from the Portuguese article. Mr. Hamilton was well received by the chief, who told him that he was the first white man that had seen the tribe at home. The men and women of the Quissama are addicted to hunting; they are virtuous, practice monogamy, marry young, and are very prolific. The men largely preponderate in numbers over the women, the result, it is supposed, of infanticide, but of that practice Mr. Hamilton had seen no evidence. The Quissama believe in the existence of a Supreme Being.—A paper was read by Lieut. George C. Musters, R.N., on the races of Patagonia inhabiting the country between the Cordillera and the Atlantic, which the author had traversed during the years 1869 and 1870. The Patagonians consist of three races distinctly differing in language and physique, and partially differing in religion and manners, Tehuelches or Patagonians, Pampas, and Manzaneros, the latter being an offshoot of the Araucanians of Chile. The Tehuelches and Pampas are nomadic tribes subsisting almost entirely by the chase. The proverbial stature of the Patagonians was so far confirmed by the observation that the Tehuelches give an average height of five feet ten inches, with a corresponding breadth of shoulders and muscular development; the Manzaneros come next in order of height and strength, the Pampas being the smallest of the three races. The Manzaneros are remarkable for their fair complexions, whilst the Tehuelches are, literally speaking, Red Indians. Lieut. Musters had visited all the various tribes of those races, from the Rio Negro to the Straits of Magellan, for political purposes, and he estimated the population, which he described as diminishing, as follows:—Tehuelches 1,400 to 1,500, Pampas 600, and the remainder Manzaneros, amounting in all to about 3,000.—Dr. Eatwell contributed a communication on Chinese burials.—Mr. Josiah Harris announced the arrival from the coast of Peru of various pieces of rag, of wooden images, pottery, and other articles of great interest; and the chairman stated that the specimens would be exhibited and described at the next meeting of the Institute.—Mr. George Harcourt exhibited a flint implement found near a stream flowing from Virginia Water, and a bronze Celt discovered in the root of a tree in the parish of Thorpe, Surrey.

### Paris

**Academy of Sciences**, May 1. M. Chasles contributed a rather long but very important paper on Conic Sections. The illustrious mathematician gives the theorems rather than the mode of demonstrating them. It is a reminiscence of the old academy in the golden age of the seventeenth century. The theorems are very numerous.—M. Trécul read a rather long account of the analysis of the juices which can be extracted from aloes.—M. Decaisne read a memoir, which is printed at full length, on the Temperature of Children when they are taken ill.—M. Delaunay presented the second number of his monthly meteorological report for the month of April. It is to be noticed that April expired on a Sunday, and that M. Delaunay spared not a single hour, as his *résumé* was ready on the following day. The observatory had suffered scarcely any injury up to the end of the second siege. No delegate of the Commune had presented himself either to take possession of it or to blow it up.

May 8.—It was only at this late date that M. Longuet's death was officially made known to the Academy. M. Delaunay, who presided over the proceedings, gave expression to a few becoming sentences of regret at the loss the Academy had experienced. M. Longuet was a physiologist of much ingenuity and ability.—M. Duchartre, member of the Botanical Section, read a rather long paper on our knowledge of Liliaceæ.—M. Sedillat, the learned Arabic scholar, read a paper on the etymology of French words having an Arabic origin. Their number is immense, and M. Littré, in his great "Etymological Dictionary," supposes it to be even much larger. The intercourse with Arabs was very active even in mediæval times, as is proved by the history of the University of Paris, which so long defended Averrhoes. M. Sedillat gives many instances chosen from an immense number of others.—M. Stanislas Meunier sent a very interesting paper on meteorites. The experiments were made by him according to the precepts given by M. Daubrée, to whom M. Stanislas Meunier is assistant. M. Daubrée is now a refugee at Versailles. The museum where these experiments were executed is said to be safe, contrary to previous assertions. M. Stanislas Meunier explained by what process serpentine mountains can be changed into tadjerite. Tadjerite is found in some meteorites which belong to the museum collection. Specimens are also to be found in the British Museum, Yale College, U.S., &c. M. Boilot, the scientific editor of the *Moniteur*, read a paper which was written to show astronomers that they must study carefully the different kinds of combustion on the surface of the earth, natural or artificial, to gain some quasi-experimental knowledge of the celestial phenomena of the origin and variations of star light. The doctrine was illustrated by some interesting observations.—M. Quesneville, editor of the *Moniteur Scientifique*, presented a set of his papers.—M. Tremeschini presented three drawings representing one large solar spot seen on the 6th, 7th, and 8th of May at noon. These drawings are inserted in the *Comptes Rendus*. M. Tremeschini lives at Belleville, the spot where the rebellion fought its last desperate struggle. It is to be hoped that he escaped safe, though up to this moment nothing has been heard from him.

## BOOKS RECEIVED

English.—A Memoir of the Indian Survey: C. R. Markham (India Office).—Light Science for Leisure Hours: R A. Proctor (Longmans).—At Last, 2 vols.: Rev. Canon Kingsley (Macmillan and Co.).—The Modes of Origin of Lowest Organisms: Dr H. C. Bastian (Macmillan and Co.).

Foreign.—(Through Williams and Norgate)—Lehrbuch der Mechanik: Dr. Wernicke.—Le Soleil: Padre Secchi

## DIARY

*THURSDAY*, June 8.

Society of Antiquaries, at 8.30.—On the important Excavations in Rome during the present season: J. H Parker, F.S.A.

Mathematical Society, at 8.—On Plücker's Models of Certain Quartic Surfaces: Prof. Cayley.—On the Motion of a Plane under certain Conditions: Mr. S. Roberts.

Royal Institution, at 3.—Sound: Prof. Tyndall.

*FRIDAY*, June 9.

Astronomical Society, at 8.

Quekett Microscopical Club, at 8.

Royal Institution, at 9.—On Dust and Smoke: Prof. Tyndall.

*SATURDAY*, June 10.

Royal Institution, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

*MONDAY*, June 12.

Royal Geographical Society, at 8.30.

*TUESDAY*, June 13.

Photographic Society, at 8.

*THURSDAY*, June 15.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Chemical Society, at 8.—An Experimental Inquiry as to the Action of Electricity upon Oxygen: Sir B. C. Brodie, Bart.

Linnean Society, at 8.—On British Spiders: Rev. O P. Cambridge.—On a Luminous Coleopterous Larva: Dr. Burmeister.

## CONTENTS



Errata.—Vol. iv., p. 95, 2nd column, line 30, for "R. T. Friswell" read "R. J. Friswell"; line 37, for "$Fl_2$" read "$Tl_2$"; for "$FlO_2$" read "$TlO_2$."

THURSDAY, JUNE 15, 1871

## *PRIMITIVE CULTURE* *

I.

WHEN the commencement of Mr. Buckle's great "Introduction" appeared, some fourteen years ago, no small controversy arose as to the possibility of constructing a Science of History. On the one hand it was argued that for two or three centuries past every generation had demonstrated certain events to be regular and predictable, which previous generations had considered irregular and unpredictable; had generalised facts which it was supposed were incapable of being generalised; and had indicated the existence of order, method, and law, in events which earlier ages had regarded as regulated only by the fitful vagaries of a blind chance, or the inscrutable decrees of a supernatural interference. On the other hand, it was asserted that, even supposing the universal prevalence of law and order to be proved, our necessary nescience would still remain so totally unenlightened with regard to the operation of the law and the sequence of the order, that no ingenuity could achieve such a classification of human motives and actions as could justly be dignified with the name of a science. Since then we have passed through what amounts to a scientific revolution. Not only has archæology vastly extended the limit of its domain, but the doctrine of evolution—itself the most striking generalisation deduced from a comparison of the world's present with the world's past—points decisively to archæology as the most fruitful province of inquiry to the student of the science of History. Before Buckle wrote, archæology had indeed already discovered more than one new world for the conquest of modern science. In the last generation, the archæology of organic nature, brought to light by geology, had afforded a sure basis for the science of Comparative Anatomy; and in a precisely analogous manner the archæology of language and religious worship, revealed in the early literary monuments of India, Assyria, and Egypt, had more recently altogether regenerated the science of Comparative Philology, and created that of Comparative Mythology. But the value and importance of archæological research in other directions had not yet been understood and appreciated. It was not until the discoveries of human implements and remains in the drift and cavern deposits had directed attention to the multifarious problems presented by primitive culture, that investigators began to regard the sciences of Language and Religion as merely departments of the more general and comprehensive science of Comparative Civilisation, and to recognise the fact that the science of Comparative Civilisation is the very corner-stone of any real science of History. As indicating the direction of scientific research, it is significant that Mr. Darwin's last work, which surely should have been entitled the "Ascent" rather than the "Descent of Man," should be so closely followed by the volumes of Mr. Tylor on Primitive Culture. The main argument, indeed, of both writers is fundamentally the same. The difference between them is that Mr. Darwin traces it out in connection with what man *is*, Mr. Tylor in connection with what man *does*. One applies the theory of evolution to man in relation to organic nature, the other to man in relation to human culture. Both, too, have pursued the same method. It was no part of Mr. Darwin's design to write an exhaustive physical history of mankind, or of Mr. Tylor's to detail the history of civilisation. Each has selected the most salient and significant points to illustrate his argument, and has instanced only sufficient facts to supply a reasonable proof of the propositions enunciated.

It is not, however, merely as an exponent of the theory of development that Mr. Tylor has taken his work in hand. Leibnitz long ago pointed out the supreme importance of a study of mankind in connection with that of what he terms the natural history of the world, in order to ascertain what ought to be introduced and what banished from among men. This principle Mr. Tylor has recognised throughout, and the facts he brings forward have quite as often been selected for the light they throw on vexed questions of the day as for the illustration they afford of the theory of evolution.

One great stumbling-block in the way of the student of culture is the extreme imperfection of the only records to which he has access. The comparative anatomist, however, who is perhaps even more closely beset by the same difficulty, has pointed out the means by which it may to a great extent be effectually overcome. If analogy be as trustworthy in the one case as in the other, the historian of culture can study the past in the present with the same confidence as the anatomist, and can as readily reconstruct the shape of human society in primeval ages as his fellow-worker can restore the outward form of an extinct flora and fauna from their fossil remains. But is this analogy to be trusted? Can it be demonstrated that any such vital connection exists between antique and modern barbarism as will enable the inquirer to study prehistoric culture in that of still-existing races, savage, barbaric, or semi-civilised? Can it be proved that savage, barbaric, and civilised life are really correlated as various stages of growth and development? To these questions Mr. Tylor's work supplies a satisfactory answer. Carefully reviewing a number of the most important departments of culture, he proves the existence in all of innumerable relics—the fossils, as it were, of primeval thought and life—traces the modes of connecion of one age with another in progress, degradation, survival, revival, and modification, and demonstrates the utter inadequacy of any theory but one of development to explain the complex and varied phenomena of civilisation. Survival in culture, the origin of language, the art of numbers, mythology, religion, rites and ceremonies, are each in turn discussed, and it is not too much to say that the extent of research, the rare felicity of illustration, the breadth of view and signal originality which Mr. Tylor has brought to bear on these subjects really render the appearance of his work an epoch in the annals of the philosophy of history.

To follow Mr. Tylor through his entire argument, and the evidence he produces in support of it, would be to write a somewhat larger work than his own. We can here only indicate the general method he has pursued, and comment briefly on a few facts which he has collected. Commencing with a general survey of the science of culture, he proceeds to give a rough outline of the course of its development. In so doing, he necessarily touches

* "Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Art, and Custom." By Edward B. Tylor, author of "Researches into Early History of Mankind," &c. Two vols. 8vo. (London: Murray, 1871.)

on the controversy between the upholders of the two theories of development and degradation, of whom Sir J. Lubbock and the Duke of Argyll are among the latest representatives. "The master-key," he well observes, "to the investigation of man's primeval condition is held by pre-historic archæology. This key is the evidence of the Stone Age, proving that men of remotely ancient ages were in the savage state." While he shows, however, that the study of archæology has gradually cut away the ground under the feet of those who, like Archbishop Whately and the Duke of Argyll, appear to consider that civilisation was originally created in a state of happy mediocrity, from which it has since more frequently fallen than risen, he is careful at the same time to recognise the agency of degradation as secondary only to that of progress. One circumstance in connection with this argument has perhaps hardly been sufficiently considered by the advocates of either side. The distribution of mankind over the face of the globe is an event for the most part belonging to pre-historic ages, but it is quite clear in some cases, and strongly probable in many others, that the occupation of new territories widely divided by the sea from the earlier inhabited portions of the world, was the result of seafaring disaster; that, in fact, the first denizens of many islands, and perhaps of some continents, were the shipwrecked crews of primeval canoes, cut off from further intercourse with their countrymen, destitute of all the materials and appliances of such rude culture as they may once have possessed, and ignorant of even the primitive industrial arts necessary to utilise them even if they were at hand. Under such circumstances—and a consideration of the actual distribution of mankind in historic times countenances the supposition that the contingency must have occurred over and over again—the march of degradation must have been certain and swift; and even allowing that in the case of mariners belonging to a somewhat advanced tribe, the degradation might be only temporary, the event would account for at least some portion of the diversity which is only less striking than the uniformity perceptible in the various civilisations of the world. Be this, however, as it may, the entire evidence available on the subject fully bears out Mr. Tylor's conclusion, that "throughout the whole vast range of the history of human thought and habit, while civilisation has to contend not only with survivals from lower levels, but also with degeneration within its own borders, it yet proves capable of overcoming both and taking its own course. History within its proper field, and Ethnology over a wide range, combine to show that the institutions which can best hold their own in the world gradually supersede the less fit ones, and that this incessant conflict determines the general resultant course of culture.

The next two chapters are devoted to "Survival in Culture," the strange permanence in the midst of a higher civilisation of certain customs, arts, opinions, &c., long after the real and earnest meaning has died out of them, which in a lower stage commended them to acceptance. Among these metamorphic remains of an earlier world are many, if not most, of the games, rhymes, proverbs, riddles, and minor social customs of civilised peoples. A notable instance is to be found in archery. "Ancient and widespread in savage culture, we trace the bow and arrow through barbaric and classic life and onward to a high mediæval level. But now, when we look at an archery meeting, or go by country lanes when toy bows and arrows are 'in' among the children, we see, reduced to a mere sportive survival, the ancient weapon which, among a few savage tribes, still keeps its deadly place in the hunt and the battle." In another passage Mr. Tylor remarks: "the practice of poisoning arrows after the manner of stings and serpents' fangs is no civilised device, but a characteristic of lower life, which is generally discarded, even at the barbaric stage." Perhaps one of the most striking instances of linguistic survival is to be found in the word "intoxication," derived from "toxicon," the material employed for poisoning the arrow. Among other instances of survival, Mr. Tylor quotes the custom of casting lots. It is noteworthy that both Wesley and Whitfield in certain cases employed this means of ascertaining what they considered the Divine will, and that even yet many Englishmen are to be found who attach under certain circumstances the old sacred significance to the process. That the theory of survival suggested by Mr. Tylor does really account for nearly all the otherwise utterly unaccountable customs and ways in vogue among civilised nations, will not be doubted by anyone who has taken the trouble to trace their history in any considerable number of cases. It is not, for example, many years since the present Lord Leigh was accused of having built an obnoxious person—one account, if we remember right, said eight obnoxious persons—into the foundation of a bridge at Stoneleigh. Of course so preposterous a charge carried on its face its own sufficient refutation; but the fact that it was brought at all is a singular instance of the almost incredible vitality of old traditions. The real origin of a story such as this dates from a time when the foundations of bridges, palaces, and temples were really laid upon human victims, a practice the tradition of which is handed down to us in the Romance of Merlin, and a thousand other legends old and new, to be finally embalmed for the benefit of posterity in Mr. Tylor's volumes. The most telling, however, of all Mr. Tylor's instances of survival are those which bear upon the history of modern spiritualism.

"Beside the question," he observes, "of the absolute truth or falsity of the alleged possessions, manes-oracles, doubles, brain-waves, furniture movings, and the rest, there remains the history of spiritualistic belief as a matter of opinion. Hereby it appears that the received spiritualistic theory of the alleged phenomena belongs to the philosophy of savages."

This conclusion may possibly astonish and even "exercise" the spirits of some of the faithful; but assuredly it is abundantly borne out by the evidence adduced, which parallels with most afflicting minuteness the various phenomena of spiritualism from mediæval story and tales of witchcraft, from classic fable and ecclesiastic miracle, from Chinese divination and Indian divinity, from the feats of North American mountebanks, the hocus-pocus of the angekoks in Greenland, the juggleries of the Siberian shamans. Even this array of evidence, however, is but a fraction of what might be produced. Mr. Tylor quotes Lucian's Hyperborean, who flew and walked on the water clad in undressed leather breeks, and who by the way is possibly only an allotropic form of our own Regnar Lodbrok; but he spares us that other

Hyperborean, Abaris, "the air-walker," to whom Pythagoras, the Miss Kilmansegg of antiquity, displayed his precious leg. In fact here, as elsewhere, Mr. Tylor has acted on the principle that the half is greater than the whole. He selects enough for his purpose, and resolutely declines to overburden himself with superfluous testimony. Fortunately there are two sides to the theory of survival. If on the one hand we have survivals of the type of modern spiritualism, we have on the other survivals of ideas, which, first broached in a stage of civilisation when they are considered foolish or mischievous, become in a higher stage the dominant influences which direct human opinion. To take a single case :—It is now near upon two centuries since Balthazar Bekker, a D.D. of Amsterdam, corrupted, may be, by certain impious notions propounded by the arch-infidel Descartes, published his "Monde Enchanté," a crime for which he was at once deprived of his benefice ; since, as a learned Englishman remarked in reference to the case :—

> Dæmonas ex mundo quisquis proscripserit audax,
> Esse brevi nullum dicet in orbe Deum.

If the English reader of to-day will take the trouble to read this work—and it is worth the trouble—he can scarcely fail to be struck with the remarkable survival of the ideas contained in it, expanded, corrected, developed as they are in these chapters by Mr. Tylor. Not that Mr. Tylor has borrowed anything from Bekker, but simply that Bekker was the first, as Mr. Tylor is the last, to apply science systematically to the phenomena of sorcery, witchcraft, and spiritualism of his age. Survivals of this kind are indeed proofs as decisive of the vitality of civilisation as survivals of the other kind are of the vitality of barbarism.

In the following chapters on Language, emotional and imitative, Mr. Tylor makes out a strong case in favour of what Prof. Max Müller, with a felicity worthy of a better cause, has nicknamed the "pooh-pooh" and "bow-wow" theories. "It may be shown," he says, "within the limits of the most strict and sober argument, that the theory of the origin of language, in natural and directly expressive sounds, does account for a considerable fraction of the existing *copia verborum*, while it raises a presumption that, could we trace the history of words more fully, it would account for far more." Among other matters touched on in this inquiry, Mr. Tylor refers to the language employed in addressing beasts, particularly dogs and horses. Some curious samples of dog-language are to be found in the Book of St. Alban's, and, indeed, in almost every old treatise on hunting. Sir Tristram, however, the hero of the Arthurian cycle, who is generally considered the *rédacteur en chef* of this particular dialect, appears to have thought plain Norman French best adapted to the intelligence of greyhounds, and is very sparing in his use of mere "brutish interjections." Of horse-language one of the best examples is to be found in "The Enterlude of John Bon and Mast Person," a tract belonging to the middle of the sixteenth century. This is how John Bon addresses his team :—

> Ha, browne done ! forth that horson crabbe !
> Ree, comomyne, garlde, with haight blake hab !
> Have agayne, bald-before, bayght ree who !
> Cherly boy, cum of, that whomwarde we may goo !

One branch of inquiry into which Mr. Tylor partly enters in these chapters and the following one on the Art of Numbers, appears to deserve closer attention than it has yet received. Considering the important part which gesture plays in all the lower languages, it is a fair hypothetical inference that, as language gradually became more and more developed, a number of words and phrases would creep into it, formed on the principle of translating gesture into phonetics. Thus, for instance, the universal gesture for "likeness" or "sameness" is to hold out both hands together. If, in several different languages, the words meaning "likeness" or "sameness" have an etymological connection with the word meaning "together," a strong presumption would be raised that they were translated from the gesture ; and if any large number of correspondences of the same kind were detected, the presumption would be raised into a theoretical certainty. Whether such evidence exists of the translation of action into sound in general language, none could determine better than Mr. Tylor himself, whose essay on the gesture-language in one of his earlier works, forms really almost a complete handbook on the subject. That it does exist in language, as applied to numbers, is clearly shown in his chapter on the art of counting, where he traces the quinary, decimal, and vigesimal systems to their origin in the fact that the average man possesses five fingers on each hand, and as many toes on each foot. He perhaps, however, has not sufficiently noticed the further strong probability that the duodecimal system owes its origin to the circumstance that, in addition to his fingers and toes, a man possesses two hands and two feet—a consideration not without its bearing on the obscurity attending the numerals eleven and twelve in certain languages.

## *LEA'S UNIONIDÆ*

*A Synopsis of the Family Unionidæ.* By Isaac Lea, LL.D. 4th edition. 4to. (Philadelphia, 1870.)

THIS work, by a veteran American conchologist, contains 184 pages, and is a memorial of his labour and zeal during a period of more than forty years. The *Unionidæ* are generally known as "fresh-water mussels." Their variability is notorious ; for almost every river, lake, and pond yields different forms, which some writers call species and others call varieties.

> Non nostrum est tantas componere lites.

But while giving Dr. Lea ample discretion to make as many species as he pleases, and full credit for his honest wish to keep down the number, it certainly strikes one as somewhat singular that he admits only "seven or eight species of the family *Unionidæ* living in Europe," when he enumerates 720 species as North American, of which latter number he has himself described no fewer than 582 ! According to Kreglinger's catalogue, which is the newest on the land and fresh-water shells of Europe, fifteen species of this family inhabit Germany. We have but five, including one debateable species of *Anodonta.* The total number of living species recognised by Dr. Lea is 1,069, besides 224 unknown to him or doubtful. To distinguish varieties from species is one of the great difficulties which perplex the naturalist ; but the rule which I have adopted may serve the purpose to a considerable extent, viz., "that all distinct groups of individuals living together and having a common feeding-ground, and which are not connected or blended with each other by insensible gradations, are *primâ facie* entitled to the rank of species." (British Conchology, vol. i., Introduction, p. xix). Now we may see several species of *Rissoa* living under the same stone between tide-marks, several species of *Limnæa* in the same stream or ditch, and more than one species of *Helix* feeding together on the same leaf. In such cases there is no fusion or confusion of species; each has its own definite limits, and retains its own peculiar characters. I say nothing of genera and more comprehensive groups which form communities in a still more diversified fashion, but are equally free from intermixture.

J. GWYN JEFFREYS

## OUR BOOK SHELF

*Echinides du Département de la Sarthe, considérés au point de vue zoologique et stratigraphique.* Par Cotteau et Triger. (Paris: Baillière, 1855-1869. London: Williams and Norgate.)

WE fear that some time must elapse before science will resume its place in unhappy France; but in the meantime its professors, who are innocent of the mischievous and insane acts which have caused so much ruin, demand our heartfelt sympathy. M. Cotteau, of Auxerre, whose work we are about to notice, is well known to English geologists, and is highly esteemed by them for his long and conscientious labours in the field of Mesozoic echinology. His coadjutor, M. Triger, died during the progress of the work. It consists of two royal octavo volumes, one containing an account of Echinoderms found in the Jurassic and Cretaceous formations in the Department of the Sarthe, the other having sixty-five well-executed plates of species, besides several charts to show their geological and stratigraphical distribution. It appears from the preface that this most creditable production of French palæontology was commenced in 1857, and finished in 1869. We therefore regret to observe that M. Cotteau was not aware of Dr. Wright's admirable monograph on British fossil Echinodermata, which was published by our Palæontographical Society in 1856, and which goes over a great deal of the same ground as M. Cotteau. Had the latter author consulted it, he would probably have avoided some mistakes, *e.g.* in attributing the specific name of *Pseudodiadema hemisphæricum* to Desor instead of to Agassiz. A comparison of the figures of this and other species given in both works is decidedly favourable to the British artist (Mr. Bone) as regards accuracy and completeness, although MM. Levasseur and Humbert are deservedly eminent in their style of lithography.

The Echinoderms found in the Jurassic and Cretaceous formations must have inhabited a soft bottom in seas of considerable depth, judging from the present habits of allied species; and their variability was not less in those remote periods of the world's history than it was in the epochs which preceded and followed.

J. G. J.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Paris Observatory

HAVING read only yesterday the agonising account written by M. Marie-Davy and countersigned by M. Delaunay, descriptive of the Communists having made the Paris Observatory one of their chief strategical points, of the domes of the observatory having five hundred bullet-holes through them, and of the rabid attempts made by the citizens in arms, before retiring on the approach of the Versaillists, to burn or blow up the whole building, I am not a little surprised to find in NATURE of June 8, received here this morning, a statement to the effect "that the Paris Observatory had suffered scarcely any injury up to the end of the second siege. No delegate of the Commune had presented himself either to take possession of it or to blow it up."

I presume that you wrote in ignorance of the real facts, and perhaps not without some intention of whitewashing the poor Communists from the exaggerated denunciations which have been poured on them since their fall; yet neither they nor you should object to true accounts of what they actually did while in power appearing before the world without menace and without favour.

The mere showing of the Commune during this second siege, and still more its international organisation, seems to have surprised most persons; yet the character of the association, and its imminence under the feet of all the Governments of Europe, was duly noted in the section on Metrological Legislation of my report presented to the Board of Visitors of the Royal Observatory, Edinburgh, in June 1870; the association, though political, having obtained mention there on account of its having adopted the scientific metrical system of weights and measures, and professing to find it a most efficient agent for assisting in breaking down the barriers between nations, and rooting up traditional customs and beliefs. I must confess, however, that I was not prepared for these revolutionaries taking up so very early in their outward career, as this their first and just-concluded essay in Paris, the chronological department of the metrical system, thereby repudiating, as the order found on General Delescluze indubitably shows, both the Christian Era and the accustomed months, for decimal periods of days and the era of the first French Revolution. In my book, "Our Inheritance in the Great Pyramid," published in 1864, I did indeed remind that that most revolutionary method in chronology was originally a part of the metrical system, and though deposed under Napoleon Bonaparte, might be expected to reappear when the present promoters of French metrology in this country had acquired more boldness; but here is the accomplished fact upon us at this very moment, and it would be well for all those metrical agitators who were so loud at the British Association last summer in Liverpool in their outcries to Government to make the metrical system compulsory throughout this country, now to declare honestly whether they are inwardly with the Communists in desiring ultimately the abolition of the Christian era and the destruction of the week of seven days.

C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, June 9

### Science Lectures for the People

IT is all very well to say, Let our children be taught science in the schools; but that does not meet the need of a large section of the nation, the product of the schools of a former generation. Many hard-worked men who had no scientific teaching whilst at school, have now acquired the wish to know more of nature's mysteries, but know not whither to turn for aid. Books are plentiful, but it is very tiresome to wade through dry pages, scientifically dried of their sap by the use of terms which are not commonly understood—especially after the wearying labours of the day. Experimental lectures, like those at the Royal Institution, but a little more specialised, are wanted for popular use; the question is, How are we to get them? Are we to go to Government for aid, or shall we bestir ourselves and voluntarily endow these lectures?

Surely Huxley or Tyndall would be quite as much sought after as Spurgeon if they came forward and announced a series of lectures; St. James's Hall would be as crowded as the Tabernacle if they held a weekly lecture; pew-rents would be as certain of collection from scientific as from religious devotees. Those busily engaged professors can indeed hardly be asked to undertake such a task as this; but any competent man of science, able to explain the facts of science in popular language, might reckon on public support if he made such a venture as this. Let him, for example, give a series of twelve lectures on Biology, as it affects our daily existence; not wandering into the remote regions of extraordinary phenomena, but simply expounding ordinary life laws. Here would be a subject refreshingly new and interesting to thousands of City-born and bred toilers.

The lectures, if on week days, must be after office hours—from nine o'clock to ten, say; and in some hall easily accessible, as St. James's.

GEORGE FRASER

169, Camden Road, N.W.

### The Eclipse Photographs

IT would have given me much pleasure to have shown Mr. Winstanley the original negatives of the photographs of the late eclipse of the sun if he had called on me to see them, and by so doing he would have avoided falling into the mistakes which his letter contains.

At the time when the last photograph was taken the sky was perfectly clear, and unless Mr. Winstanley is in possession of exclusive information he has no right to assume that the American photograph was not taken under equally favourable conditions. Some of my photographs (which Mr. Winstanley cannot have seen) were taken through the edges of a cloud, the whole of which could be covered by the hand when held with the arm extended; and there was a perfectly cloudless sky near the sun excepting towards the east.

The imperfection in my No. 5 picture, which Mr. Winstanley's experienced eye detects, arose from the shaking of the telescope, caused by the high wind blowing at the time. Probably a single gust during the eight seconds while the plate was exposed caused the mischief, and this defect would never have been seen but for the extremely actinic power of the red prominences which leave their impression on the sensitive plate instantaneously. The moon's limb is perfectly sharp, excepting where the red prominences appear.

Let it be clearly understood that this "indifferent definition" refers to the moon's limb only; the details of the corona do not appear to have suffered; after the gusts of wind the telescope has returned to its proper position, and Mr. Winstanley must know from experience that the image of an object giving off feeble light would not be materially injured by a slight blow given to a firmly mounted camera.

Mr. Winstanley says that "the identity of the coronal rifts in the Cadiz and Syracuse photographs is not satisfactorily conclusive." Assertion is not proof. In NATURE of March 9 I gave evidence which appeared to me to be conclusive (I need not here refer to the opinions of others who are equally satisfied), and up to the present time no counter-evidence has been produced.

It is not for me to defend the American photograph. In due time we shall know all about how that was produced. But has Mr. Winstanley failed to notice that the light on the moon's disc does not extend all round and all over it as it would do if caused by our atmosphere? It is chiefly on the east and west sides. We may expect the explanation of this defect when we hear how it happens that the corona in this photograph is cut off instead of extending as in all the other photographs.

I fail altogether to see the connection between the solar corona and a lunar halo—the phenomena bear no resemblance to each other. The solar corona comes close up to the perfectly black disc of the moon. I never saw a lunar halo close up to the moon's limb. When seen through a mist or in a "sky burdened with innumerable clouds," there can be no doubt that the lunar surface is obscured by the moisture in our atmosphere.

A. BROTHERS

### Ocean Currents

HAVING had occasion in the spring of 1868 to consider the subject of Ocean Currents as discussed by Captain Maury and Sir John Herschel, I was led to certain views respecting the origin of the oceanic circulation, which are briefly touched upon in a paper which appeared in the *Student* for July, 1868. At that time an experimental test of my theory (or rather of that portion of the theory I advocated, which was, as I judged, novel) occurred to me. The experiment might, I conceive, be very readily tried. It somewhat resembled that by which Dr. Carpenter illustrated lately at the Royal Institution his views respecting the influences of evaporation and polar cold; but as I wished specially to show how the westwardly equatorial current came about, the experiment was somewhat more complex. Let the circumference of a large and shallow cylindrical basin represent the equator and the central part the north polar regions. Within this cylinder let solid matter be so placed as to represent the northern halves of the continent, in such sort that the resulting configuration would correspond to that of a map of the northern hemisphere (say on the equidistant projection). Let sea-water be poured in to represent the northern portions of the terrestrial oceans. Now to represent the Arctic ice-fields, let lumps of ice be placed at the centre of the cylindrical vessel (they should be circled round by a wire-guard); and to represent the effects of equatorial heat, let a stout iron ring round and above the rim of the cylindrical vessel be heated. In this state of things the process of circulation, which actually took place in Dr. Carpenter's experiment, would take place after such modified sort as the contour of the continent masses permitted. Now suppose that the cylindrical vessel is set in steady and somewhat slow rotation about its axis. It is clear that on the currents flowing from the pole and polewards, effects will be produced which precisely resemble those due to our earth's rotation. If I am right in regarding these effects as the true cause of the direction in which the equatorial currents, the Gulf Stream, and in fact all the currents in open ocean are observed to flow, abundant evidence to that effect will be obtained. If no such evidence be obtained, the westwardly direction of the equatorial currents must, I imagine, be ascribed to the trade winds, as Franklin and Sir J. Herschel have maintained.

In the summer of 1868 I suggested to Prof. Pepper that such a contrivance as the above, if it worked as I judged (and still judge) that it would, would form an interesting and instructive addition to the models exhibited at the Polytechnic Institution. Dr. Carpenter has already proved that the vertical circulation takes place in an experiment of this sort. If the eastwardly and westwardly circulation takes place as I expect, the expe imental illustration of oceanic circulation would be singularly complete. The circulation in the southern hemisphere could be illustrated in like manner.

I may note here that the vast distance separating the polar from the equatorial regions must not be overlooked in theories respecting oceanic circulation. The influence of arctic cold may be paramount in very high latitudes; but equatorial evaporation must, it should seem, be the prime moving cause in tropical and sub-tropical regions.

RICHARD A. PROCTOR

Brighton, June 6

### Day Auroras

I HAVE read attentively the numerous letters which have appeared in your columns on this subject; but so far as I can discern it seems not to have occurred to any of your correspondents that the auroral force, whatever it may be, affects every kind of cloud as well as the cirrus. On June 15, 1870, at 9 A.M., I witnessed here as complete a display of auroral motions in the cirrus cloud as ever I beheld in a midnight sky; and from that date I dismissed in my own mind all doubt as to the identity of auroral force, whether seen by day affecting the cirrus cloud or appearing as streams and rays of light at night. On Thursday and Friday last I witnessed a configuration of cirro-stratus cloud, evidently the result of magnetic polarisation, which I have no hesitation in characterising as auroral. There were on Thursday two poles, both in the line of the magnetic meridian; but on Friday night, at nine o'clock, only one pole in the direction of the true meridian. The phenomenon to which I refer is of very frequent occurrence, especially before a track of fine warm weather; and without at present offering a theory on the subject of auroras, I venture to class polarisations of clouds, whether cirrus or not, as arising from the same cause as luminous aurora. The transverse or dia-magnetic lines are generally as well marked in cloud auroras, and it is an interesting task to watch the transformation of cirrus cloud from the meridional to the equatorial direction. I have also noted that when these auroral lines converge towards the magnetic pole, a steady barometer and fine weather ensue; but that when the transverse or equatorial lines predominate and continue long visible, rain soon follows. These transverse lines of cloud are always lower and seem to be dia-magnetic.

D. LOW

Burntisland, May 22

PERHAPS you will allow me to add my mite to the discussion which has been going on in your pages on the question of the visibility of the aurora in daylight.

On the 3rd of September in last year, when at Nairn in the north of Scotland, I witnessed an aurora, such as I never heard or read of, or saw before; and strangely enough it was not noticed, as far as I am aware, in any of the newspapers. I had gone down to the beach at about 10 15 P.M., and immediately noticed what appeared at first to be a kind of haze over the whole sky, which slightly dimmed the light of the stars.

For a few minutes I thought no more about it, but, happening to turn my eyes towards the zenith, there was a sight I never shall forget. A number of sheets of whitish light were constantly darting with a flickering motion from the surrounding haze of similar light, and meeting in the zenith; the length of their course was as much as 15° to 18°; they appeared to proceed indiscriminately from all points in azimuth.

I immediately became aware that the whole sky, down to the very horizon, was illumined by a white, colourless aurora; but I was so fascinated by the incessant play of the streamers overhead, that for some time I could notice nothing else. At last I turned away in order to observe accurately the full extent of the aurora in all directions. I found that it reached quite down to the horizon all round, except in one place, viz., in the S.S.E., and at that point there was a symmetrical arc (of a great circle, as far as I could judge) the summit of which was about 7° or 8° above the horizon. This arc was perfectly well defined; within it was blue sky, and above and around, over the whole heavens, nothing but the auroral light, except in the gaps between the darting streamers in the zenith. I carefully took the bearings of this remarkable arc, and found by means of a compass the next day, that it was bisected by the magnetic meridian. The phenomena underwent no diminution during the time (an hour and a half) I was watching it. The sky appeared to be quite free from cloud.

I have often witnessed fine displays of aurora; one in the winter of (I think) 1848, in this county, the colours and streamers of which were magnificent, far finer than those I saw on the 24th and 25th of last October at Edinburgh; but I never before observed the stars to be so much dimmed as they were at Nairn in September, notwithstanding the light on that occasion was colourless. On other occasions, I have always thought the stars quite unaffected by the auroral light, both to the naked eye and in the telescope, but on this they were obviously dimmed as by a haze.

My impression is, that no aurora that I ever saw could be visible in daylight, with the exception perhaps of this last, and the only portion of this that could possibly be seen in daylight was the well-defined arc low down in the S.S.E. I think it is just possible that in a clear and cloudless sky such an arc as this might be visible. HENRY COOPER KEY

Stretton Rectory, Hereford, June 6

## Red and Blue

I WAS much interested by the letter of Mr. T. Ward (NATURE, vol. iv. p. 68) describing the appearance of a blue colour when looking at white chalk marks on a black board while the sun was shining in the eyes, as I have frequently noticed a precisely complementary phenomenon.

While walking along the chalky roads of East Kent in bright sunshine, and reading under an umbrella, I have frequently noticed that the letters appear of a deep blood-red colour; the black colour of the type reappearing immediately on passing over the shadow of a tree on the ground, or on allowing the sun to shine directly on the book. This was so striking when first seen that I had to convince myself that the page was not printed in red ink. This is obviously the exact converse of the observation of Mr. Ward, who saw a blue colour from white marks on a black surface, while I saw a red colour from black marks on a white surface. A short time since I observed a precisely similar colour when looking down on the platform of a railway station with the setting sun shining on the eyes, the cracks between the boards also appearing red. HERBERT M'LEOD

## Influence of Barometric Pressure on Ocean Currents

A LOW barometric pressure and an increased height of the ocean was, I believe, first assumed to stand in the relation of cause and effect by Mr. Piddington. The abnormally high tidal waves that sometimes rushed up the Hooghly during Calcutta hurricanes were ascribed to the low pressures which accompanied them. There is no doubt that unequal pressure is a true cause of currents in the ocean. But I think it, as well as difference in specific gravity, may be regarded as infinitesimal in amount, compared to the influence of the winds.

The high tidal waves at the mouth of Hooghly are not experienced during the first stage of the hurricane, or so long as the wind blows from a northerly quarter. The waters in the Bay of Bengal are then propelled towards the south. It is only after the wind changes to the south and the barometer is rising that the waters are driven against its northern shores. It is when the wind happens to change to the south at the time of the flow of the tide that great inundations occur in the Delta.

On the coast of the United States, however, there is apparently an intimate relation between low pressures and high tides. In general, so long as the barometer remains low, easterly winds are blowing on the coast and heaping up the waters in every bay. During the second stage of the storms the winds generally blow violently from the west or north-west, often at a right angle to the whole coast. These high westerly gales cause very low tides along the United States when the barometer is rapidly rising. But low tides are not experienced with high pressures if the air is calm.

High tides only occur on the coast of Europe after westerly winds have been blowing for some days in the Atlantic. It is a well-recognised fact among the fishermen on the east coast of Scotland that high tides are due to this cause. I think Hugh Miller was right in maintaining that the friction of the south-west winds on the wide surface of the Atlantic must be quite as powerful in maintaining the flow of the stream through the Florida Channel as the action of the Trade winds in forcing the tropical waters into the Gulf of Mexico.

Owing to the great rapidity with which barometric disturbances are propagated in our temperate latitudes, it is difficult to conceive how barometric pressure of itself can have an appreciable influence on the currents of the ocean. The rate of their propagation in winter is from thirty to sixty miles an hour. In the fourth number of the "Board of Trade Weather Report" an instance is given, in which the rate is affirmed to be upwards of seventy miles an hour. The velocity in this case, however, as I may try to show on another occasion, is estimated about ten miles an hour too high. But let us suppose that no winds accompanied these rapidly propagated depressions and elevations of the barometer. A difference of an inch of pressure existing between places on the ocean two or three hundred miles apart would create only a very slow moving current, even though the diminished area of pressure were stationary. But these low pressures pass so rapidly onwards that the *vis inertiæ* of the waters of the ocean would hardly be overcome before they were again subjected to the opposite influence of an increase of pressure.

The effect of barometric pressure on the level of inland seas, like the Baltic and Mediterranean, must be still less than in the open ocean. Winds are often localised, but great depressions of the barometer extend over immense areas; in most cases far larger than the area of the Baltic. Any higher level from this cause would be brought about by the flow of the waters from either end, as the pressure might be assumed to be the same on both sides of that narrow sea. The mere effect of changes of barometric pressure, it will be admitted, would be quite inappreciable in any inland lake in Britain. But any one who is in the practice of fishing in the smallest of our lakes may always observe that there is an under current or "drag" created when the wind blows strongly towards the shore, in consequence of the accumulation there of its waters. The Niagara is sometimes suddenly raised two feet by strong west winds blowing over lake Erie. It is long since M. Volney, as regards the Mediterranean, stated that east winds caused a rise or flood of from two to three feet in the harbour of Marseilles, and that westerly winds produced opposite effects. It was by a careful deduction of effects produced on a small scale that this acute observer was enabled to give a consistent outline of the causes which produced the ocean currents in general. The currents of the ocean may be regarded as coinciding very closely with the average force and direction of the winds over its surface. Since, however, Humboldt assures us that the surface water of the Gulf Stream in the Florida Straits is sometimes reversed by the force of the winter "northers," it does seem vain to attempt to trace permanent surface currents in any part of the North Atlantic, vexed as its surface is by winds so inconstant in their force and direction.

Pitmuir, Leven, Fifeshire R. RUSSELL

## St. Mary's Loch, Selkirkshire

To the student of Nature it may seem easy to decide whether the water of any given lake is good for domestic uses. But as regards St. Mary's Loch, where the question has to be settled by dint of a squabble in the Auld Reekie municipality, with all its complementary dust, smut, and heat, the true aspects of Nature are liable to misrepresentation.

Although not resident in Edinburgh, nor subject to its prejudices and ratings, your correspondent has taken considerable interest in the subject as discussed both locally and in Parliament. Last week he visited St. Mary's Loch, and took pains to compare it with its very various reputation. He was prepared to find it an oozy swamp, fed by a moorland drainage of bogs and peat mosses.

A true poet is credited with seeing clearer and telling better than other people can, and in this case the credit is fairly earned by Sir Walter Scott, poet laureate of Scottish scenery. In the "Lay of the Last Minstrel" we read of "Fair St. Mary's silver Wave." In "Marmion,"

> Nor fen nor sedge
> Pollute the pure lake's crystal edge.
> Abrupt and sheer the mountains sink
> At once upon the level brink,
> And just a line of pebbly sand
> Marks where the water meets the land.

In plain prose your correspondent saw as follows:—St. Mary's Loch, a practically inexhaustible natural reservoir; in a district pastoral, not moorland. The surface of surrounding hills and flats, formed of loose rock, shingle, or gravel, with sprinkling of light earth, the very type of natural draining to prevent or exterminate bog, morass, and swamp. Not a trace of peat, except peat-reek odour from Tibbie Shiell's chimneys. Tibbie burns peats got from exceptional points high among the hills. The various feeders of the loch run in pure as water can be. That the loch, bedded in a shingly, gravelly flat, and surrounded with bare, smooth, lawn-like hill-slopes, should *appear* to contain brown-tinted water, arises from three concurrent causes. First, the extreme purity of the water; second, the tawny-brown hue of aquatic growths, enveloping the shingle under water, by transmission; third, the pipe-clay whiteness of the dry shingle on the beach, by contrast. From the first-stated cause the water varies in apparent tint according to the bottom hues. Aside from chemical analyses, the relative physical features at once decide St. Mary's Loch to excel the famous Loch Katrine as a water source.

The Edinburgh people are about to celebrate the centenary of Sir Walter Scott. His evidence, before cited, and the occasion, may serve to excuse the spontaneous testimony of

A STUDENT OF NATURE

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### Sun Spots and Earth Temperatures

I NOTICED lately the deduction by Mr. Stone of a connection between Wolf's solar spot periods and the earth temperatures at the Cape of Good Hope: and now Professor Smyth, at Edinburgh, recalls to our attention the fact that his own investigations had, a year ago, led him to a similar conclusion.

Will you permit me to call attention to a further discussion of this subject, as contained in a short article, published in Silliman's "American Journal of Science." The compilations were mostly made in February, 1869, and afford interesting confirmation of the results, which I suppose to have been deduced by Messrs. Smyth and Stone.

CLEVELAND ABBE,
Director Cincinnatti Observatory,
Meteorologist to the Signal Office

Washington, May 6

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### Bessemer Bombs

ALLOW me through the medium of your columns to call the attention of scientific men to the significant inference which, it appears to me, is to be drawn from the formation of "bombs" in the Bessemer process, incidentally described by Mr. Williams in a recent number of NATURE.*

These "bombs" are minute hollow spherules; the smaller are for the most part perforated. These minute hollow spherules are formed of liquid incandescent matter, the smaller showing the true form—perforated spheres.

The point to which I wish to direct the attention of Mr. Williams and other scientific men is this:—May we not have here an experiment which supplements those ingenious ones of M. Plateau on revolving liquid spheres? Mr. Williams will perhaps kindly examine some of the most perfect of these bombs, and let us know whether he sees trace of revolution in their formation. I believe he will find such evidence, and that the revolution is about the perforation. C. E.

Brighton

* See NATURE, vol. iii. p. 410.

## THE STRASBOURG MUSEUM

A BRIEF notice of this Museum may not at this time be devoid of interest. It occupies ten large rooms in the Academy House of Strasbourg. Two rooms are devoted to Comparative Anatomy, and eight to the Zoological, Geological, and Mineralogical collections. One large hall is exclusively devoted to a collection of species indigenous to Alsace, and here its flora and fauna, both fossil and recent, will be found well represented. The large hall of Mammals contains about 2,000 specimens belonging to between 600 and 700 species, among which may be noticed a fine series of Felidæ, including two specimens of the rare *Felis pardina* of Portugal. Among the Ruminants are a grand specimen of the *Ovis nivicola* of Kamtschatka; four specimens of Tragelaphus from the mountains of Constantine, a large series in all stages of growth of the *Antilope rupicapra* from Switzerland, the Carpathians, and the Pyrenees; *Capra semlaica* of the Nilgherries; six specimens of *C. ægagrus* from Kurdistan, of which two are magnificent adult males and the others females and young; *C. walee* from Abyssinia, male and female; nine specimens of *C. hispanica* from the Sierra Nevada; seventeen of *C. ibex*, representing it in all its ages and in all states of wool; not to mention excellent specimens of *C. pyrenaica*, *C. caucasica*, *C. altaica*, and *C. sinaica*; indeed, it may be doubted if there is in any Museum a more complete collection of this interesting group. Of the Antelopes the Museum also possesses a grand series, and the attention in this corner of the hall will be at once attracted by the case of Reindeer, containing eight perfect specimens, representing the wild race of Norway, the domesticated animal of Lapland, and the varieties from Siberia, Greenland, and Labrador. There are also beautifully stuffed specimens of the European and American Bison, and among the Cervidæ we noticed a most interesting little variety from Corsica of *Cervus elaphas*.

Among the Rodents the Collection of Hares and Rabbits from all parts of the world is very fine. The Collection of Madagascar Lemurs is nearly complete. There are also fine specimens of *Colobus ursinus* and *C. vallerosus* from the Gaboon, and a skeleton of the female Gorilla; one of the largest specimens known of the Walrus, and an immense series of Phocidæ from the North Seas. We have omitted to mention two nice specimens of *Chlamydophorus truncatus*.

The Bird Galleries are very extensive, and contain upwards of 5,000 species and nearly 14,000 birds. The Collection of Vultures is very rich; *Gypætus barbatus* from Switzerland, Pyrenees, Sierra Nevada, the Atlas, and Abyssinia; *Aquila pelagica* from Kamtschatka. Of Strix there are about 200 specimens and 60 species. The Birds of Paradise are represented by perfectly fresh specimens of *Semiophora Wallacei*, *Paradisea alba*, *Craspedophora magnifica*, male and female; *Astrapia nigra*, male and female; good specimens of both species of *Cephaloptera; Turacus giganteus; Anas Stelleri*, male and female; *Alca impennis*, a very old specimen. Passing by the grand series of Pelicans, of Grouse we record magnificent specimens of *Oreophatis derlyanus*, *Lophalector Macartneyi*, male and female, *Balaniceps rex*, &c.

The Reptiles and Fishes occupy two large halls.

The Entomological Collection is very fine; a portion of it is exposed to the public in one of the halls; but the greater part is kept in the Cabinet Room. Nothing can surpass the beauty and freshness of the collection of Alsace Insecta. The collection of Coleoptera numbers about 8,000 species.

The Palæontological Collection is arranged according to the geological formations; and one must remark a magnificent example of *Teleosaurus Chapmani*, 12 ft. long; a grand mass of *Pentacrinus fascicularis*, 5 ft. by 3, and containing 15 individuals established on a mass of oysters.

The fossil plants are, as might be expected, very numerous, and many of them are well known through the memoirs, as well as the fine monograph on Fossil Plants, by Dr. Schimper.

In thus noticing some of the chief objects which attracted our attention during a visit to this Museum in January last, it is well also to remember that this grand collection owes its very existence to the life-long labours of Dr. Schimper. Since 1838 he has been the Director of the Museum, which before that time existed only in name. Throughout the terrible bombardment the Museum escaped almost without any damage, and has now become one of the most valuable prizes of war gained by the conquerors. The thanks of the world of science are due to the excellent director of this collection, for all that he has done for it and for science, and we hope we are not wrong in here expressing the wish that, should Prof. Schimper, having been all his life a Frenchman, find it impossible to change his nationality and so continue to reside in Strasbourg, that that new rule which knows so well when it likes how to appreciate the man of science, will not forget to whose care it is indebted for the magnificent prize which it has won.

E. P. W.

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## *DUST AND SMOKE* *

AFTER a few preliminary experiments illustrative of the polarisation of light, Prof. Tyndall adverted to the polarisation of light by fine dust, by the sky, and by the coarser particles of smoke. In the former the direction of maximum polarisation, as in the case of the sky, is at right angles to the illuminating beam. In the latter, according to the observations of Govi, the maximum quantity of polarised light was discharged obliquely to the beam. Govi's observation of a neutral point in such beam, on one side of which the polarisation was positive and on the other side negative, was also referred to. The additional fact was then adduced that the position of the neutral point varied with the density of the smoke. Beginning, for example, with an atmosphere thickened by the dense fumes of incense, resin, or gunpowder, and observing the neutral point, its direction was first observed to be inclined to the beam *towards* the source of illumination. Opening the windows so as to allow the smoke to escape gradually, the neutral point moved down the beam, passed the end of a normal drawn to the beam from the eye, and gradually moved forward several feet down the beam. The speaker did not halt at these observations, they were introduced as the starting point of inquiries of a different nature, and after their introduction the discourse proceeded thus :—

But what, you may ask, is the practical good of these curiosities? And if you so ask, my object is in some sense gained, for I intended to provoke this question. I confess that if we exclude the interest attached to the observation of new facts, and the enhancement of that interest through the knowledge that by-and-by the facts will become the exponents of laws, these curiosities are in themselves worth nothing. They will not enable us to add to our stock of food or drink or clothes or jewellery. But though thus shorn of all usefulness in themselves, they may, by leading the mind into places which it would not otherwise have entered, become the antecedents of practical consequences. In looking, for example, at this illuminated dust, we may ask ourselves what it is. How does it act, not upon a beam of light, but upon our own lungs and stomachs? The question at once assumes a practical character. We find on examination that this dust is organic matter—in part living, in part dead. There are among it particles of ground straw, torn rags, smoke, the pollen of flowers, the spores of fungi, and the germs of other things. But what have they to do with the animal economy? Let me give you an illustration to which my attention has been lately drawn by Mr. George Henry Lewes, who writes to me thus :—

"I wish to direct your attention to the experiments of von Recklingshausen should you happen not to know them. They are striking confirmations of what you say of dust and disease. Last spring, when I was at his laboratory in Würzburg I examined with him blood that had been three weeks, a month, and five weeks, out of the body, preserved in little porcelain cups under glass shades. This blood was living and growing. Not only were the Amœba-like movements of the white corpuscles present, but there were abundant evidences of the growth and development of the corpuscles. I also saw a frog's heart still pulsating which had been removed from the body (I forget how many days, but certainly more than a week). There were other examples of the same persistent vitality or absence of putrefaction. Von Recklingshausen did not attribute this to the absence of germs—germs were not mentioned by him; but when I asked him how he represented the thing to himself, he said the whole mystery of his operation consisted in keeping the blood *free from dirt*. The instruments employed were raised to a red heat just before use, the thread was silver thread and was similarly treated, and the porcelain cups, though not kept free from air, were kept free from currents. He said he often had failures, and these he attributed to particles of dust having escaped his precautions."

Prof. Lister, who has founded upon the removal or destruction of this "dirt" great and numerous improvements in surgery, tells us of the effect of its introduction into the blood of wounds. He informs us what would happen with the extracted blood should the dust get at it. The blood would putrefy and become fetid, and when you examine more closely what putrefaction means, you find the putrefying substance swarming with organic life, the germs of which have been derived from the air.

Another note which I received a day or two ago has a bearing particularly significant at the present time upon this question of dust and dirt, and the wisdom of avoiding them. The note is from Mr. Ellis, of Sloane Street, to whom I owe a debt of gratitude for advice given to me when sorely wounded in the Alps. "I do not know," writes Mr. Ellis, "whether you happened to see the letters, of which I enclose you a reprint, when they appeared in the *Times*. But I want to tell you this in reference to my method of vaccination as here described, because it has, as I think, a relation to the subject of the intake of organic particles from without into the body. Vaccination in the common way is done by scraping off the epidermis, and thrusting into the punctures made by the lancet the vaccine virus. By the method I use (and have used for more than twenty years) the epidermis is lifted by the effusion of serum from below, a result of the irritant cantharadine applied to the skin. The little bleb thus formed is pricked, a drop of fluid let out, and then a fine vaccine point is put into this spot, and after a minute of delay it is withdrawn. The epidermis falls back on the skin and quite excludes the air—and not the air only, but what the air contains.

"Now mark the result—out of hundreds of cases of re-vaccination which I have performed, I have never had a single case of bloodpoisoning or of abscess. By the ordinary way the occurrence of secondary abscess is by no means uncommon, and that of pyæmia is occasionally observed. I attribute the comparative safety of my method entirely, first, to the exclusion of the air and what it contains; and, secondly, to the greater size of the apertures for the inlet of mischief made by the lancet."

I bring these facts forward that they may be sifted and challenged if they be not correct. If they are correct it is needless to dwell upon their importance, nor is it necessary to say that if Mr. Ellis had resigned himself wholly

* Lecture delivered at the Royal Institution, Friday evening, June 9, 1871.

to the guidance of the germ theory he could not have acted more in accordance with the requirements of that theory than he has actually done. It is what the air contains that does the mischief in vaccination. Mr. Ellis's results fall in with the general theory of putrefaction propounded by Schwann, and developed in this country with such striking success by Prof. Lister. They point, if true, to a cause distinct from bad lymph for the failures and occasional mischief incidental to vaccination; and if followed up they may be the means of leaving the irrational opposition to vaccination no ground to stand upon, by removing even the isolated cases of injury on which the opponents of the practice rely.

We are now assuredly in the midst of practical matters. With your permission I will recur once more to a question which has recently occupied a good deal of public attention. You know that as regards the lowest forms of life, the world is divided, and has for a long time been divided, into two parties, the one affirming that you have only to submit absolutely dead matter to certain physical conditions to evolve from it living things; the others, without wishing to set bounds to the power of matter, affirming that in our day no life has ever been found to arise independently of pre-existing life. Many of you are aware that I belong to the party which claims life as a derivative of life. The question has two factors: the evidence, and the mind that judges of the evidence; and you will not forget that it may be purely a mental set or bias on my part that causes me throughout this discussion from beginning to end, to see on the one side dubious facts and defective logic, and on the other side firm reasoning and a knowledge of what rigid experimental inquiry demands. But judged of practically, what, again, has the question of Spontaneous Generation to do with us? Let us see. There are numerous diseases of men and animals that are demonstrably the products of parasitic life, and such disease may take the most terrible epidemic forms, as in the case of the silkworms of France in our day. Now it is in the highest degree important to know whether the parasites in question are spontaneously developed, or are wafted from without to those afflicted with the disease. The means of prevention, if not of cure, would be widely different in the two cases.

But this is by no means all. Besides these universally admitted cases, there is the broad theory now broached and daily growing in strength and clearness—daily, indeed, gaining more and more of assent from the most successful workers and profound thinkers of the medical profession itself—the theory, namely, that contagious disease generally is of this parasitic character. If I had heard or read anything since to cause me to regret having introduced this theory to your notice more than a year ago, I should here frankly express that regret. I would renounce in your presence whatever leaning towards the germ theory my words might then have betrayed. Let me state in two sentences the grounds on which the supporters of the theory rely. From their respective viruses you may plant typhoid fever, scarlatina, or small-pox. What is the crop that arises from this husbandry? As surely as a thistle rises from a thistle seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox virus into small-pox. What is the conclusion that suggests itself here? It is this:—That the thing which we vaguely call a virus is to all intents and purposes a *seed*: that in the whole range of chemical science you cannot point to an action which illustrates this perfect parallelism with the phenomena of life—this demonstrated power of self-multiplication and reproduction. There is, therefore, no hypothesis to account for the phenomena but that which refers them to parasitic life.

And here you see the bearing of the doctrine of Spontaneous Generation upon the question. For if the doctrine continues to be discredited as it has hitherto been, it will follow that the epidemics which spread havoc amougst us from time to time are not spontaneously generated, but that they arise from an ancestral stock whose habitat is the human body itself. It is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease germs which no bad air or foul drains can create, but which may be pushed by foul air into virulent energy of reproduction. You may think I am treading on dangerous ground, that I am putting forth views that may interfere with salutary practice. No such thing. If you wish to learn the impotence of medical science and practice in dealing with contagious diseases, you have only to refer to a recent Harveian oration by Dr. Gull. Such diseases defy the physician. They must burn themselves out. And, indeed, this, though I do not specially insist upon it, would favour the idea of their vital origin. For if the seeds of contagious disease be themselves living things, it will be difficult to destroy either them or their progeny without involving their living habitat in the same destruction.

And I would also ask you to be cautious in accepting the statement which has been so often made, and which is sure to be repeated, that I am quitting my own *métier* when I speak of these things. I am not dealing with professional questions. I am writing no prescription, nor should I venture to draw any conclusion from the condition of your pulse and tongue. I am dealing with a question on which minds accustomed to weigh the value of experimental evidence are alone competent to decide, and regarding which, in its present condition, minds so trained are as capable of forming an opinion as on the phenomena of magnetism and radiant heat. I cannot better conclude this portion of my story than by reading to you an extract from a letter addressed to me some time ago by Dr. William Budd, of Clifton, to whose insight and energy the town of Bristol owes so much in the way of sanitary improvement.

"As to the germ theory itself," writes Dr. Budd, "that is a matter on which I have long since made up my mind. From the day when I first began to think of these subjects, I have never had a doubt that the specific cause of contagious fevers must be living organisms.

"It is impossible, in fact, to make any statement bearing upon the essence or distinctive characters of these fevers, without using terms which are of all others *the most distinctive of life*. Take up the writings of the most violent opponent of the germ theory, and, ten to one, you will find them full of such terms as 'propagation,' 'self-propagation,' 'reproduction,' 'self-multiplication,' and so on. Try as he may—if he has anything to say of those diseases which is characteristic of them—he cannot evade the use of these terms, or the exact equivalents to them. While perfectly applicable to living things, these terms express qualities which are not only inapplicable to common chemical agents, but as far as I can see actually inconceivable of them."

Once, then, established within the body, this evil form of life, if you will allow me to call it so, must run its course. Medicine as yet is powerless to arrest its progress, and the great point to be aimed at is to prevent its access to the body. It was with this thought in my mind that I ventured to recommend, more than a year ago, the use of cotton-wool respirators in infectious places. I would here repeat my belief in their efficacy if properly constructed. But I do not wish to prejudice the use of these respirators in the minds of its opponents by connecting them indissolubly with the germ theory. There are too many trades in England where life is shortened and rendered miserable by the introduction of matters into the lungs which might be kept out of them. Dr. Greenhow has shown the stony grit deposited in the lungs of stonecutters. The black lung of colliers is another case in point

In fact a hundred obvious cases might be cited, and others that are not obvious might be added to them. We should not, for example, think that printing implied labours where the use of cotton-wool respirators might come into play; but I am told that the dust arising from the sorting of the type is very destructive of health. I went some time ago into a manufactory in one of our large towns, where iron vessels are enamelled by coating them with a mineral powder, and subjecting them to a heat sufficient to fuse the powder. The organisation of the establishment was excellent, and one thing only was needed to make it faultless. In a large room a number of women were engaged covering the vessels. The air was laden with the fine dust, and their faces appeared as white and bloodless as the powder with which they worked. By the use of cotton-wool respirators these women might be caused to breathe air more free from suspended matters than that of the open street. Over a year ago I was written to by a Lancashire seedsman, who stated that during the seed season of each year his men suffered horribly from irritation and fever, so that many of them left his service. He asked me could I help him, and I gave him my advice. At the conclusion of the season this year he wrote to me that he had simply folded a little cotton-wool in muslin, and tied it in front of the mouth; that he had passed through the season in comfort and without a single complaint from one of his men.

The substance has also been turned to other uses. An invalid tells me that at night he places a little of the wool before his mouth, slightly moistening it to make it adhere; that he has thereby prolonged his sleep, abated the irritation of his throat, and greatly mitigated a hacking cough from which he had long suffered. In fact, there is no doubt that this substance is capable of manifold useful applications. An objection was urged against the use of it: that it became wet and heated by the breath. While I was casting about for a remedy for this, a friend forwarded to me from Newcastle a form of respirator invented by Mr. Carrick, an hotel-keeper at Glasgow, which meets the case effectually, and, by a slight modification, may be caused to meet it perfectly. The respirator, with its back in part removed, is shown in Fig. 1. It consists of the space under the partition of wire-gauze *q r*, intended by Mr. Carrick for "medicated substances," and which may be filled with cotton-wool. The mouth is placed against the aperture *o*, which fits closely round the lips, and the air enters the mouth through the cotton-wool, by a light valve *v*, which is lifted by the act of inhalation. During exhalation this valve closes; another breath escapes by a second valve, *v'*, into the open air. The wool is thus kept dry and cool; the air in passing through it being filtered of everything it holds in suspension.*

We have thus been led by our first unpractical experiments into a thicket of practical considerations. In taking the next step, a personal peculiarity had some influence upon me. The only kind of fighting in which I take the least delight, is the conflict of man with nature. I like to see a man conquer a peak or quench a conflagration. I remember clearly the interest I took twenty years ago in seeing the firemen of Berlin contending for mastery with a fire which had burst out somewhere near the Brandenburger Thor; and I have often experienced the same interest in the streets of London. Admiring as I do the energy and bravery of our firemen, and having heard that smoke was a greater enemy to them than flame itself, the desire arose of devising a fireman's respirator. But before I describe what has been done in this direction, let me draw your attention to the means hitherto employed to enable a man to live in dense smoke. Thanks to the courtesy of Capt. Shaw, I am enabled to show you the action of the "smoke-jacket," known abroad as the "Appareil Paulin," from its supposed inventor. The jacket is of pliable cowhide. It has arms and a hood, with eye-glasses. With straps and buckles the jacket is tied round the wrists and waist, and a strap which passes between the legs prevents it from rising. On the left side of the jacket is fixed a screw, to which the ordinary hose of the fire-engine is attached, and through the hose air instead of water is urged into the space between the fireman's body and the jacket. It becomes partially inflated, but no pressure of any amount is attainable, because the air, though somewhat retarded, escapes with tolerable freedom from the wrists and waist. Hence the fireman, when his hose is long enough, can deliberately walk into the densest smoke or foulest air. But you see the use of the smoke-jacket necessitates the presence of several men; it also implies the presence of an engine. A single man could make no use of it, nor indeed any number of men without a pumping engine. Its uses are thus summed up in a communication addressed to me by Captain Shaw:—

"This smoke-jacket is very useful for extinguishing fires in vaults, stopping conflagrations in the holds of ships, and penetrating wells, quarries, mines, cesspools, &c.—any places, in short, where the air has become unfit for respiration.

"The special advantages of this jacket are its great simplicity, its facility for use, and the rapidity with which it can be carried about and put on; but its drawback is that it requires the use of an engine or air-pump, and consequently is of no service to one man alone. For this latter reason smoke-jackets, although very effective for enabling us to get into convenient places for extinguishing fires, have very rarely proved of any avail for *saving life*."

Now it is that very want that I thought ought to be supplied by a suitable respirator. Our fire-escapes are each in charge of a single man, and I wished to be able to place it in the power of each of those men to penetrate through the densest smoke into the recesses of a house, and there to rescue those who would otherwise be suffocated or burnt. I thought that cotton wool, which so effectually arrested dust, might also be influential in arresting smoke. It was tried; but, though found soothing in certain gentle kinds of smoke, it was no match for the pungent fumes of a resinous fire, which we employ in our experiments in the laboratory, and which, I am gratified to learn from Captain Shaw, evolves the most abominable smoke with which he is acquainted. I cast about for an improvement, and in conversing on the subject with my friend Dr. Debus, he suggested the use of glycerine to moisten the wool, and render it more adhesive. In fact, this very substance had been employed by the most distinguished advocate of the doctrine of spontaneous generation, M. Pouchet, for the purpose of catching the atmospheric germs. He spread a film of glycerine on a plate of glass, urged air against the film, and examined the dust which stuck to it. The moistening of the cotton-wool with this substance was a decided improvement; still the respirator only enabled us to remain in dense smoke for three or four minutes, after which the irritation became unendurable. Reflection suggested that in combustion so imperfect as the production of dense smoke implies, there must be numerous hydrocarbons produced which, being in a state of vapour, would be very imperfectly arrested by the cotton wool. These in all probability were the cause of the residual irritation; and if these could be removed, a practically perfect respirator might possibly be obtained.

I state the reasoning exactly as it occurred to my mind. Its result will be anticipated by many present. All bodies possess the power of condensing in a greater or less degree gases and vapours upon their surfaces, and when the condensing body is very porous, or in a fine state of division, the force of condensation may produce very remarkable effects. Thus, a clean piece of platinum-foil placed in a mixture of oxygen and hydrogen so squeezes the gases together as to cause them to combine; and if the experiment be made with care, the heat of combina-

* Mr. Ladd, of Beak Street sells these respirators.

tion may raise the platinum to bright redness, so as to cause the remainder of the mixture to explode. The promptness of this action is greatly augmented by reducing the platinum to a state of fine division. A pellet of "spongy platinum," for instance, plunged into a mixture of oxygen and hydrogen, causes the gases to explode instantly. In virtue of its extreme porosity, a similar power is possessed by charcoal. It is not strong enough to cause the oxygen and hydrogen to combine like the spongy platinum, but it so squeezes the more condensible vapours together, and also acts with such condensing power upon the oxygen of the air, as to bring both within the combining distance, thus enabling the oxygen to attack and destroy the vapours in the pores of the charcoal. In this way, effluvia of all kinds may be virtually burnt up, and this is the principle of the excellent charcoal respirators invented by Dr. Stenhouse. Armed with one of these, you may go into the foulest-smelling places without having your nose offended. Some of you will remember Dr. Stenhouse lecturing in this room with a suspicious-looking vessel in front of the table. That vessel contained a decomposing cat. It was covered with a layer of charcoal, and nobody knew until told of it what the vessel contained.

I may be permitted in passing to give my testimony as

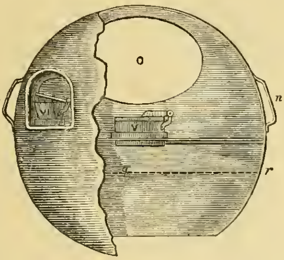


FIG. 1.

to the efficacy of these charcoal respirators in providing warm air for the lungs. Not only is the sensible heat of the breath in part absorbed by the charcoal, but the considerable amount of latent heat which accompanies the aqueous vapour from the lungs is rendered free by the condensation of the vapour in the pores of the charcoal. Each particle of charcoal is thus converted into an incipient ember, and warms the air as it passes inwards.

But while powerful to arrest vapours, the charcoal respirator is ineffectual as regards smoke. The particles get freely through the respirator. In a series of them tested downstairs, from half a minute to a minute was the limit of endurance. This might be exceeded by Faraday's method of emptying the lungs completely, and then filling them before going into a smoky atmosphere. In fact, each solid smoke particle is itself a bit of charcoal, and carries on it, and in it, its little load of irritating vapours. It is this, far more than the particles of carbon themselves, that produces the irritation. Hence two causes of offence are to be removed: the carbon particles which convey the irritant by adhesion and condensation, and the free vapour which accompanies the particles. The moistened cotton-wool I knew would arrest the first, fragments of charcoal I hoped would stop the second. In the first fireman's respirator, Mr. Carrick's arrangement of two valves, the one for inhalation, the other for exhalation, are preserved. But the portion of it which holds the filtering and absorbent substances is prolonged to a depth of four or five inches (see Fig. 1). On the partition of wire gauze *qr* at the bottom of the space which fronts the mouth, is placed a layer of cotton-wool, *c*, moistened with glycerine; then a thin layer of dry wool, *c'*; then a layer of charcoal fragments; a second thin layer of dry cotton wool, succeeded by a layer of fragments of caustic lime. The succession of the layers may be changed without injury to the action. A wire-gauze cover, shown in plan below Fig. 2, keeps the substances from falling out of the respirator. In the densest smoke that we have hitherto employed, the layer of lime has not been found necessary, nor is it shown in the figure; in a flaming building, indeed, the mixture ot

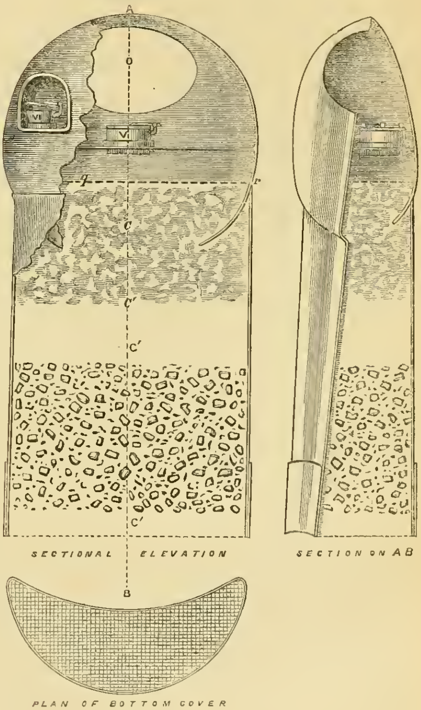


FIG. 2.

air with the smoke never permits the carbonic acid to become so dense as to be irrespirable. But in a place where the gas is present in undue quantity, the fragments of lime would materially mitigate its action.

In a small cellar-like chamber downstairs, with a stone flooring and stone walls, the first experiments were made. We placed there furnaces containing resinous pine-wood, lighted the wood, and placing over it a lid which prevented too brisk a circulation of the air, generated dense volumes of smoke. With our eyes protected by suitable glasses, my assistant and I have remained in this room for half an hour and more, when the smoke was so dense and pungent that a single inhalation through the undefended mouth would be perfectly unendurable; and we might have prolonged our stay for hours. Having thus far perfected the instrument, I wrote to Captain Shaw, the chief officer of the Metropolitan Fire Brigade, asking him whether such a respirator would be of use to him. His reply was prompt; it would be most valuable. He had, however, made himself acquainted with every contrivance of the kind in this and other countries, and had found none of them of any practical use. He offered to come and test it here, or to place a room at my disposal in the City. At my request he came here, accompanied by three of his men. Our small room was filled with smoke to their entire satisfaction. The three men went successively into it, and remained there as long as Captain Shaw wished them. On coming out they said that they had not suffered the slightest inconvenience; that they could have remained all day in the smoke. Captain Shaw then tested the instrument with the same result. From that hour the greatest interest has been taken in the perfecting of the instrument by Captain Shaw himself. He has attached to the respirator suitable hoods. The real problem is practically solved, and I can only say that if a tithe of the zeal, intelligence, and practical skill were bestowed on the cotton-wool respirator that Captain Shaw has devoted to the fireman's respirator the sufferings of many a precious life might be spared, and its length augmented.*

The lecture was concluded as follows:—"Thus have we been led from the actinic decomposition of vapours through the tails of comets and the blue of the sky to the dust of London, from the germ theory of disease down to this fireman's respirator. Instead of this trivial example, I could, if time permitted, point to others of a more considerable kind in illustration of the tendency of pure science to lead to practical applications. Indeed those very wanderings of the scientific intellect which at first sight appear utterly unpractical, become in the end the wellsprings of practice. Yet I believe there is a philosophy embraced by some of our more ardent thinkers (who I fear on many points commit the well-intentioned, but fatal mistake of putting their own hopeful fancies in the place of fact) that would abolish these wanderings of the intellect and fix it from the outset on practical ends alone. I do not think that that philosophy will ever make itself good in the world, or that any freedom-loving student of nature could or would tolerate its chains."

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A short time before the lecture I had an opportunity of inspecting the apparatus of Mr. Sinclair, which has been tested and highly spoken of by the superintendent of the Manchester Fire Brigade. The original idea is due to Von Humboldt, who proposed it for the Hartz mines. Galibert constructed the apparatus in an improved form, and it has been still further improved by Mr. Sinclair, who has purchased Galibert's patent. It consists of an air-tight bag, from which issue two tubes that unite on a single one with a respirator mouth-piece. The bag is filled with air, and the wearer inspires through one valve and expires through another. The expired breath is carried to the bottom of the bag, and is stated to remain there in consequence of the chilling experienced in its passage downwards. A bag of not inordinate size is stated to be sufficient to supply a man with air for twenty minutes. Mr. Sinclair's apparatus was exhibited during the lecture.

J. T.

* Mr. Ladd has also proposed a form of mouth-piece which promises well, and Mr. Cottrell has attached to it an ordinary fencing-mask. This will probably be the form of apparatus finally adopted.

## NOTES

We are able to state that the Council of the Royal Astronomical Society are considering the steps necessary to be taken to insure observations being made of the Total Solar Eclipse visible in Ceylon next December. We need scarcely remark that there is no subject which is at present engaging the attention of scientific men more important than that of the nature of the Corona, and it will be a disgrace to the science of the age if the next eclipse is allowed to pass over without every effort being made to increase our knowledge.

The Astronomer Royal requests us to state that he will be obliged for the loan of any unpublished observations made during the recent total eclipse. Communications to be addressed to him at the Royal Observatory, Greenwich.

We are glad to learn that the Right Hon. Mr. Robert Lowe has been elected a Fellow of the Royal Society. We have before in these columns stated our belief that Science has every reason to expect a favourable recognition of her claims from him as Chancellor of the Exchequer if proper claims are put forward in a proper manner, and we reiterate the assertion. It was unfortunate that the first grant of public money for which Mr. Lowe was asked, for scientific purposes, was allocated in a way which made Mr. Lowe somewhat indignant, a feeling which was however shared by many men of science. It was also unfortunate that the requirements of science in the matter of the Eclipse Expedition were not properly put before the Government in the first instance, but it is now a matter of history that Mr. Lowe was satisfied with a semi-official statement of the claims of Astronomy, and not only at once granted the required aid, but threw all the power of the Government into completing the necessary arrangements. The same may be said of the Dredging Expeditions. The willingness of a Chancellor of the Exchequer after all, however, is not the only thing requisite for State recognition of the claims of Science. We want a proper scientific organisation, and proper scientific representation. That Science here is in a chaotic state, is the well-founded opinion of many of our scientific men; and if this condition of things is allowed to continue, students of Science must expect that their wishes shall be ignored or lost sight of in the rush of other more emphatically asserted claims.

The official statements made under the head of "University Intelligence" in the daily papers have been lately very remarkable. We noticed, not very long ago, that Prof. Max Müller was called Professor of Comparative Physiology! What will Dr. Rolleston say to this? A few days afterwards it was announced that Mr. Reinold, the Lee's Reader of Physics at Christ Church, would give a course of lectures on Statistical Electricity!! Surely no one but Mrs. Malaprop herself could have made such blunders; while to cap all, a day or two ago we were informed (again in the official "University Intelligence") that the Commemoration at Oxford was an "interesting event!" Surely this is rather hard on Alma Mater!

Astronomy, may we say astrology, like many other things, is being put on a new footing at Constantinople. For many years the chief functionary in this department of science has been the Sultan's chief astrologer, but we believe he is now little called upon by Abdul Aziz to cast horoscopes for a lucky time, as the Sultan starts at a punctual hour, and the astrologer has chiefly to cast the ephemerides for the Salnameh, or official almanack, a periodical growing in respectability. Lectures in Physical Science are given in Turkish by Mussulman Professors at the Darul Funoun, or University, though there are godly men in Islam who maintain that such teaching is contrary to scripture. The time in Constantinople is a sore puzzle. As the day begins at sunset, and has to be divided into twenty-four hours, at sundown begins a general setting of watches, because steamboat departures and other incidents are regulated by Turkish time. The chief object for which expensive clocks and watches are bought by the Turks is for working out the canonical hours of prayer. Now a reform is setting in, H. E. Halil Pasha, Director-General of Arsenals, brought up at Woolwich, the Rev. Mr. Gribble, F.R.A.S., H.M. Chaplain, and Mr. Coumbary, Director of the Meteorological Observatory, have set up a noon-gun at the Tophaneh Arsenal.

WE regret to have to record the death of M. Payen, Professor of Chemistry at the Conservatoire des Arts et Métiers. Although seventy-six years of age, he had enjoyed very good health, and died suddenly of apoplexy. He had been a member of the Academy for twenty-nine years. An address was delivered at the grave by M. Decaisne, and another in the Academy by M. Chevreul.

DURING the last siege of Paris many learned Frenchmen experienced heavy losses through the firing of private houses. M. Bertrand lost the manuscript of the third volume of his great Treatise "On the Differential and Integral Calculus." The amount of labour required for writing it a second time is immense. M. Bertrand had calculated many formulæ from new methods of his own invention. Many instances of similar misfortunes could be given, but altogether it is astonishing that the amount of mischief done was not even larger. During the whole of the rebellion the Academy, with only one exception, held its meetings regularly. No fact more honourable for the members can be written in the eventful history of the learned society.

WE congratulate Marlborough College on the determination of the head master, the Rev. F. W. Farrar, to introduce an adequate amount of natural science as a part of the school work; as well as on the selection he has made of Mr. G. Farrer Rodwell as Science Master, by whose appointment to Marlboro' a vacancy is occasioned in the post of science master at Clifton College.

THE Examiners for the Johnson Memorial Prize Essay at Oxford have awarded it to Mr. James S. Gamble, B.A., Magdalen College. *Proxime accesserunt:* Mr. J. P. Earwaker, Merton College; and the Hon. F. A. Russell, Christ Church. This prize is only awarded every four years, the subject this year being—"On the Laws of Wind; (1) with regard to Storms, (2) with regard to average Periodical Phenomena at given places on the Earth's Surface."

MESSRS. H. N. READ, SOLLAS, and YULE have been elected to exhibitions of 10*l*. each for merit shown in the annual college examination in natural science at St. John's College, Cambridge, in addition to the usual prizes in books conferred upon all of the first class in that examination.

HERBERT MOGG, from Clifton College, Bristol, and S. Cook, private tuition, have been elected to scholarships for Natural Science at Pembroke College, Cambridge.

A NATURAL SCIENCE DEMYSHIP, of the annual value of 80*l*. for five years, will be given in October next at Magdalen College, Oxford. The limit of age is twenty years, and a sufficient knowledge of classics to pass the College Matriculation Examination is also required. At the same time, and with the same papers, a Natural Science Postmastership, of the value of 80*l*. for five years, will be awarded at Merton. Each candidate will be considered as standing in the first instance at the college at which he has put down his name, and unless he has then given notice to the contrary will be regarded as standing at the other College also. The Examination will commence on October 3.

PROF. LIVEING gives notice that the chemical laboratory of the University of Cambridge will be open during the long vacation, as in Term time. The demonstrator (Mr. Liversedge) will attend to give assistance to students on mornings and afternoons alternately during July and August. Dr. Michael Foster also gives notice that the physiological laboratory will be open, and that, under his superintendence, Mr. Martin will give a course of instruction in practical physiology.

ON Wednesday, the 7th inst., a grace passed the Senate of the University of Cambridge, granting 200*l*. from the Worts' Fund to Mr. G. R. Crotch, who is about to proceed in the autumn to the United States of America, *en route* for the Sandwich Islands, New Zealand, and Australia, for the purpose of enabling him to collect and transmit to Cambridge specimens illustrative of the natural history of those parts of the world, and of any others he may visit, with the understanding that such specimens shall be the property of the University, and be accompanied by reports from time to time, which may hereafter be published, of the investigations he has made and the objects he has collected. Though Australia will be the basis of Mr. Crotch's operations, he does not intend to confine his researches to that continent, and should the opportunity offer itself of visiting from its northern settlements the Island of New Guinea, it is to be hoped he will be able to avail himself of it.

THE following have been elected honorary members of the Cambridge Philosophical Society:—Sir B. C. Brodie, Dr. W. B. Carpenter, Capt. Clarke, R.E., Prof. Huxley, Prof. Bartholomew Price, Mr. W. Spottiswoode, Prof. Argelander (Bonn), Prof. Clebsch (Göttingen), Prof. Des Cloiseaux (Paris), Prof. Helmholtz (Berlin), Prof. Wöhler (Göttingen).

IN the *Times* of last Monday Mr. F. Galton calls attention to the small extent to which the great public schools have availed themselves of the opportunity of competing for the medals offered by the Royal Geographical Society for the best candidates in physical and in political geography, and to the very unprepared state in which those candidates who do go up present themselves. He believes it is the Universities that stop the way of reform, and suggests that one branch of geography be included as a necessary subject in a matriculation examination before admission to the Universities.

ON Trinity Monday, according to ancient custom, the Provost and Senior Fellows of Trinity College, Dublin, proceeded to the election of a fellow and of fifteen scholars. Eight candidates had presented themselves for the Fellowship Examination. Mr. William F. Burnside was declared elected as fellow, Mr. G. S. Minchin obtained the Madden Premium; Mr. F. Purser was only half a mark below Mr. Minchin. The following were elected classical scholars:—Tyrrell, Hewitt, Wilson, Barry, Hill, Baker, H. W. White, Butler, and Roberts; and as science scholars:—Graham, Fitzgerald, Downing, Hackett, Byrne, and Adair.

AT the annual meeting of the Royal College of Surgeons of Ireland, held on Trinity Monday, Dr. J. H. Wharton was elected President, Dr. F. Kirkpatrick Vice-President, Mr. William Coles, Secretary; and on the Council:—Messrs. R. Adams, W. Colles, A. Carte, M.D., J. Denham, W. A. Elliott, A. H. Jacob, G. W. Hatchell, E. Hamilton, M.D., W. Hargrave, H. Irvine, W. Jameson, H. Labatt, E. Ledwich, R. Macnamara, J. Morgan, M.D., R. M'Donnell, M.D., F.R S., B. M'Dowel, M.D., G. H. Porter, M.D., and A. J. Walsh. There was a severe contest for the Vice-Presidentship, in which Dr. Kirkpatrick defeated Dr. Mapother by a few votes.

THE *Gardener's Chronicle* publishes more recent intelligence of Dr. Hooker and his party under date "Camp, Atlas Mountains, May 19th." They had succeeded in reaching the top of the crest of the Atlas, nearly due south of Morocco, at an altitude of about 11,500 feet, the upper 3,000 feet of which were very steep indeed, very rocky and stony, with a good deal of snow, and the temperature 24° Fahr. The flora of this upper region appears to be excessively poor; they did not find a single really Alpine plant, "and few plants of any kind; no gentians, primroses, anemones, ranunculi, or other types of an Alpine flora. The rocks were chiefly a very hard porphyry, red, black, and grey, with granite here and there, and beds of limestone, all hard and obnoxious to plants. Moreover, these steep upper cliffs of the Atlas are alternately roasted by a blazing sun, or parched by a Sahara sirocco, or swept by moist north-west Atlantic gales, which bring heavy snowstorms such as we experienced, probably throughout the year. The flora up to 7,000 feet, on the contrary, is exceedingly rich, varied, and beautiful;" and Dr. Hooker thinks that their collections will prove of very great interest and considerable extent. Many English plants find their southern limits here, and there is an abundance of roses, bramble, elder, honeysuckle, ivy, ash, poplar, &c.

A French physician, Dr. Brierre de Boismont, has discovered a new disease, a form of contagious mental alienation, from which, he states, the members of the Commune suffered during the late insurrection in Paris, and which he terms the *morbus democraticus*.

We have received during the past week *Les Mondes* for March 23, and the *Moniteur Scientifique* for May 1 and 15, together with the *Comptes Rendus* to May 29, we trust an earnest of the re-opening of the scientific intercourse with Paris which has for long been so lamentably interrupted.

We learn that the celebrated collection of Egyptian antiquities made by the late artist antiquary, Robert Hay, of Linplum, recently exhibited at the Crystal Palace, has been purchased by a well-known banker in Boston, U.S.A., and it is now being shipped for that city. We regret that so valuable a collection should be allowed to leave this country, and congratulate America on the acquisition of so important and in many instances unique an addition to its antiquities.

On Saturday, June 10, the boys belonging to the Geology Class at Christ's Hospital went to some pits at Woolwich and Charlton, under the care of Professor Tennant. Unfortunately the rain detained them for an hour, but, notwithstanding the inconveniences caused by the wet, some interesting specimens were discovered, and a somewhat better idea of what geology really is was obtained from the practical work that they did.

In a note in a recent number of the *American Naturalist*, Prof. Dawson repeats his assertion that his *Prototaxites Logani* of the Erian formation of Canada is a true exogenous tree, with bark, rings of growth, medullary rays, and well-developed, though peculiar woody tissue; and not, as Mr. Carruthers has maintained, a gigantic alga.

Mr. F. G. Sanborn has been appointed instructor in Practical Entomology in the Bussey Agricultural Institution of Harvard University.

Congress recently appropriated 40,000 dols. for the annual expenses of the U.S. Geologist, Dr. F. V. Hayden, who is surveying the Rocky Mountain region of Colorado.

Prof. Henry, of the Smithsonian Institution, has received the decoration of "Commander of the Order of St. Olaf," from Charles, King of Sweden and Norway. A joint resolution has been offered in Congress to enable the Professor to accept the honour.

Dr. Wills de Hass is about to start on an extended visit of exploration in the Valley of the Mississippi, with a view of determining the character of certain ancient works in the vicinity of St. Louis.

At a meeting of the New York Lyceum, held on May 15, the president, Prof. Newberry, of Columbia College, gave a summary of what was being done in the line of geological exploration throughout America—a task he was well qualified to perform, from his intimate acquaintance with the subject, and from his own connection with one of the most important of these enterprises. He congratulated the Lyceum upon the prospect that what he called the Chinese puzzle of New England geology is in a fair way to be worked out during the present season by means of the concurrent labours of several eminent geologists. Among these he mentioned Sir William Logan, late director of the Geological Survey of Canada; Prof. C. H. Hitchcock, the State Geologist of Vermont; and Profs. Shaler and Hyatt, of Eastern Massachusetts. Prof. Dana also proposes to carry a geological section from the valley of the Connecticut to that of the Hudson. The following additional items were mentioned in the communication of Prof. Newberry: the geological survey of Canada will be continued under the direction of Mr. A. R. C. Selwyn, who succeeded Sir William Logan in its chief direction.

We learn from *Harper's Weekly* that Prof. Winchell has resigned the directorship of the geological survey of Michigan; but the work will be carried on by the board of trustees, Major Brooks devoting himself to the iron region. Prof. Pumpelly declines to continue the survey of the copper district, and asks that the small appropriation may be turned over to Major Brooks. The current surveys of Indiana, Illinois, and Missouri are to be continued during the year, and a new survey has been authorised in Arkansas. No positive appointment of director for this work has yet been made, although the place has been offered to Prof. Orton, of the Ohio geological corps, and declined by him. Bills providing for surveys of Pennsylvania and West Virginia were introduced into the Legislatures of those States during the past winter, but failed to become laws.

A laboratory for the use of students in practical inorganic chemistry having been opened last September at the Oldham School of Science and Art, twenty-two *artisan* students presented themselves for examination by the Department on the 4th of May, and fourteen of them have passed.

## ON THE NECESSITY FOR A PERMANENT COMMISSION ON STATE SCIENTIFIC QUESTIONS*

THE duty of the Government with respect to Science is one of the questions of the day. No question of equal importance has perhaps been more carelessly considered and more heedlessly postponed than this. And now that a hearing has been obtained for it, neither the governing class nor the masses are qualified to discuss it intelligently. The governing class, because it is for the most part composed of men in whose education, as even the highest education was conducted thirty to fifty years ago, science occupied an insignificant place; and the masses, because they may be taken to be virtually destitute of scientific knowledge. Those who wield, and those who confer, the powers of government being alike incapable of dealing with this question, it devolves on another section of the community to urge its claims to attention.

The section qualified to do this is composed of scientific men, properly so called, of professional men, such as engineers, and certain manufacturers who are engaged in applying science practically, and of a limited number of officers in the naval and military services. This section is without much political influence, but its intellectual power is enormous, and this power has never been so strongly exerted, or so decidedly acknowledged, as at the present time.

A tangible acknowledgment of the claims of science consists in the recent appointment of a Royal Commission "on Scientific Instruction and the Advancement of Science," which is now sitting. The problem which this Commission is expected to solve is one of very great complexity, delicacy, and difficulty. It has to survey the whole world of scientific thought, and to construct a chart on which the districts that it is the duty of the State to occupy shall be clearly delineated, with boundary lines so drawn as not to trench upon tracts which may be best left to individual or corporate management. It has then to devise a form of government of which not a trace at present exists, fitted

* Abstract of a paper read at the Royal United Service Institution, by Lieut.-Colonel Strange, F.R.S.

to administer the affairs of the newly-acquired territories. Instruction in science is one thing, and, I admit, an indispensable thing, without which there can be no foundation for future scientific progress; scientific investigation is another and perfectly distinct thing, constituting the end to which instruction is the means. Each may be pursued separately and solely. But if instruction be scanty, investigation will be unsound; if investigation is neglected, progress must be impossible. It will be for the Royal Commission now sitting to point out the relation of instruction to investigation, and to decide how far and by what agency the Government may beneficially aid each.

The first report of the Royal Commission has been published. It deals with certain limited matters of detail only, relating to the occupation of some new buildings at South Kensington. Possibly the settlement of these details may have claimed immediate attention with reference to the arrangement of the buildings in question. This first report has therefore not touched the great problems above adverted to, which await the deliberation of the Commission, and an authoritative solution of which at its hands is anxiously expected by the scientific world.

It may be asked why, as a Royal Commission is investigating the relation of Science to the State, the subject of the present paper should be brought forward independently of that body. The reply is, first, that discussions of any of the questions on which the Royal Commission is deliberating can hardly fail to afford light and assistance useful to the inquiry; secondly, that the problem submitted to the Royal Commission is, "How should the State aid Science?" whereas the question on which I am to address you is totally different, namely, "How can Science aid the State?" Although this latter question may be considered by the Royal Commission, it is certainly not necessarily a part of their programme, and as it is a question of at least equal importance with the former one, it is most undesirable that it should be overlooked.

To the question, "How can Science aid the State?" I reply, "By means of a permanent scientific commission or council, constituted for the purpose of advising the Government on all State scientific questions."

In order to apprehend the aim of this proposal, its practical operation and probable results, it must be examined systematically and in detail. I propose to do this under the following heads:—

I. The scope implied by the term State scientific questions, and the importance of those questions.

II. How are such questions at present dealt with, and with what results?

III. What should be the constitution and functions of the proposed Council of Science?

IV. What objections can be alleged against the proposed council? . . . .

[On the second point we quote the following:—]

II. *How are such questions at present dealt with, and with what results?*—I wish to preface my remarks on this head by saying that they are not intended to apply to any particular party in politics. In speaking of the shortcomings of the Government, I mean to include ALL administrations, whatever political principles they may have represented. I cannot perceive any difference worth noting between different Cabinets as regards science. All have, in my opinion, displayed, in the most elaborate manner, their incapacity to grasp science as a national matter. I am not aware of a single attempt on the part of any Government that has ever existed in England to define its duties with regard to science, or to model any administrative agency for dealing with it in a rational, efficient, and comprehensive manner. It would be invidious and unjust to single out any set of Ministers as having been more negligent in this matter than others, where all have been to all intents and purposes equally indifferent to it.

How then are State scientific questions now dealt with? The answer is, desultorily, capriciously, inefficiently, irresponsibly, when they are dealt with at all, but in many instances of the greatest moment they are absolutely neglected. The number of questions involving science on which Government has to decide, are innumerable and never ending. Every day adds to their number and their urgency. This vast increase of such questions has taken place within a period which, in the life of a nation, is very brief. . . . . Our official scientific arrangements are substantially the same now as they were in the pre-scientific era —they may be more extensive in degree, but they are the same in kind—the butter may be spread further, but there is not more butter. The enormous scientific activity of the last 30 or 40 years does not seem to have struck the official world as a fact having a bearing on the humdrum routine of the Departments—more secretaries—more clerks—more subordinates of various kinds have been appointed to prevent accumulation of arrears; more committees of inquiry have sat, more scientific witnesses have been examined, more reports published, if not read. But not a single step has been taken towards the creation of an *organisation* capable of concentrating and directing all this scattered effort.

The example of foreign nations, the pressure of the public, and the demands of inventors, daily set before the Government scientific puzzles, which they are often, if not generally, at their wits' end to solve. It never seems to occur to them that these puzzles will never cease, and that they will increase in difficulty as a matter of absolute certainty. The attempt is made to stave off by temporary expedients work of a permanent character. The puzzles are guessed at, and the guess is oftener wrong than right. Problems too deep for guessing are either pushed out of sight or submitted to methods of investigation that end in a blunder, perhaps a catastrophe.

I do not wish either to declaim or to exaggerate. I will briefly indicate the provision that does exist for the solution of State scientific questions. It is of three principal kinds. First, official subordinates in various departments. Second, temporary and special Committees. Third, consultation with individuals eminent in science, or with scientific bodies. I omit debates in Parliament, because no scientific question ever was or will be solved by such an assembly, and I omit also the press, which is so influential in other respects, as altogether unreliable for such inquiries.

The objections to the first kind of provision, viz., official subordinates, are, that such persons have almost invariably other duties of an executive nature to perform, and have not therefore the leisure necessary for scientific investigation. Science, moreover, is now in a stage in which scarcely any one problem can be adequately grasped by a single mind; this remark particularly applies to State scientific problems, which are invariably of a mixed order, requiring a great variety of attainments for their perfect comprehension. Lastly, subordinates are disqualified for the office of advisers by the very fact that they are subordinates. No inferior can be expected to urge distasteful counsels on a powerful superior, and no superior can be expected to abandon his own preconceived ideas in consequence of the timid and feeble remonstrances of an inferior under his orders. Subordinates then are unfitted to be counsellors, because they must in the majority of cases be deficient in leisure, attainments, and independence.

One clear, decided example of the inadequacy of this source of scientific advice is as good as a thousand. [The loss of the *Captain* was then referred to.]

The second expedient—temporary Committees, has been very largely employed for the purpose of guiding the Government through their scientific difficulties. There are very serious objections to this expedient. First, there seems to be no rule, either for their appointment or for their composition. If the Government is much pressed by public opinion (which on such subjects is not over-well informed), or if it sees a difficulty ahead, which, however, it often fails to do, a Committee is the result. But there is no guarantee for the proper composition of the Committee. There always lurks about some of the names a suspicion either of incompetence, or of leaning towards the supposed foregone conclusion of the Government. But, passing by such suspicions, there remains the fact, that the members are selected either by some Minister who, not being a scientific man, probably knows nothing about the qualifications necessary for conducting the proposed inquiry, or by some outside and irresponsible person to whom the Minister has applied for help. It is quite overlooked that the selection of the proper persons for conducting any given inquiry can only be made by some one having a knowledge of the subject of the inquiry, or of subjects cognate thereto; the selection is in itself a scientific question. Though some temporary Committees have done good service, it may be safely declared that on the whole they have failed to give reasonable satisfaction.

A second objection to such Committees consists in the fact itself that they are temporary. As such they necessarily commence their labours, however well they may have been selected, with but a partial and confused knowledge of the question at issue, and much time is lost in gaining some insight into it. After much work and expense, they reach a certain stage in the inquiry at which a report is possible. Perhaps by that time the public pressure, or other cause that led to their appointment, has died out, or action is necessary,—in either case the Committee is considered to have served its purpose, and is broken up; the members disperse, take up other duties, the knowledge of a particular subject which they gained in the course of their inquiry is lost to the country, and a scientific problem is left half solved, until at some future day it must be taken up again for completion, and the old work gone over *de novo*. The system of temporary Committees, in fact, implies a belief that finality is attainable in those mixed scientific problems in which chiefly the State is interested, or that such problems can advantageously be taken to bits and studied piecemeal; whilst the fact is, that no one such problem that can be mentioned ever has, or ever will, as long as human ingenuity survives, come to an end. Permanent arrangements alone can deal with the unbroken continuity and unceasing change of scientific development.

A third objection to such Committees consists in the fact that much of the investigation carried on by several Committees may be common to each. This involves the repetition of the same work, and great consequent waste of time, effort, and money. . . . .

I come now to the third source from which the Government draws its scientific inspiration, namely, individuals eminent in science, and scientific bodies. Recourse is had to such sources without any system whatever; there exists no rule, for instance, defining what cases should be submitted to an individual, what cases to a scientific society, and what cases to a temporary Committee. Nor is it possible to assess the degree of responsibility attaching to an individual or to a scientific society advising the Government. If the advice so obtained is rejected, nothing about it is known publicly; if it is adopted and turns out unsound, the right to blame the adviser is absent. It is impossible to ascertain when such consultations have occurred, and with what results. The probability is that they are not frequent. During the two years that I served on the Council of the Royal Society, I only remember one application from Government for advice. It was on some point connected with coppering ships. A committee was formed of the most competent persons, and probably very sound counsel was afforded. But it is evident that this is an expedient that cannot be frequently employed, as it would occupy too much of the time of the Society, which should be devoted to its legitimate objects. Advising the Government is certainly not one of these, nor should the Government of a great, powerful, and opulent nation like England be reduced to such makeshifts as private societies for direction in matters of such tremendous national moment.

Having shown, I trust, that Government is without recognised scientific advisers, I proceed to discuss:

III. *What should be the constitution and functions of the proposed Council of Science?*—The ground requires to be cleared before approaching this question. I have heard it urged that the various departments of the State should be complete in themselves, each with its own consultative element, as distinct from its executive. This appears at first sight a plausible arrangement, but it will not bear examination. Many of the scientific inquiries that devolve on the Government affect several departments, and in such cases it would be wasteful to have numerous repetitions of the same investigation when one would do; and if, under the supposed arrangement, one investigation of a given class of subjects was decided on, the selection of the particular department to which it should be referred would cause endless bickerings and jealousies,—the co-operation of departments being, like universal peace, a somewhat remote hope. Again several departments would require identical scientific advisers. . . . .

For these reasons I discard this suggestion, and revert to the proposal which forms the subject of this paper, namely, that there should be one permanent great council for advising and assisting the Government on all State scientific questions. This council should be purely consultative, not executive. All departments should equally be entitled to its assistance. The Council should not be expected to initiate questions, though it might occasionally see fit to propose certain investigations to the Government, without whose sanction, however, they should not be undertaken. The Government should not be bound on all occasions of scientific difficulty, either to resort to, or be guided by, the opinion of the Council; but it would of course become in either case absolutely responsible for all consequences. . . . .

The duties that would devolve on this Council, stated broadly, would be—

1st. To advise the Government on all questions arising in the ordinary routine of administration, submitted to it by the various departments.

2nd. To advise the Government on special questions, such as the founding of new scientific institutions, and the modification or abolition of old ones; the sanctioning of scientific expeditions, and applications for grants for scientific purposes.

3rd. To receive, consider, and decide upon inventions tendered to Government for the use of the State.

4th. To conduct or superintend the experiments necessary to enable it to perform the above duties.

As to the first branch of its duties little need be said. The number and variety of questions involving scientific considerations entering into the current work of the different departments are almost unlimited. A large proportion of them could be answered at once by competent persons, but there would remain many that require investigation, discussion, and often experiment.

The second branch, special questions, would not perhaps be so extensive, but it would be exceedingly important. At present there exists literally no provision for dealing with such questions. Sometimes one person, supposed to have a knowledge of the matter at issue, sometimes another is consulted, sometimes no one. At present the Royal Commission now sitting is probably dealing with the subject of existing and required scientific institutions. But supposing this body settles all such matters in the most satisfactory manner at the present time, a reconsideration of them will very soon be demanded by the rapid advance of science, and the perpetually changing relations of different lines and modes of physical inquiry. But the Royal Commission is a temporary body. Its functions will sooner or later cease, whilst the mutations and permutations of scientific thought are incessant. Questions relating to State scientific institutions require ceaseless watching,—never-ending modification. A permanent body, such as I propose, alone can preserve the national scientific establishments in a condition of vigorous efficiency on a level with the existing state of physical knowledge.

The sanctioning of special scientific researches and expeditions will be a very important duty, which there is at present no one qualified to perform. . . . .

Sanctioning of grants of money for aiding scientific objects comes under the same head as sanctioning expeditions. At present £1,000 per annum is granted by Government for such purposes, and it is distributed by the Government Grant Committee of the Royal Society. As a member of this committee I can testify to the extreme care, fidelity, and impartiality with which it performs this gratuitous duty, The amount of the grant might with advantage be much increased, as at present only small sums can be given out of it to each applicant; these are often quite insufficient, and as they must unavoidably be small, no application for aiding extensive and costly researches can expect efficient aid from so narrow a source. The proposed council would be a public body, precisely qualified to perform the duty now imposed on private individuals.

The third branch of duties devolving naturally on the council would be the dealing with inventions tending for the use of the State. . . . .

The fourth class of duties which the Council would have to perform would relate to the experiments and investigations necessary to enable it to perform the duties previously enumerated. Regarding the necessity for providing the Council with the agency, appliances, accommodation, and funds requisite for these purposes, there can hardly be two opinions. They are absolutely indispensable. I need not here attempt to define what would be wanted. Such details would follow naturally the affirmation of the great principle involved in the creation of the Council.

I come now to a question on which opinions may differ—namely, the question whether the Council should be a paid or an unpaid body. I say, unhesitatingly, that it should be handsomely paid. If the heads of duties to be performed, of which I have given but an outline, be duly considered, it will be seen that they will be laborious, responsible, and beneficial in the highest degree; and that they can only be adequately performed by highly qualified persons. It is idle to expect that such men as will be necessary, will devote themselves almost exclusively, as they will have to do, to such labour from pure love of science and of their fellow-creatures. The delights of philosophical speculation are one thing, carrying with them their own reward—a reward beyond any money consideration; downright official routine work is quite another thing. In no other professional field is it unpaid; nor is it ever worth much if not paid for. It has hitherto been too much the custom to treat men of science as exceptions to all other professions; to assume that whilst it is quite proper to enrich and ennoble soldiers who fight for pay, lawyers who evade or apply the law according to circumstances; physicians who kill or cure as seemeth best to them; and even divines, whose mission to save souls might be deemed a sufficient privilege: the man of science who contrives the arms with which the soldier wins his fortune and his coronet, who surrounds the lawyer, the physician, and the divine with the luxuries which their superior privileges enable them to command, should work for love, and die, as he too often does, in poverty.

If the Council, the creation of which I now advocate, does its duty, it will confer benefits untold on every member of the community, from highest to lowest; from the military and naval appliances necessary to protect our unequalled national wealth, down to the smallest and least regarded necessaries of our ordinary life, the influence of this Council will be felt; and is it either just or wise to expect such benefits for nothing? . . . .

[The author then gives some indication of the mode of constituting the Council.]

IV. *What objections can be alleged against the proposed Council?*—Difficulties innumerable can of course be conjured up in this as in every case of reform, but I have only heard three definite objections raised that seem to me to deserve any notice. They are:—

1st. That this is a system of centralisation, and therefore objectionable.

2nd. That it will be liable to jobbery.

3rd. That it will be too costly.

I will touch on each of these briefly.

As to centralisation, I admit the impeachment, but claim it as an advantage, not an evil. Those who are scared by centralisation forget that it constitutes the very basis of civilisation and of stable efficient government. In primitive savage life there is no centralisation, no united effort for a common purpose. Each individual struggles single-handed for his rights. Civilisation teaches us to set apart certain members of the community for purposes beneficial to the whole, to form them into distinct bodies, having definite duties to be executed, under the direction of a head central authority. The army, the navy, the police, the post-office, are examples of such bodies, the animating and ruling law of which is centralisation. In the case of the police, we have local, in the other cases imperial, centralisation. The body we are considering will have to perform duties of a strictly imperial character, contributing directly to the efficiency of the defensive power of the empire, and to the security and well-being of every member of the community. It is a body which not only would not be effective, but which could not exist but in a centralised form.

As to the second objection, that the arrangement I have proposed would be liable to jobbery, I must own that, as I contemplate the employment of human beings only, I do certainly expect to see the operation of human motives. But if jobbery be a fatal objection to the scheme, then on the same principle we ought to have no army, navy, church, bench, magistracy, municipalities, or Parliament, for in each of these the discovery of some traces of jobbery will probably reward a diligent scrutiny. It is not apparent why a degree of purity not dreamt of in regard to any other profession should be insisted on when science is in question; nor is it clear why men of science should, *a priori*, be deemed more corrupt than their neighbours. Of course every precaution should be taken against corruption in so important a body, and the rest must be left to that sense of honour to be found in all other professions, and of which even men of science are perhaps not entirely devoid.

The third objection, undue costliness, is, in my opinion, as invalid as the other two. My proposal has two main objects—to increase efficiency, and to diminish blunders. Both are in the strictest sense economical objects. If it does not seem calculated to attain these objects, it should on no account be adopted. If it gives satisfactory promise of their attainment, no expenditure that it is likely to occasion will be too great in order to secure them. Let any one who is terrified by the cost, visit our ports, dockyards, and arsenals, and there see the ships that have been built which should not have been built, the cannons made that should never have existed, and the useless arms and equipments of the pre-scientific ages. Let him count the cost of these, and compare it with the probable cost of substituting for the reign of haphazard ignorance a reign of systematic intelligence. To take one example—that of Her Majesty's ship *Captain*. This vessel, with her armament and stores, probably cost the nation three or four hundred thousand pounds. Who shall assess in money the value of the 500 noble lives that perished with her? Would not the nation willingly give a million to have them back? If so, we have as the cost of one single blunder committed by one Department something like a million and a half of money, a sum that would go a long way to permanently endow a body which, had it existed a year ago, must have prevented that blunder. But if I dwelt on the preservation, prolongation, and increased comfort of civil life which such a Council would tend materially to secure, the cost of its maintenance would appear absolutely insignificant in comparison with the blessings it would shower on the nation. Against the cry of costliness I oppose the assertion, easily established, that nothing is so ruinous as disregard of the laws of nature, and nothing so profitable as intelligent obedience to them. Science, looked at in the dryest commercial spirit, must, in the long run, *pay*.

I must guard myself against the supposition that the proposal I have here advocated comprises all that is necessary for the efficient administration of scientific State affairs. It is only one part of a great system that has to be created. Other parts of the system will, no doubt, receive due attention from the Royal Commission now considering them. But there is one part so important that I feel called on to name it; I mean the appointment of a Minister of Science. He need not necessarily be exclusively devoted to science; he might, perhaps, with advantage, have charge of education and the fine arts also; but some one in Parliament directly representing the scientific branches of the national services has become absolutely indispensable. . . .

When we have all Scientific National Institutions under one Minister of State, advised by a permanent, independent, and highly qualified consultative body—when we have a similar body to advise the Ministers of War and Marine in strategical science—then the fact that, in accordance with our marvellous constitution, these Ministers must almost necessarily be men without pretension to a knowledge of the affairs which they administer, need cause us no alarm. When these combinations have been, as they assuredly will be, sooner or later, effected, the wealth, resources, and intelligence of the nation, having due scope, will render us unapproachable in the arts of peace, and unconquerable in war—but not till then.

In conclusion, I must claim for the proposal I have advocated that there is nothing revolutionary in its character.

I aim at creating no new principle. We have already, as an integral part of our administration, a body constituted on the very same principle as that now advocated. I allude to the Council of India. . . .

My proposal, therefore, I maintain aims at the creation of no new principle,—but only at the extension of one already existing, and universally approved after long experience. Nor do I aim at creating new labours. The work of which I have been speaking is now being done, or suppposed to be done, and it is paid for heavily by the nation, but it is not well done. I propose to improve its quality by improving the agency to which it is assigned. I propose to substitute concentration for scattered effort, system for chance, organisation for disorder. I propose neither to exact from the Queen's advisers new duties, nor to fix upon them new responsibilities. The end and aim of my proposal is to lighten their labours and anxieties by putting into their hands better arms than those with which they now vainly strive to uphold the power and glory of the nation.

A. STRANGE

## *SCIENTIFIC SERIALS*

THE last part of the *Sitzungsberichte* of the Isis Natural History Society of Dresden contains the proceedings of the Society for the months of July, August, and September, 1870. In the section of Prehistoric Archæology, Dr. Mehwald described kitchen-middens on Zealand and Jutland, and on the Andaman Islands, and stated that M. Lorange of Fredrikshald, in Norway, has investigated a grave in that neighbourhood which he believes to have been a family grave, in which the bodies were deposited one above the other, the one first buried being probably at a depth of 600 to 700 feet.—Prof. Geinitz explained Delesse's Geological Map of the Department of the Seine, and remarked upon the occurrence in that district of the bones of extinct animals associated with artificial products and the remains of man. He also glanced at the well-known phenomena of the same kind at St. Acheul and Schussenried, and gave a list of articles received by the Museum of Dresden from the pile-buildings of Robenhausen, in the Pfaffikon Lake. Prof. Geinitz also noticed the contents of some recent anthropological publications.—M. Klemm exhibited a ring of serpentine, measuring about two inches in diameter, found in the year 1835, in an urn in Lower Lusatia. —In the mineralogical and geological section, M. C. Bley noticed the occurrence of roestone in the neighbourhood of Bemburg, and ascribed the peculiar structure of the stone to the great amount of salt contained in the water from which the carbonate of lime for its formation was precipitated.—Prof. Geinitz referred to the discovery of a well-preserved molar of *Elephas primigenius* in the bed of the Elbe below Kötschenbroda, and also exhibited a great number of marly concretions and transported blocks from the loam pits between Strehlen and Mockritz. He also noticed some of the localities in which fossils are to be found in the Loess. M. H. Engelhardt communicated notices of some plants from the brown coal of Saxony, namely, *Anona cacaoides* Zenk. sp., *Gardenia pomaria* Schl. sp. (=*G. Wetzleri* Heer), *Livistona Geinitzi* n. sp., *Glyptostrobus europæus* Brongn. sp., and a species of *Carpolithes*.—Prof. Geinitz communicated a list of some corals from the Lower Pläner of Plauen, which had been determined by Dr. W. Bölsche ; eleven species are enumerated of which six are indicated as new, namely, *Montlivaltia* (?) *tourtiensis*, *Thecosmilia* (?) *Geinitzi*, *Latimæandra*, *Fromenteli*, *Psammohelia granulata*, *Thamnastræa tenuissima*, *Dimorphastræa Dunkani*, and *Astrocœnia tourtiensis*.—M. Engelhardt communicated a paper on the Loess in Saxony, in which he described the general nature and mode of occurrence of the deposit, and the special peculiarities presented by it in particular localities. In connection with this paper and the concretions from the Loess exhibited by Prof. Geinitz at a previous meeting, M. Klemm presented a memoir on concretions and on the globular forms occurring in the minerals and rocks.—Dr. O. Schneider noticed the minerals occurring in the granite of the Königshayner mountains, and in the Zechstein of Niederludwigsdorf near Görlitz, and described some crystals of zircon received from Haddam in Connecticut.—Prof. Geinitz reported upon some fossils from a sandy deposit of Cretaceous age at Château de Meauene near Angers. The predominant form is *Siphonia pyriformis* Goldf. Three species of *Palmacites* are noticed, and one of them is described as a new species under the name of *P. Boxbergæ*— In the mathematical, physical, and chemical section, the only paper of which particulars are given is a description by Prof. Klein of an apparatus invented by him to enable the magnetic needle to be employed on board of armour-plated ships. The arrangement consists of a compass placed at the mast-head and connected with an electro-magnetic apparatus, by which an index is moved.—In the Zoological Section Prof. Günther gave a short exposition of the comparative anatomy of the brain in mammalia. —M. Engelhardt exhibited some corals and shells obtained from Guano.—Dr. Ebert remarked upon Huxley's *Bathybius*.—M. C. F. Seidel described the excrescences and other deformities produced on the stalk of the common cabbage by a small weevil, *Baris cuprirostris*.—Dr. Ebert referred to the support afforded to the theory of the evolution of organic types by the discovery of the curious lizard, *Hatteria punctata*, upon the anatomy of which Dr. Günther has given us such interesting information. Dr. Ebert tabulates the characters of the orders of reptiles to show in what a singular manner *Hatteria* combines their peculiarities.— Dr. Schneider noticed the scorpions collected by him in Egypt.— Dr. Mehwald noticed the occurrence of a snake (*Coronella lævis*) and of a lizard (*L. agilis ?*) as far north as 62° and 63° in Norway ; and M. Kirsch gave some account of experiments with vipers and the common snake. According to the latter the bite of a new-born viper, five inches long, killed a mouse in a short time ; snakes killed by decapitation exhibit irritability by galvanism for a very much longer time than those destroyed by poison ; and the common snake (*Tropidonotus natrix*) is the only snake indigenous to Bavaria that attacks frogs.—The Botanical Section received from M. C. Wilhelmi an account of those Australian plants which may furnish nourishment to man. The abstract of this paper here published enumerates a considerable number of plants, parts of which are used as food chiefly by the natives.— The rest of the communications to this section require no mention, except a report by M. F. A. Weber upon Hildebrand's work on the sexual relations of the Compositæ.—At one of the general meetings Prof. Hartig reported upon the applicability of various kinds of wood to the manufacture of paper.

## SOCIETIES AND ACADEMIES

### London

Zoological Society, June 6.—Mr. G. Busk, vice-president, in the chair. Prof. Owen, F.R.S., read a paper on Dinornis, being the seventeenth of his series of communications on these extinct birds. The present paper gave a description of the sternum and pelvis, and an attempted restoration of the whole skeleton of *Aptornis defossor*.—Prof. Flower, F.R.S., gave a description of a specimen of the so-called Risso's Dolphin which had been taken in a mackerel-net near the Eddystone Lighthouse, and of a second specimen of the same dolphin subsequently purchased in Billingsgate Market. After a searching investigation of the history of this supposed species, Prof. Flower came to the conclusion that the differences usually held to separate it from the *Delphinus griseus* of Cuvier were untenable, and that the species should be correctly designated *Grampus griseus*.—A second paper was read by Prof. Flower on a specimen of the Ringed or Marbled Seal, which had been obtained on the coast of Norfolk, being the first certain instance of the occurrence of this seal in the British seas. To this was added some remarks on the difficult questions presented by the synonymy of this species, which, after full consideration, Prof. Flower came to the conclusion ought to be called *Phoca hispida*.—A paper was read by Prof. W. Peters, giving a description of the Bats collected by Mr. F. Day, in Burmah. The collection contained a very interesting new form of *Rhinolophi*, which Dr. Peters proposed to call *Phyllorhina trifida*.—A communication was read from Dr. A. Günther, F.R.S., containing the description of a new species of Teius (*Teius rufescens*) from Mendoza, founded on five specimens of this lizard living in the Society's Gardens.—Mr. A. G. Butler communicated a Monograph of the Lepidoptera hitherto included in the genus *Elymnias*.—A second communication was read from Mr. Butler, containing a revision of the species of Butterflies formerly included in the genus *Terias* (*Pierinæ*).—A paper by Dr. J. E. Gray was read, containing a reply to Mr. Theobald's observations on Dr. Gray's paper on the families and genera of Tortoises, printed in a recent part of the Society's "Proceedings."

Chemical Society, June 1.—Prof. Frankland, F.R.S., president, in the chair.—The following gentlemen were elected Fellows : H. Adrian, H. Durham, G. Martineau, E. Neison.— Dr. Debus, F.R.S., delivered a lecture "On Ozone." The first who had observed that the passage of electric sparks through oxygen brings about a change in the properties of this gas was Van Marum. The next to take up the subject was Schönbein, in 1840. He ascribed the peculiar odour and the more energetic oxidising properties of the altered oxygen to a substance which he termed ozone. He also found that ozone may be prepared by many other methods. His experiments, however, led to no positive results, as regards the nature of ozone. It was through the researches of Marignac and De la Rive that ozone was shown to be nothing but an allotropic modification of oxygen. Dr. Debus then discussed the question whether there existed another modification of oxygen, called antozone, and answered the proposition negatively—the substance called antozone was only peroxide of hydrogen. The lecturer concluded by calling special attention to one of the characteristic reactions of ozone, viz., the decomposition of potassic iodide, which reaction is differently explained by the various observers. Schönbein has shown that potassic iodide protects free iodine against the action of potassic hydrate. It may be assumed that potassic hydrates and an iodine solution react upon one another thus :

$$KHO + I_2 = KIO + HI, \text{ and then } KHO + HI = KI + H_2O ;$$

if now an excess of potassic iodide be added, the potassic hypoiodite and potassic iodide produce again potassic oxide (which becomes in its turn a hydrate) and iodine, and the excess of iodide prevents the action of KHO on the iodine, but not that of the latter on starch.

Society of Biblical Archæology, June 6.—Mr. Samuel Birch, LL.D., F.S.A., president, in the chair. The following ladies and gentlemen were proposed by the council for ballot at the next meeting :—Rev. A. H. Sayce, Queen's College, Oxford, E. R. Hodges, late of Jerusalem, Mrs. J. W. Bosanquet, and Miss Dorothy Best, of Maidstone. Mr. George Smith (British Museum) read an elaborate and interesting paper "On the Early History of Babylonia." Commencing with a *résumé* of facts already ascertained by the labours of Sir Henry Rawlinson and other *savans*, he proceeded to describe seriatim the principal localities where excavations had been already undertaken, and to identify them with many of the cities mentioned in the older Books of the Pentateuch. A chronological list of kings and a brief account of the military and political changes, including several new facts from contemporary inscriptions, concluded the first part of the paper. In its second division, the theology, the arts, the social and moral characteristics of the ancient Chaldeans were examined, and the examination was further illustrated by the exhibition of sundry casts of ancient bricks and cylinders, translations of which were also given.—Mr. J. W. Bosanquet, F.R.A.S., treasurer, read a paper "On the Date of the Nativity."

Linnean Society, June 1.—On the nomination of the President the following members of the Council were elected vice-presidents for the ensuing year:—Mr. J. J. Bennett, F.R.S., Mr. George Busk, F.R.S., Dr. J. D. Hooker, F.R.S., and Mr. W. Wilson Saunders, F.R.S.—The following papers were read:—"On some plants from North China," by Dr. Hance; "On South American *Hippocrateaceæ*," Mr. J. Miers, F.R.S. The history of this family shows the widely divergent opinions of numerous botanists in regard to its affinities, the absolute want of knowledge to guide these opinions at last culminating in the extinction of the *Hippocrateaceæ* by the authors of the new "Genera Plantarum," who have reduced it to a mere tribe of the *Celastraceæ*; and not only so, but have amalgamated the several genera previously established into 2, viz.: *Hippocratea* and *Salacia*. The large amount of evidence here presented will, however, show its right to stand as a distinct natural order, having in fact little connection with *Celastraceæ*. The chief characters in its floral structure consist in having five sepals, five alternate petals imbricated in æstivation, and only three stamens (very rarely five); the most important feature is the hypogynous disc, variable in shape, but constantly placed between the stamens and petals; the ovary is always superior, usually 3-locular, with definite anatropous ovules fixed in the axis. The mode of growth of the ovary varies greatly, dividing the family into three separate tribes. 1. *Hippocrateæ*, where, in the progress of growth, the axis of the ovary never lengthens, remaining completely atrophied, the cells growing upwards vastly, sometimes to 100 times the length of the axis at the maturity of the flower, thus producing three distinct capsules from a single ovary, which sometimes open 2-valvately, and have winged seeds, or are indehiscent with nuciform seeds borne upon a carinated ovular support; upon such differences five several genera are established. 2. *Tontelieæ*, distinguished by a drupaceous fruit, often a large size, the growth of an ovary wherein the axis lengthens commensurately with the cells, the fruit being thus 3-locular, with several seeds, which in most cases are covered by an *arilline*, a fleshy complete coating, resolving itself into a mucilaginous pulp that envelopes the seeds; this tribe consists of eight genera. 3. *Kippistieæ*, remarkable for a floral development hitherto unknown among Dicotyledones, but long ago described by Robert Brown in Monocotyledones; here the stigmata, instead of alternating as usual with the stamens and standing opposite to the cells of the ovary, are opposite to the stamens and alternate with the cells of the ovary; the fruit is drupaceous, variable in the position of the seeds, but with characters resembling those of *Tontelieæ*; this tribe consists of three genera. There are thus seventeen genera in all, with well-marked characters, which are separately illustrated by as many drawings, each amply explained by analytical figures. The numerous facts here shown in regard to structure are, for the most part, hitherto undescribed, many being derived from analyses made of plants in the living state. In summarising these details, the author points out the many salient points of distinction in the structure of *Hippocrateaceæ* and *Celastraceæ*. 1. In the former the stamens are generally anisomerous in regard to the petals (three to five); in the latter they are constantly isomerous with stamens equal to, or double the number of, the petals. 2. In the former the stamens are distinctly inserted *inside* the disc; in the latter they are invariably inserted *outside* of the disc. 3. In the former the anthers, generally of a peculiar form, are constantly *extrorse*; in the latter they are of the usually normal structure, and always *introrse*. 4. In the former the disc is generally elevated, and presents a free wall of separation between the stamens and more external parts; in the latter it is a mere expansion of the torus, intervening between the ovary and all other floral parts. 5. In the former the sepals, petals, stamens, and disc are persistent at the base of the fruit, and are never seen in such position in the latter family. 6. In the former, the superior ovary is always elevated above the torus and quite free from it; in the latter it is always more or less partially imbedded in the disc and half agglutinated with it. 7. The atrophied condition of the axis of the ovary, though not a constant feature, is one quite peculiar to the *Hippocrateaceæ*, and on the other hand, in *Celastraceæ*, we find no growth at all approaching the several kinds of large drupaceous fruits seen in the *Hippocrateaceæ*. 8. In the development of the seeds there is a constant distinction. In *Hippocrateaceæ* they are invariably without albumen, in the *Celastraceæ*, without exception, the embryo is enveloped in albumen, usually copious. In the former the cotyledons are often closely conferruminated in a solid mass, a circumstance quite unknown in the latter. 9. In the *Hippocrateaceæ* no trace of an arillus can be seen, in *Celastraceæ*, though not universal, a distinct arillus, in most cases, partially surrounds one extremity of the seeds. In the former, in one tribe, the seeds exhibit a greater or smaller expansion of the testa, in the shape of a large membranaceous wing, or a narrower alar keel, while in the two others they are invested by an *arilline*, an entire fleshy coating, the nature of which was explained many years ago, a feature seen in some other families, though too often unnoticed by botanists. 10. In the *Hippocrateaceæ*, the leaves, but more particularly the branches of the inflorescence, the pedicels, sepals, and petals, contain numerous white elastic threads, which hold the parts together when broken, and these spiral threads often extend to the pericarps, to the integuments of the seeds, and even occasionally to the fleshy cotyledons. Nothing of this kind has yet been observed in *Celastraceæ*. Any one of these peculiarities, by itself, would tend little to support any separation of these two families, but the sum of the whole tells powerfully to mark a great distinction in their organisation. The only arguments that have yet been urged for their near affinity are that both generally consist of arborescent plants with evergreen leaves, an axillary inflorescence, petals and sepals with imbricated æstivation, a three-celled ovary, a simple style and stigma; but these are all characters common to many other families distantly related, and wholly insufficient by themselves to establish any near affinity. The more probable inference is that these two families should be separated by a long interval.

### DUBLIN

Natural History Society.—Prof. E. Perceval Wright, M.D., in the chair. Dr. A. W. Foot read a paper on a small collection of Hymenoptera, named for him by Mr. F. Smith. None of the species referred to were rare, and they had, for the most part, been collected in the counties of Wicklow and Kildare.—Mr. W. Andrews read "Notes on some Irish Saxifrages." Fine living specimens were exhibited of *Saxifraga geum*, and of its varieties *umbrosa*, *hirsuta*, and *elegans*. A coloured drawing by the late Mr. Du Noyer was also shown of a remarkable variety found at the Great Blasquet, in which the flowers presented a glandular disc surrounding the base of the pistil. Specimens of *S. Andrewsii* were also laid on the table. Mr. Andrews stated that he had lately given Mr. A. G. More the exact locality of this rare form, supposed by some to be a hybrid, and he fully expected that in the course of this summer Mr. A. G. More would be able to verify this as well as he had done other of his discoveries. He believed, judging from the structure of its ovaries, that this species had strong affinities with *S. nivalis*. Lastly Mr. Andrews exhibited some very remarkable specimens of *S. stellaris*, which very much resembled in shape *S. leucanthemifolia*, and indeed Mr. John Ball appears to regard this latter form as but a variety of the former.—A resolution was passed that Mr. R. J. Montgomery, Mr. R. P. Williams, Mr. A. Andrews, and Dr. A. Wynne Foot be a committee to have the museum of the society catalogued and arranged for sale, and that the committee be requested to report to the society in November as to any offers they may receive for it. The museum is very rich in Irish birds, containing some unique specimens; but the society not having a house of its own, and holding its meetings in the Royal Irish Academy House, has considered it advisable to dispose of its collections.

Royal Geological Society, May 12.—The Rev. Maxwell Close in the chair. Prof. Traquair exhibited a collection of Carboniferous Ganoid fishes, found in nodules of clay ironstone from Wardie near Edinburgh.—Mr. G. H. Kinahan read a paper on the Geological Drift of Ireland.—Rev. Prof. Haughton read some analyses made by the late M. H. Ormsby of the Geological Survey of India of Granitic Rocks and their Constituent Minerals, found in Lower Bengal and Ceylon. These analyses were made by Mr. Ormsby in 1868, and in expressing his sense of their importance Prof. Haughton also expressed his deep regret, shared in by the Society, at the loss geological science had sustained by the untimely death of so promising a geologist and mineralogist as Mr. Ormsby.—Mr. Edward T. Hardman read a paper on an Analysis of a Trachyte porphyry from Tardree Quarry near Antrim. The paper gave tha result of a careful analysis of one of the two specimens of trachyte known to exist in the British Islands, and from it Mr. Hardman was able to come to such conclusions with regard to the age and altered state of the rocks as led him to controvert the theories of Cotta and Richthofen on the relative ages of basalts and trachytes. Prof. Haughton, who had seen the rock *in situ*, was able to endorse the view taken by Mr. Hardman as to the altered condition of it.

Royal Irish Academy, May 8.—Rev. President Jellett in the chair. Prof. J. M. Purser read a second report on the researches of Prof. Cohnheim on inflammation and suppuration, which was referred to the Council for publication. The secretary read a paper by Mr. Hyde Clarke, on the ancient name Hibernia. This paper was very severely criticised by several members of the Academy, the impression apparently being that the author had no true scientific basis for the conclusion at which he had arrived, and further that the method adopted by him was helping—if it had not already done so—to bring discredit upon this branch of Ethnology.

May 22.—Rev. President Jellett in the chair. The president read a paper on Saccharometing, with special reference to an examination of specimens of sugar beet grown in Ireland.—Prof. Sullivan read a paper on the comparative composition of ancient Bronzes in connection with the ethnology of the ancient people of Europe; also, one on the Great Dolomite Bed of the North of Spain in connection with the Lithonic stage of opal.

### Paris

Academy of Sciences, May 5.—M. Delaunay in the chair. M. Roulin, a member of the Institute, delivered an address, discussing the last communication from M. Ledillot on Arabic Etymology. The learned member contends that M. Ledillot is deceived by superficial and casual analogies, and gives too large a credit to the Arabic language in the formation of French. He frequently quoted the great Etymological Dictionary written by M. Littré, who is now a representative of Paris in the Versailles Assembly, and attempted to vindicate M. Littré's various etymologies.—M. Elie de Beaumont read a letter from M. Bertrand, now at Tours, where he is a delegate for teaching the pupils of the National Polytechnic School. M. Bertrand has worked out theoretically the assertions of M. Navier on the flight of birds. He asserts that the clever mathematician was deceived in supposing the birds exerted an immense force in flight. M. Navier's assertions, which were supposed correct for upwards of thirty years, were very often assailed on practical grounds, and almost generally supposed worthless. But it was necessary to revise his mathematical analyses. M. Bertrand's communication will be welcomed by people engaged in the construction either of flying machines or of apparatus for guiding aërostats.—M. Martins, director of the Montpellier Botanic Gardens, sent a communication on the extraordinary frost of last December. He showed by reliable observations that the temperature at Montpellier was lower by 4° C., than the temperature at Paris. If the Paris minimum is supposed to have been −12°, the Montpellier minimum must have been −16°. M. Martins explains the circumstance by the influence of the Gulf Stream, which diminishes the coldness of the air at Paris more than at Montpellier, owing to the greater distance of that southern station from the ocean.—M. Charles Emmanuel read a paper on certain movements of floating bodies, which he attributes to some electrical influences unnoticed and consequently unexplained hitherto.

May 29.—M. Marié Davy, director of the Meteorological service at the Observatory, read a paper on the effects of the two great atmospheric currents of the atmosphere; one of them north-east, and the second, opposite to the first, south-west. To recite the history of the struggle between these two primary currents would be to recite the eventful history of temperature. Judicious balloon ascents would greatly help meteorologists in executing useful work.—M. Yvon Villarceau gave some most interesting details on the state of things at the Observatory during the night of May 22. The Communists tried to set fire to the establishment, but succeeded only in burning down the wood casements, used to protect the instruments from shelling during the Prussian investment. One circle constructed by Rigault was destroyed. This circle was intended to be used as a mural circle for observations connected with the next international geodesic congress to be held at Vienna, in order to revise the determination of the earth's radius. M. Yvon Villarceau declares that in spite of this misfortune, the French Republic will be able to hold its ground on that pacific battle-field.—M. Chevreul read the speech delivered on his behalf at the funeral of the lamented M. Payen. The learned orator reviewed at full length the different processes resorted to in order to render edible different substances during the first investment of Paris. M. Payen was the originator of these ingenious processes. One of them will be largely used in naval expeditions for procuring fresh albumine for crews and passengers. Ordinary albumine, as it is used by dyers and photographers, is melted at a temperature of 100° C., and can be used for all the cooking purposes. Distant marine expeditions will always remember with gratitude the exertions of M. Payen and his associates for feeding 2,500,000 people surrounded during months by an hostile overpowering force.—M. Chevreul gave some interesting details on the protection of the Museum, and the losses experienced by the great Gobelins' conflagration.

## BOOKS RECEIVED

English.—Hours of Exercise in the Alps: Prof. Tyndall (Longmans)—Astronomy Simplified for General Reading: J. A. S Rollwyn (Tegg and Co.)

Foreign.—Verhandlungen der k.k. zoologisch-botanischen Gesellschaft in Wien: Trübner.

## DIARY

*THURSDAY*, June 15.

Royal Society, at 8.30.—On the Fossil Mammals of Australia. Part V. Genus Nototherium.—Contribution to the Fossil Botany of the Coal Measures. II.: Prof. W. C. Williamson, F.R.S.—On Cyclides and Sphero Quartics: Dr. Casey.—On a Law in Chemical Dynamics, and other Papers: Dr. Gladstone, F.R.S., and A Tribe.

Society of Antiquaries, at 8.30.—On a Reliquary of Sculptured Ivory of the Sixth Century: A. Nesbitt, F.S.A.

Linnean Society, at 8—On British Spiders: Rev. O P. Cambridge.—On a Luminous Coleopterous Larva: Dr. Burmeister.

Chemical Society, at 8.—An Experimental Inquiry as to the Action of Electricity upon Oxygen: Sir B. C. Brodie, Bart.

*FRIDAY*, June 16.

Royal Institution, at 9—On the Esquimaux and Ice of Greenland, illustrated by Drawings and Photographs: Mr. William Bradford, Artist, of New York. (Extra meeting.)

*MONDAY*, June 19

Anthropological Institute, at 8.—Mode of Preserving the Dead among the Natives of Queensland: Albert M'Donald.—Forms of Ancient Interments in Antrim: Dr. Sinclair Holden.—Analogies and Coincidences among Unconnected Nations: Hodder M. Westropp.—Peruvian Antiquities: Josiah Harris.

*TUESDAY*, June 20.

Zoological Society, at 9.—Report on Additions to the Society's Menagerie in May: The Secretary.—On some Arachnida, collected by Cuthbert Collingwood, M.D., during rambles in the China Sea: Rev. O. P. Cambridge.—Notes on some Rodents from Yarkand: Dr. J. Anderson.

*WEDNESDAY*, June 21.

Meteorological Society, at 7.—Anniversary Meeting

Royal Society of Literature, at 8.30.—On the Life and Writings of William of Malmesbury: Mr. W. Birch.

Geological Society, at 8.—Notes on the Geology of Part of the County of Donegal: A H. Green, F.G.S.—On some Supposed Vegetable Fossils: W. Carruthers, F.R S., F.G.S—Memoranda on the Most Recent Geological Changes of the Rivers and Plains of Northern India, to show the Practical Application of Mr. Login's Theory of the Abrading and Transporting Power of Water to effect such Changes. T. Login, C.E.

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THURSDAY, JUNE 22, 1871

## STATE MEDICINE

AMONG the duties which the State owes to Science, none are of more practical and vital importance, and none are more urgent, than those which concern the care exercised, or that should be exercised, over the public health by properly appointed State Medical Officers. The essays of Dr. H. W. Ramsay have so fully explained the term "State Medicine" that we do not feel it necessary here to do more than allude to the subject in very general terms. It will readily be acknowledged that some sort of a medical polity is a necessity for a State; but while in this country certain laws and regulations exist for the improvement of the public health, still there has been but little or no effort made to establish these laws on a scientific basis.

In the recently issued Second Report of the Royal Sanitary Commission, the true relations of the State towards the public in these matters are thus admirably enforced:—"Every person should be entitled to such reasonable public protection in respect of his health as he is in respect of his liberty and his property. For instance, he should no more be liable to have the water of his well poisoned by the neglect of his neighbour, than to be robbed with impunity. And he should be under this protection, as far as it is reasonably attainable, everywhere and at all times. The first principle, therefore, of sanitary administration is, that no member of the community shall wilfully or for profit damage another man's supply of the three absolute essentials of life, food, water, and air; and therefore that it is the duty of the State to secure, as far as possible, that these essentials shall be supplied in sufficient quantity and the greatest attainable purity in all circumstances in which these objects cannot be attained by individual care and resources. In this point of view it may appear a question whether the State should allow that any man, even by prescription, shall be held to have acquired the right to pollute, for his own advantage, another man's food, water, or air, or in any manner poison him. At any rate care should be taken that no one shall acquire such right in future."

The second requirement is laid down with equal clearness, viz.:—"Universality, through constant supervision by public health officers in every part of the country. The efficiency of the agents in sanitary administration is as important as their ubiquity. They must be well instructed and capable, without the pedantry or officiousness of sciolists. Ignorance, pretentiousness, or overmeddling on the part of the agents, would bring into disrepute any sanitary system. In a free country disrepute would bring about failure. Fitness in the agents is the third requisite in sanitary legislation."

When, however, the Commission comes to apply these principles to the existing state of things, the only practical suggestion offered is that the supervision of the public health be entrusted to the Poor Law Medical Officers, of whom there are in England alone about 4,000. The Commissioners have evidently a suspicion that this suggestion will not be favourably received by the country. And we have no hesitation in saying that it is miserably inadequate. When we look at the value of the examinations to which alone medical students are compelled to submit themselves before they obtain a license to practise, or when we look (must we say it?) at the life of the average medical student attached to any of our great hospitals, no two conclusions are possible on this subject. It is notorious that, as a rule, it is not the most competent of the London students who ultimately arrive at the position of general practitioner in a country village; and the Poor Law authorities, however discriminative their choice, can only select from the material to their hand. To effect the objects arrived at by the Sanitary Commission, a far more highly educated class of men is required.

That medical men should be educated in a knowledge of State medicine will probably not be denied, and that the State for its own good should encourage such knowledge will probably also be granted; but it is not easy to persuade a State to adopt even approved of principles, if these principles require a wholly new machinery for the effectual carrying of them into practice. The Universities are, however, engaged in the work of education, and upon them, we think, devolves the duty not only of keeping up the standard of education, but of endeavouring to push this standard ever a little advance of the day.

The training necessary for the medical profession is very different from that required to qualify one to be an authority on State medicine; it most certainly assists in this qualification, but a man might be a most excellent surgeon or a most skilled physician, and yet not be able to pronounce an opinion on many of those subjects on which his advice would be required by the State.

In a medical school belonging to a college which holds out considerable rewards to those students who distinguish themselves as classical or science scholars, there is always a probability that some of the students in medicine will have also been distinguished students in arts. Experience has proved that this is the case in Trinity College, Dublin; and experience has proved the incalculable advantage of a high training in art-subjects to the future medical man. What better combination of knowledge, indeed, could there be to form a model officer of State medicine than that of a thorough knowledge of science (using the term as it is generally understood at the Universities) and of an equally thorough knowledge of medicine? We are glad, therefore, to find that, acting on the suggestion of Dr. Stokes, their Regius professor of physics, the University of Dublin has determined to hold a yearly examination for a diploma on State medicine, the first of which was held on the 12th inst. This examination was open to all doctors of medicine of the Universities of Oxford, Cambridge, and Dublin.

The course is a long but a highly interesting one. It resolves itself into the following among other subjects:—1. Law: The legislation relative to sanitary measures, to the conduct and duties of medical men, to vaccination, inoculation, lunatic asylums, &c. 2. Engineering: This chiefly in connection with the construction of hospitals, barracks, troop ships, prisons, and the sewerage and waterworks of cities. 3. Vital and Sanitary Statistics, including the science of statistics as applied to man, and the practical application of statistics to medicine. 4. Meteorology, including a knowledge of climates, &c. In addition the candidates will be examined in Pathology, *i.e.*, the laws of epidemics, of contagion and infection, influence of hereditary disposition, &c.; in Chemistry, under the heads of—1, air; 2, water; 3, gaseous poisons; 4, principal deodorising and disinfecting agents; and in Medical Jurisprudence under the divisions of Hygiene and Forensic Medicine. This course has been evidently selected with great care, and appears well calculated to test the qualifications of the candidates. The medical men who successfully pass it and obtain the diploma, ought certainly to be able to assist in establishing on a scientific basis the laws relating to the public health.

One very serious omission we observe in the list of subjects to be examined in, and it is one we would have least expected, viz., the Microscope and Spectroscope. It is perfectly astonishing to find the number of well-educated men in the medical profession who are unable to understand the ordinary manipulation of an ordinary microscope, or of a spectroscope in connection with the microscope. The medical men who pass this examination will, we believe, take rank at once as medical experts—but fancy one qualified to act as a medical expert and yet not knowing how to manage an achromatic condenser!

At present this movement of the Dublin University can but be regarded as an experiment, but it is an experiment in the right direction, and one that has been, and we hope for years will be, conducted under the watchful eye of a most able physician, who thoroughly understands the subject of medical education, and who, throughout his whole life, has laboured to elevate the profession that he adorns.

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## *PRIMITIVE CULTURE**

### II.

THE chapters on mythology, which naturally follow those on language, form an admirable summary of the history of myth from its vigorous infancy in the earlier ages of human thought through the various stages of growth and maturity onwards to second childhood, death by ossification of the heart, and final post-mortem existence through millenniums of disembowelled mummydom. Myth, in fact, is as ubiquitous, as multiform, as language. Nay, it is perhaps more ubiquitous, more multiform. The spaniel, who fawns on his master or flies at a beggar, who bays at the moon or cowers from the thunder, has evidently framed to himself some simple dog-theory in connection with certain phenomena, which is closely analogous to, if it be not absolutely identical with, a rudimentary myth. It is, indeed, probably not too much to say that wherever a phenomenon is stated or explained, whether with or without the intervention of language, there exists a myth, though a higher knowledge than that which creates the myth is always requisite in order to recognise its mythic character. The Ptolemaic system of astronomy, for instance, has been long ago conclusively demonstrated to be a myth, although a myth belonging to an advanced stage of culture, and a thousand and a thousand others are everywhere around us only waiting for the extension of knowledge to effect the metamorphosis requisite for their recognition. It is evident that if this theory of myth be even approximately correct, the statement or explanation of any phenomenon in language is in effect merely the creation of another phenomenon out of which myth may be evolved *ad infinitum*; in short, that myth is essentially the outcome of the complex action, reaction, interaction, and counteraction of human thought on the one hand, and the sensible phenomena of the universe, including those of language, on the other. The sensible phenomena of the universe may thus not inaptly be regarded from the standpoint of Democritus or Lucretius as continually throwing off films or likenesses of themselves, which films or likenesses, once seized and appropriated by language, become additional phenomena, with a vitality, so to speak, and reproductive power of their own. On the other hand, if, in accordance with the spirit of Scandinavian philosophy, we regard philosophy itself, art, poetry, science, morality, and religion—all the products of human thought—as a single living organism, we may then consider myth as the former substance of the organism, the physical atoms which have been gradually eliminated and replaced in the process of growth and development. Or, not to complicate matters by the introduction of evolution,—civilised knowledge, as a whole, may be likened to an old canoe, of which no plank nor nail is the same as when she started on her first voyage, and myth to the old timbers and metal which once formed a part of her, but have now been some lost, some metamorphosed into wholly different shapes, some utilised again in the construction of other vessels. We can thus understand how every department of thought has absorbed and assimilated more or less of myth,—how myth has absorbed and assimilated more or less of every product of the human intellect. It is, in fact, the non-appreciation of the true place of myth in human knowledge, which has led so many earlier students of mythology astray. One school looked on all mythology as crystallised poetry; another as indurated chronicle; a third as frozen philosophy; a fourth as petrified religion, and so forth;—each school doing something towards really making mythology what it believed mythology to be, and all, as a net result, extracting from one of the most vitally-interesting investigations a mere *caput mortuum* of doubly-distilled platitude, and quintessential commonplace. So long as "mythology" meant simply an acquaintance from without with the Greek and Roman Pantheon, such a result was, perhaps, inevitable. Unfortunately the doctrines of these schools are not even yet by any means universally recognised as being themselves mythic; and many of them are still to be found reproduced in contemporary works of no inconsiderable learning, to supply future students with illustrations of Mr. Tylor's theory of survival. It must be admitted, too, that even the late brilliant achievements of more scientific inquirers still leave a vast field untouched for classification and comparison. Nor is this task an easy one. A myth is always the statement or explanation of a phenomenon, and myths may thus be classified according to the phenomena to which they refer; but first of all "to catch your myth," and then to determine the phenomenon to which it refers, are feats, for the most part, beyond the skill of ordinary students. An amusing instance of these difficulties is afforded by Mr. Tylor himself. "No legend," he observes, "no allegory, no nursery rhyme, is safe from the her-

* "Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Art, and Custom." By Edward B. Tylor, author of "Researches into Early History of Mankind," &c. Two vols. 8vo. (London: Murray, 1871.)

meneutics of a thorough-going mythologic theorist. Should he, for instance, demand as his property the nursery 'Song of Sixpence,' his claim would be easily established: obviously the four-and-twenty blackbirds are the four-and-twenty hours, and the pie that holds them is the underlying earth, covered with the over-arching sky; how true a touch of nature it is that when the pie is opened—that is, when day breaks, the birds begin to sing; the King is the Sun, and his counting out his money is pouring out the sunshine, the golden shower of Danae; the Queen is the Moon, and her transparent honey the moonlight; the Maid is the 'rosy-fingered' Dawn, who rises before the Sun, her master, and hangs out the clouds, his clothes, across the sky; the particular blackbird, who so tragically ends the tale by snipping off her nose, is the hour of sunrise. The time-honoured rhyme really wants but one thing to prove it a Sun-myth, that one thing being a proof by some argument more valid than analogy." This is exquisitely ingenious; but what if the rhyme should turn out to be, after all, only a quite genuine nursery riddle, of the type which Mr. Tylor has so admirably illustrated elsewhere? An archetypal clock, presented as a *haute nouveauté* to some Edward III. or Richard II. would satisfy all the conditions of the enigma. The large circular face would represent the pie;—the four-and-twenty hours duly figured thereon, in accordance with the liberal notions of archaic horology, would correspond to the four-and-twenty blackbirds; the striking, possibly with chimes, to the song of the birds; the king in his counting-house, counting out his money, would felicitously symbolise the hour-hand counting out the time, which is money, in majestic solitude, unaccompanied as yet by any fussy revolutionary minute-hand; the queen in the pantry, eating bread and honey, would typify the stealthy activity of the fine wheel-teeth of steel and brass; the maid in the garden, hanging out the clothes, would appropriately allegorise the wooden drum on which the weights were suspended by lines, at a distance from the works; while the magpie, which seems a preferable heading to "black-bird" who snaps off the maid's nose, would probably be none other than the ingenious mechanist who wound up the instrument, and, having done so, removed the key from the nozzle of the drum. Whatever may be thought of this interpretation, it seems exceedingly probable that the rhyme is really a riddle, and, indeed, many other unintelligible jingles are most likely referable to the same category. One of them, if a riddle, does also unquestionably enunciate a sun-myth. In the immortal Jack and Jill who went up a hill to fetch a pail of water, we may clearly recognise the sun and moon under an enigmatic, not to say riddle-iculous exterior, and after satisfying ourselves as to their identity, we may further admire the curious felicity with which the difference of sex between Hêlios and Selênê—etymologically identical with the difference between *leôs*, a lion, and *leaina*, a lioness—is indicated in the English ditty. To return, however, from the precincts of the nursery, Prof. Max Müller, with a natural bias in the direction of his own brilliant researches, seems to ascribe the origin of myth somewhat too exclusively to the influence of language, just as in his interpretation of myth he appears to pay a rather too marked attention to the Dawn-Goddess to do full justice to the claims of other less seductive divinities. The Professor himself, however, will probably be among the first to recognise the value of Mr. Tylor's distinction between material and verbal myth, and to acquiesce in the classification which considers the former as primary, the latter as secondary in the order of evolution.

In his account of eclipse-myths, Mr. Tylor quotes sundry remarks of Mr. Samuel Davies eighty years ago with regard to the struggle between ecclesiastical authority and science in India. "The learned Pundits," says Mr. Davies, "reject the ridiculous belief of the common Brahmuns, that eclipses are occasioned by the intervention of the monster Rahoo, with many other particulars equally unscientific and absurd. But as this belief is founded on explicit and positive declarations contained in the 'Vedus' and 'Pooranus,' the divine authority of whose writings no devout Hindoo can dispute, the astronomers have some of them cautiously explained such passages in those writings as disagree with the principles of their own science; and where reconciliation was impossible, have apologised as well as they could for propositions necessarily established in the practice of it by observing that certain things, as stated in other Shastrus, might have been so formerly and may be so still, but for astronomical purposes astronomical rules must be followed." It is, perhaps, not a mere accidental coincidence that in 1760, a few years before this was written, the following "Declaratio" appeared at the end of the third volume of the Jesuit edition of Newton's works, published at Geneva:—"Newtonus in hoc tertio libro telluris motæ hypothesem assumit. Autoris propositiones aliter explicari non poterant, nisi eâdem quoque factâ hypothesi. Hinc alienam coacti sumus gerere personam. Cœterum latis a summis pontificibus contra telluris motum decretis, nos obsequi profitemur." Fortunately for Mr. Tylor and his fellow-workers, the difficulties which beset the scientific inquirer from this source have probably almost reached their minimum in England. Ecclesiastic thunder itself has lapsed into mere survival, and roars, if it roars at all, only after the fashion of Snug the Joiner's lion.

The remainder of the work, occupying part of the first and the whole of the second volume, is devoted to a discussion of "Animism," in other words, of the philosophy of religion in relation to early and barbaric civilisation. The subject is one of almost equal interest, importance, and difficulty, and Mr. Tylor's treatment of it is eminently original and masterly. Tracing the origin of a belief in spiritual beings to the result of primæval thought on the problems presented by the difference between the dead and the living body, by sleep and waking, trance, disease, and death, he follows the course of its development upwards into the existing religions of the most civilised races. Showing how the doctrine which teaches a possible continued existence after death of the souls of individual creatures really supplied a theory adequate to explain the phenomena to the barbaric intellect, he calls attention to the process by which this belief in a ghost-soul became expanded into a belief in other spirits, who were held to control the events of the material world, and hence became the objects of worship and propitiation, until Animism reached its full development in a system inculcating a belief in controlling deities and subordinate spirits, in souls separable from bodies and a future state of existence, morality being incorporated into religion only in the later stages of culture. One of the most striking points in the whole work is Mr. Tylor's identification of the theory of "images" generally ascribed to Democritus with the savage theory of object-souls. Democritus explained the fact of perception by declaring that things are always throwing off images of themselves, which images, assimilating to themselves the surrounding air, enter a recipient soul and are thus perceived. This theory, Mr. Tylor adduces evidence to prove, is merely an application to the phenomena of thought of one of the most characteristic doctrines of savagery, the doctrine that every object, inanimate as well as animate, possesses a soul of its own. "Nor is the correspondence," says Mr. Tylor, "a mere coincidence, for at this point of junction between classic religion and classic philosophy the traces of historic continuity may be still discerned. To say that Democritus was an ancient Greek is to say that from his childhood he had looked on at the funeral ceremonies of his country, beholding the funeral sacrifices of garments and jewels and money and food and drink, rites which his mother and his nurse could tell him were performed in order that the phantasmal images of these objects might pass into the possession of forms shadowy like themselves, the souls of dead men. Thus Democritus, seeking a solution of his great problem of the nature of thought, found it by simply decanting into his metaphysics a surviving doctrine of primitive savage animism." No more pregnant identification of philosophic tenets with those of earlier religion has been achieved since Comte traced back to fetishism the conception of a soul of the universe as held by certain pantheistic schools.

In describing the nature of the soul as understood by the lower races—well indicated by the way in J. Amos Comenius's "Orbis Sensualium Pictus," where he figures *anima hominis* as a dotted outline of a man—Mr. Tylor calls special attention to the spirit-voice, which is conceived as a murmur, chirp, or whistle—as it were the ghost of a voice. Among the Algonquins souls chirp like crickets; among the New Zealanders, Polynesians, and Zulus, they squeak or whistle. Nicolaus Remigius, whose "Dæmonolatreia" is one of the ghastliest volumes in the ghastly literature of witchcraft, cites Hermolaus Barbarus as having heard the voice *sub-sibilantis dæmonis*, and, after giving other instances, adduces the authority of Psellus to prove that the devils generally speak very low and confusedly in order not to be caught fibbing. The idea of ghosts whistling is still far from extinct in England. In Leicestershire and elsewhere it is reckoned "very bad" to hear "the Seven Whistlers," though strict inquiry about them only elicits the suggestive fact that "the develin"—or common martin—"is one on 'em."

In his account of the doctrine of transmigration of souls, Mr. Tylor forbears to touch on one circumstance, which probably exercised some considerable influence on its development. When two systems of mythology, both originally derived from the same source, came into close contact after long separation, both the difference and the similarity between them could hardly escape attention. If the names of certain deities common to the two systems had been changed while their history and attributes had remained substantially unaltered, the theory of transmigration would, in some cases, satisfactorily account for the phenomenon. In fact, mythologically, the doctrine of transmigration is simply true. Mythology is just now demanding of history the extradition of William Tell, on the plea that his ghost is one which has transmigrated from her domain; and the scientific detective who falls in with Robin Hood or King Arthur will hardly fail to recognise in the one the transmigrated soul of Phœbus Apollo, in the other, the wandering spirit of the Bear-ward in Böotes, returned from his long sojourn in the northern sky.

Tempting, however, as are the inquiries suggested in this profusely suggestive work, the reviewer's limit has already been transgressed. We have not yet, we cannot have for years, or for ages, anything approaching to a complete science of history or exhaustive philosophy of religion, but the scientific student of Primitive Culture will at least admit that in these volumes the foundation of both has been "well and truly laid."

## BOOK SHELF

*Dr. Dobell's Reports on the Progress of Practical Medicine in different Parts of the World.* Vol. ii. (1871, Longmans.)

In these reports Dr. Dobell aims at obtaining from the natives of different countries concise statements of the advances made in medicine and the allied branches of knowledge, which have appeared in foreign journals, or in a more permanent form. He has obtained more or less full and detailed reports from America, Australia, California, China, France, Germany, Iceland, India, Italy, Java, Newfoundland, New Zealand, Portugal, Prince Edward's Island, Shetland Isles, Turkey, and the United Kingdom. The idea is a good one. The flood of periodical literature is so great that it is most difficult to keep up with the weekly journals of this country alone, and it becomes almost hopeless to do so with those of France and Germany. Such reports as those before us materially lighten labour, and the only objection to them is that a man who is working at any given subject cannot rely upon their being complete. The report on French progress by Prof. Villemin is a good one. That on German advances, by Dr. Alhaus is much too short. It might, with great advantage, have been extended at the expense of the excerpts from English writers. Everyone has access to the leading English journals, and, moreover, this part of the work is already well done by Braithwaite and Ranking, but comparatively few have access to Virchow's Archiv, the Deutsch Klinik, and the Wiener Medizinische Zeitung. Many of the English abstracts might have been condensed. We miss a Russian report. Yet both Russian naturalists and Russian physicians have journals of their own. On the whole the book is a useful one, and we can recommend it to our readers as containing a considerable mass of information which they will not elsewhere easily find.

*Geometrische Seh-Proben zur Bestimmung der Sehschärfe bei Functions-prüfungen des Auges.* Von Dr. Boettcher. (Berlin, 1870. London: Williams and Norgate).

This little book, with its test objects, is intended as a substitute for Snellen's test types to be used by those who are unable to read, and has been drawn up by Dr. Boettcher, with especial reference to the testing of the vision of recruits. Besides the ordinary types, it contains a number of figures of squares and rectangles, variously disposed in regard to one another at different distances, and it need scarcely be added of various sizes. The very smallest require good vision to enumerate their number and disposition at the ordinary distance of eight inches, whilst the largest should be seen at two hundred feet. They afford a good means of determining the existence of Hypermetropia, Myopia, Astigmatism, and other affections of the retina of the eye, and seem to us to be well adapted for the purpose for which they are intended. H. P.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Thickness of the Earth's Crust—Mr. Hopkins and M. Delaunay

In your numbers for March 16 and 23, 1871 (pp. 400, 420) you give brief notices of the proceedings of the Academy of Sciences in Paris on the 6th and 13th of that month, from which it appears that Mr. Hopkins's method of determining whether the crust of the earth is thick or thin has been again under discussion there. In the latter of these notices M. Delaunay observes that he had been anticipated in his objections by Mr. Hennessy.

It so happens that Mr. Hopkins sent to me in Calcutta in 1858 a copy of Mr. Hennessy's paper (which was published in the *Philosophical Transactions* of 1851) with his remarks in writing in the margin; and I think it will be interesting to your readers if I give Mr. Hopkins's opinion of the paper.

Mr. Hennessy remarks (p. 546) that Mr. Hopkins's "result was founded on the hypothesis of the non-existence of friction and pressure from molecular causes at the surface of contact of the shell and nucleus." On which Mr. Hopkins writes:—"This is not correct. My hypothesis is the absence of friction between the fluid particles themselves."

Again, Mr. Hennessy considers, as a result of his calculation, "that we are entitled to assume that the motion of rotation of both shell and nucleus takes place nearly as if the mass were entirely solid." On which Mr. Hopkins observes: "Nothing could justify the assumption of a mechanical impossibility." And he traces this erroneous conclusion to the fact that Mr. Hennessy has made two assumptions in the course of his calculations which vitiate it throughout, viz., (1) that the axes of instantaneous rotation of the shell and nucleus would coincide (which is implied in the last formula in par. 2. p. 514), "which," wrote Mr. Hopkins, "they certainly would not," and (2) that "the shell (or crust) is rigid" (p. 519, 525) so as to resist, without change of form, the internal pressure which arises from the inner surface, that is, the surface of the nucleus, ceasing to be a surface of equilibrium, which Mr. Hopkins very reasonably considers to be quite inadmissible, and that accordingly the results deduced from these assumptions are "valueless."

2. I will take this opportunity of reverting to my letter to you of April 10, 1871. I there point out that what Mr. Hopkins did consists of two parts—(1) his conception of the idea that as the crust is not solidly connected with the fluid nucleus the amount of precession must depend in some measure upon the thickness of the crust; and (2) his *calculation* of the amount of precession this idea would lead to, so that by comparison with observation the thickness might be approximately found. In this way he discovered that if the crust and nucleus were homogeneous, and of the same density (which they are far from being), the inner and outer surfaces of the crust being similar and similarly situated spheroids, the internal pressure of the fluid would act so as to leave terms in the precession depending on the thickness of the crust, only of the second order of small quantities; whereas, in the case of the earth where the mass is heterogeneous, the mean density being double of the superficial density, the thickness is involved in terms of the first order in the expression for the precession, and by a comparison with observation leads to Mr. Hopkins's result, viz., that the thickness is very great, something like 800 or 1,000 miles at least.

Calcutta, May 24 John H. Pratt

### The Duties of Local Societies

It is undoubtedly the work proper to local natural history societies to study well the productions of their own immediate neighbourhood, to catalogue all the fossils, plants, and animals, and to note any peculiarities regarding them. In the settlement of the great questions still under discussion, much will depend upon their faithfully performing this duty. All naturalists will cordially endorse whatever has been said in regarding such societies from this point of view, and will agree in declaring that it is far better they should be occupied in such labour than in the discussion of theories and abstruse general questions, which are better left to larger and more influential bodies. It is their office to collect facts upon which individual minds may generalise. This of course applies to such bodies in their collective capacity, and not to the members as individuals; it is very probable that such individuals may make use of the facts collected by the society.

But it should also be remembered that local societies have another duty to perform, and one, too, of hardly secondary importance, and that is the inculcation of the love of natural history in other minds. Indeed, it will often be found that for a time at least this must take precedence of the work already mentioned. It is well known to all who have taken any active part in field clubs and the like, that the real work devolves upon two or three members, sometimes fewer still, and it is totally beyond the power of these two or three to work up the whole natural history of the district by themselves; they may have the will and the ability, but neither the time nor the means. We have known societies numbering above a hundred members where this was the case; the great majority could not be called working members at all; they joined for various reasons, some merely because it was rather "the thing" to do in the town or village; the greater number probably because they were interested in seeing specimens, hearing pleasantly-written papers at meetings, highly delighted at microscopical conversations, &c., &c., but did not care about working in the subjects for themselves. It is very evident that in such cases the first and foremost aim should be to induce as many as possible to become students of nature, at least to enable them to make intelligent notes of what they observe. Hence we find that the reports of provincial societies occasionally appear with little addition to what is already known, very little local information in them, and though this is to be greatly regretted (and in reality is so by the editors), it should not be regarded, as it often is, merely as matter of reproach by others more advanced. Local magazines and reports are issued mainly for the perusal of the members and others living in the locality, and such persons naturally wish to see the best of the papers that have been read; and the matters that interest them most are not always new discoveries, wherever they may be made.

But besides the ordinary method of reading papers at meetings principally, if not solely, attended by members, there are others to be followed by way of inducing outsiders to join the society. One is that of "Penny Rambles," which has been very successful in some localities. These should be conducted by some one not only well versed in natural history, but gifted with the not common ability to impart his knowledge in an attractive and popular way. There should be a change of conductors as often as possible, but care ought to be taken always to secure good ones, as one disappointment lasts a long time; this implies that some competent person or committee should have the control over the arrangements. Sometimes it will be found that one man makes himself so well understood, and consequently so attractive, that he is preferred to others, and here the *vox populi*, within due limits, should have weight. As for subjects, no naturalist will ever be at a loss: the geology of the neighbourhood, some quarry, sand pit, or sea beach; the botany, some of the rarities and their peculiarities; entomology also, and an occasional chat about some interesting antiquity in the vicinity. No abstruse theories should be taken up, they may be intensely interesting to the real naturalist, but are in a general way unknown, and if known, unappreciated by the multitude.

The science lectures, lately so admirably carried out at Manchester, and already referred to in these pages, will occur at once to every one; they might well be taken up in all large towns. But could not Museum Lectures also be started? The duty of every local society to establish a museum has been ably argued by another pen—a museum not for its own sake merely, but with this secondary idea before them, the good of the town or village in which they may be located; one in which the labouring man shall find displayed specimens of the wealth of his own neighbourhood, as well as typical forms from far off lands. Prof. Huxley has well said in one of his "Lay Sermons" that there is a general impression among people that every event of importance happened a long time ago; it is equally true that they fancy any natural object worth looking at must be sought for a long way off; it is our duty to eradicate both impressions. If some plan could be adopted of giving penny lectures in the Museum, it would greatly add to the interest excited; it would assist people to understand what they see, and tend to the destruction of that languid curiosity so painfully evident in the faces of sightseers. The contents of one case would serve for one or two lectures, and those who listened would necessarily carry away a few new ideas.

We have been viewing the question solely from its popular side, being convinced that it is of great importance; other plans may occur to the reader, and may be well worthy of ventilation.

HENRY ULLYETT

### Colour

SINCE the publication in NATURE of my paper on "Colour," I have received several inquiries for references on the subject. These I should have given at the time, only that I wrote away from books; perhaps on the principle of "better late than never," the publication in NATURE of the following selection may save trouble to some interested in the matter.

Helmholtz: "Ueber die Theorie der zusammengesetzten Farben;" "Poggendorff's Annalen," lxxxvii. p. 45; "Philos. Magazine," (4) iv. p. 519.

Maxwell: "Experiments on Colour perceived by the Eye, with Remarks on Colour Blindness;" "Edinburgh Transactions," xxi. p. 275.

Maxwell: "On the Theory of Compound Colours, and the Relations of the Colours of the Spectrum;" "Phil. Trans.," 1860.

Maxwell: "Account of Experiments on the Perception of Colour;" "Phil. Mag." (4), xiv. p. 40.

Müller: "Zur Theorie der Farben;" "Pogg. Ann" vol. 139, p. 411.

These are the principal original memoirs. Of books on colour there are very few that can be trusted. Benson's "Principles of the Science of Colour" is recommended by Prof. Maxwell. There is also a tolerably complete exposition of the subject in Helmholtz's "Physiologische Optik," of which excellent work a French translation has, I believe, been published.

Have any of the readers of NATURE tried a double image prism for exhibiting the mixtures of two colours? By the aid of a Nicol the proportions of the components may be varied at pleasure, and the combination is, in my experience, more effective than the plate of glass referred to in the books. However, on account of the texture of the coloured papers or wafers, the mixture is not so perfect as that obtained by rotation.

J. W. STRUTT

### A Hint to the Longsighted

A SMALL optical expedient which has been of service to me may be new to some of your readers, and useful, on occasion, to those among them whose sight is as long as my own. The focal length of the convex lens I require for my right eye in reading is twelve inches, and I find that by holding a lens of 30-inch focus about a foot from this eye I am enabled to see distant objects not only with singular distinctness, but also perceptibly magnified. I can read moderate-sized print at the distance of twelve feet, and make out the details of a church tower half a mile off nearly as well as with a small opera glass magnifying two and a half times. The greater the distance of the lens from the eye the greater is the magnifying power; but beyond a certain point (depending on the focus of the lens and the distance of the object) the gain is more than neutralised by the loss of distinctness with eyes that deviate but slightly from the normal standard, the lens employed must be so weak that the gain is inappreciable.

I presume that a lens thus held at a distance from the eye, like the German "Stöpfel Linse" described by Sir John Herschel, "realises the notion of Descartes as to the mode of action of a telescope, which he regarded as an enlargement or prolongation of the eye. For the natural cornea we substitute an artificial one, which is more remote from the retina, and so forms there a larger image."

W. T. RADFORD

### Lignite and Selenite

WILL you kindly allow me to inquire whether any of your readers can inform me if there exists any connection between lignite and selenite when found together, and, if so, in what way the lignite assists in the formation of the crystals of calcium sulphate.

I have recently found selenite in three or four different places, and in each case associated with lignite, viz., in the Bracklesham Beds near Stubbington, in the Woolwich Beds at Dulwich, and in ochrey clay near Lewisham Chalk Pits.

June 19 AN AMATEUR

### Arctic Auroras

IN answer to your inquiry, I send you the following information on a Northern Light observed at Kooltook, S.W. end of the Baikal Lake, by Dr. Dyhoffsky. It is taken from a source doubtless not at your disposition (Bulletin of the Siberian section of the Geographical Society, 1871, No. 2):—

"On October 24 (1870) evening a northern light was observed at Kooltook. It began at 9 P.M. with a red light, which appeared more and more distinctly from behind the mountains that border the landscape on the north. It was a little towards the east from the magnetic meridian. This light now increased in the form of a column, now diminished, and at times seemed to vanish entirely. After nearly an hour of such waverings, the light gradually began to increase and get broader; at midnight it reached its utmost intensity and development.

"Its least limits on the horizon were included between N. 59° E. and N. 45° W. Six columns were distinctly visible at midnight, reaching half the distance between the horizon and the zenith, the middle column was the brightest and highest, but at the same time the narrowest, and bordered with reddish-yellow. The other columns were less brilliant but far broader. When the middle column decreased, the western one began to increase, though it never reached the intensity of the middle column. The other columns also increased and diminished by turns; then the phenomenon gradually fainted away, and at three o'clock there remained but a ruddy light, which now, as at the beginning, was brighter towards the east of the meridian."

The same aurora was observed at different localities of Europe.

P. KROPOTKINE

Catherine Channel, Petersburg, May $\frac{13}{25}$, 1871

### Day Auroras in the Arctic Regions

I CAN now answer Dr. Burder's question regarding the appearance of the Aurora Borealis in the Arctic Regions. The other evening (last Thursday) I had a conversation with a distinguished magnetician and Arctic explorer, and he informed me that he has often seen the Aurora in *broad daylight* in those regions, the colour invariably being crimson. This, I hope, will once for all settle the apparently vexed question (*pace* Dr. Burder) of "alleged" daylight Auroras. Not to repeat the entire "crusher" of Dr. Burder's, I think many will now discard as "unworthy of serious criticism" his cirrus-cloudy arguments. He must pardon me for being so unceremonious, and remember his own interesting way of confuting—or, better, his attempt.

JOHN JEREMIAH

## *SCIENCE IN PLAIN ENGLISH*

### I.

IN tracing the development of public opinion, no period is more instructive than the last three hundred years; and at present the review is particularly important, for we seem to be in a position analogous to the state of Europe just before the Revival of Classical Learning. We are evidently on the eve of great changes in principle, and one vital question is to consider the value of classical culture as compared with the study of science.

The distinctive work of the thirty years (1820-1850) was to "diffuse useful knowledge" among the middle classes. Beside the establishment of mechanics' institutions throughout the country, the London University was founded in 1828; and the British Association for the Advancement of Science held its first annual meeting at York on September 27, 1831, under the presidency of Earl Fitzwilliam.

Another agency has been brought into action, more especially directed to the practical arts, and bringing into friendly competition the various nations of Europe. The International Exhibition of 1851 had a remarkable influence upon the application of science and art to trade and manufactures, calling forth a memorial from the leading manufacturing and commercial towns as to the importance of establishments for instructing workmen in the principles of science and art, on which their respective industries depend. It was stated that unless this was speedily done the country would run serious risk of losing that position which hitherto had been its strength and pride.

This foreboding was confirmed by the Exhibition at Paris in 1867, which showed an advance made by Continental nations even in some departments in which England had been considered supreme. The conclusion was received with surprise in some quarters, and vexation throughout the country.

There was no doubt that remarkably rapid progress in manufactures had been made by some of the Continental nations; and this rapid improvement was attributed, in a great measure, to the scientific training of proprietors and managers in France, Belgium, Germany, Switzerland; and to the elementary instruction which is universal amongst the working population of Switzerland and Germany. The facilities for acquiring a knowledge of theoretical and applied science are incomparably greater on the Continent than in this country; and that knowledge is based on an advanced state of secondary education.

Hence, a great effort has been made to obtain similar advantages of education for this country, in order that we may retain the position which we now hold. There can be no doubt that scientific training has become a question no longer admitting of delay; and a demand has arisen for Technical Education, by which we are to understand scientific and artistic education, with a view to improvement in industry.

To promote this object, several educational reforms have been suggested; and first of all, that in the universities and grammar schools instruction in science and art should be placed on the same favourable footing as other studies. Only one-third of the boys in the great public schools go to our universities; and therefore, two-thirds pass directly from the schools to enter upon the various pursuits of life. Now there are two methods of education. One gives a youth direct preparation for his future pursuits; the other trains the mind by processes which are not directly adapted for any worldly career, but which are supposed to strengthen the intellectual faculties.

The latter object is pursued in classical education, which is defended upon the ground that, though it does not provide special instruction for the useful purposes of the world, it still furnishes general culture. No one can deny that classical education supplies excellent training in certain directions; but there is a growing conviction that, for the practical purposes of life, the classics have been tried and found wanting; that, while they serve for ornament and for delight, they are not "good for life."

But even with a view to culture, we should not overlook the importance of Science in mental training. Science, properly taught, is one of the best means of educating the highest faculties of the human mind. By proper teaching, however, we must understand, not merely instruction in the facts of science, but discipline in the methods of science. Mere head-knowledge may do a man very little good; it is the habit of mind, the training in method, that determines the character of the man. Hence, the minds of the young should be imbued with scientific principles and trained in scientific methods.

A twofold advantage is asserted by scientific advocates: that as science has now reached so high a stage, it may be used as a means of the best mental cultivation; while, at the same time, it communicates a kind of knowledge which may be made practically useful in every walk of life.

A movement has already been made in some quarters, but sparingly, not to say grudgingly. Some schools have admitted science on about equal terms with dancing, that is to say, they give one or two hours a week to it. Or they may even admit it on equal terms with French; but it is generally made quite subordinate; and while classics are rewarded with high honours, science receives few distinctions. At Harrow the teaching of physical science has been introduced, but has not yet been made part of the regular curriculum; boys are not obliged to learn physical science, though they may get prizes for it. The most difficult point in this part of the subject is where to find suitable masters for the teaching of science. This, no doubt, must be a work of time; but if the demand springs up, the supply will follow.

But beside the demand for a reform in the institutions already existing, there is a general conviction that scientific and technical schools are required in all the great centres of industry; that such schools ought to be established; that we must have "Technical Education." In many districts those who desire to send their own sons or the sons of their better workmen for instruction in science, are unable to carry out their views because no suitable schools exist in their neighbourhood. There are numerous grammar schools in different parts of the country, but many of them were founded in the two centuries which followed the Revival of Classical Learning. Consequently, they are generally under the influence of classical traditions; and a comparatively small proportion of the boys are learning the physical or natural sciences.

The fact is that technical schools cannot be permanently supported unless we diffuse a taste for science and art. If we create the taste, the technical schools will be well filled. We must introduce the elements of science and art into the primary schools, and we shall soon change the secondary education of the working men.

It too often appears that, from the utterly defective education of the people, they do not know what is good for them, and have not the slightest conception of the methods that should be taken to improve their present ignorant and imperfect condition. In some instances so deplorable is the state of elementary education that it is found impossible to give the working classes the instruction which they desire to receive in the sciences connected with their work. They are not able to read with sufficient facility to master the books put before them; they cannot write well enough to take notes of the lectures which they hear; nor are they sufficiently familiar with arithmetic to make the necessary calculations. Hence it results that one of the first difficulties in promoting technical instruction is the want of fundamental training as the basis of scientific knowledge.

The learned will have to revise the method of teaching. There is a well-founded suspicion that the course commonly pursued has been wrong in principle. The teachers proceeded from generalities, constructed very pretty systems, and dealt largely in refinements. Many people now believe, on the contrary, that we ought to begin with individual instances, then lead the pupil to construct a broad outline, and gradually to fill up the picture as his knowledge advances.

Or take another illustration. If a man works his way up the mountain side he meets with many difficulties, but at length, when he reaches the top, he enjoys a fine prospect all around. Now, if that man wishes to guide others up the mountain, it is not sufficient for him to harangue from the top, or to dilate upon the fine prospect which he enjoys. He must come down again to the valley; he must take others by the hand, and lead them by the way which he took himself, or very nearly by the same way.

Until recently elementary treatises on science were written *from the top of the mountain*. The authors, enjoying an expanded prospect, were disposed to take general views; and to discuss principles which, however interesting to themselves, had little or no interest for the pupil. There was a want of sympathy with the learner. For example, the writers on Geography began with the globe, and expounded the elements of Spherical Trigonometry and Astronomy, talking of *meridians*, *parallels*, the *tropics*, the *equator*, and the *ecliptic*. At present the best teachers of geography *to young children* begin with the place where the pupil lives and dwells; thence they proceed to the surrounding districts, to neighbouring countries, and end with the Globe.

Bacon says that "wherever it is possible knowledge should be *insinuated* into the mind of another in the manner in which it was first discovered." If this principle were fairly carried out it would work great changes in our methods of teaching.

WILLIAM RUSHTON

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## *MOSS LOCHS*

AS these lochs are seldom visited save by sportsmen of either the rod or the gun, it will be necessary for me to give a short description of them. These lochs are generally situated high up, near the tops of the hills, the hills being wholly or in part covered with heather and moss. They are of small size, varying from about a mile to a hundred yards in length; the water is of a dark porter colour. They look as if an immense hole had been dug in the peat, and the hole then filled with water; the banks, which are wholly or in part composed of peat, rising almost perpendicularly out of the water, and at some places extending downwards for many feet under it; at other places going only to a depth of a foot or two, and then extending for some feet in a nearly horizontal direction, when they again dip abruptly to a considerable depth. These abrupt precipices of peat, as seen under the water, are often formed in curious, fantastic shapes, and look more like rock than soft peat; and when seen by the sunshine—broken by the passing waves– through the dusky water, with the surroundings of bleak, bare hill, total silence, save the plaintive cry of some bird passing overhead, and no life, save the lizard and the snake—the whole presents a scene, the weird effect of which on the imagination is seldom if ever exceeded by anything else in nature.

What strikes the observer of these lochs is, that not only are the banks made of peat, but the sides and bottom are wholly or in part made of the same material; and there seems to be no difference between the peat at the bottom of the loch and that on the banks. It looks exactly like as if the peat had begun to be formed at the bottom of the loch, and had gradually extended upwards till it had risen above the water. Yet it could not have done so, because, although water-lilies and some grasses are seen growing under a depth of a foot or two of water, yet all vegetation ceases at a depth of a very few feet. How then came the sides and bottom of these lochs to be formed of peat? There are no signs of any convulsions of nature after the peat had been formed to account for it. If produced by any upheaving of the earth stopping the exit of the water, the upheaving must have been very violent, because many of these lochs are deep and yet of but small size. How, then, came the peat in the position in which we now find it? An examination of the outlet will at once explain the difficulty. The stream which leaves the loch winds its way through mossy ground, the bottom of its channel being covered with water plants. These water plants, as they grow from year to year, are gradually filling up the channel, and so adding to the depth of the loch. It is now easy to understand how peat is found at such depths in these lochs. We will suppose the loch to begin from marshy ground or from a small loch. The channel of the outlet—being covered with water plants—gradually gets filled up, so increasing the depth of the water in the lake, while vegetable life is busy adding peat to the banks. And thus marshy ground or a shallow loch with shelving beach is converted into a deep moss loch with perpendicular sides. The rising of the channel of the outlet and of the sides does not always take place at the same relative rate. In one loch recently visited the peat bank was about eight feet above the water, whilst in another where there was a vigorous growth of water plants in the outlet, the water was within a few inches of being over its banks. That water plants are capable of producing this result will be doubted by none who have seen them fairly establish themselves in a pond, how soon they over-run, and, if left alone, fill it up.

Moss lochs stand in marked contrast to other lochs. In other lochs the water, as it passes from them, has worn their channels, and is year by year wearing them further, so lowering the water in them; whilst in moss lochs the channels are year by year being filled up, so gradually raising the water in them. It may be objected that the water plants in the outlet would be uprooted by the water from the loch during floods; but such is not the case, because in most cases, when the water leaves the loch, it passes through a nearly level channel, so that it never gets up speed sufficient to damage its bed. And besides, these lochs being situated near the tops of the hills, they drain but a small extent of country. In no case visited had any of the lochs a stream of any size running into it, and the amount of water which passed from them was in every case small.

As there are few rules without exceptions, it is possible that the rule that the outlets from moss lochs are covered with water plants may not hold good in every case; it is quite possible that the outlet from a moss loch might be over a rocky channel. If such should happen to be found, it does not necessarily prove that it was not formed in the way shown. The plants might continue to fill up the outlet till the water was raised to such a height that it found a passage over a new channel at a part of the hill where there was no moss and nothing but bare rock. We would thus have a moss loch grown in the way shown, but which had ceased to grow.

JOHN AITKEN

## *WRITERS ON SCIENCE*

AT the recent dinner of the Royal Literary Fund, Sir Henry Anderson proposed the toast of "Writers on Science." We make the following extracts from the reply by Dr. Richardson from the report of the Society:—

"Who are the writers on science? Are they as well known as other great writers? They are not. They are less fortunate, and, therefore, the more worthy of the exceptional honour you would bestow on them. Excuse me a moment or two while I indicate the peculiarities of the position of the writer on science. He is a man communicating to the word that which is, by comparison, new to the world. The poet can cast back for his models to a time when the Greeks had not so much as the figment of an alphabet. The theologian may go back for his lesson to the earliest manifestations of the life of intellect on the planet. The historian finds subject and matter ready for his hand from the oldest and remotest, as well as the newest, writings and traditions of races and peoples. The story-teller is embarrassed with the richness of the past, and troubled by the greed of his admirers for more of his work. These all, indeed, are but the continuing interpreters of things, events, thoughts, which every man who claims to read claims also to understand. The writer on science has none of these advantages; he is but newly born into an old world of thought, and is not simply telling of new wonders, but is often himself learning at the same time as he is instructing an audience unlearned in his knowledge. Thus he comes slowly into the recognised brotherhood of men of letters ; at the best he speaks to but a small audience, amuses rarely, excites sometimes without intention hopes that are delusive, and requires always, in order that he may be fairly understood, a degree of patience it is vain to expect from the multitude. To these difficulties others are added belonging to the work he accomplishes. The most original writers on science are destroyed constantly by the magnitude and overpowering character of the work they have written, and by the practical results that spring from the work. In other literature the book produced lives as the book, and the learner from it, age after age, must go back to the fountain head to drink and drink ; in science literature the book sinks into the fact it proclaims, and the fact remains the exclusive master of the field. A striking example of this flashes across my mind at the present moment. Every reading man and woman knows that in the reign of Queen Elizabeth the book of Shakespeare's plays had its origin, and nearly everyone who has read the book (and who has not?) remembers the curious saying in it, 'I'll put a girdle round the world in forty minutes.' But how many are there who have read another great book of that same reign, entitled 'De Magnete ;' or are aware that at the time when Shakespeare was writing his now-familiar phrases, the author of the book on the Magnet, the Queen's physician, one William Gilbert, when his daily toils of waiting upon the sick were over, was working with his smith in the laboratory at his furnace, needle, and compass, was writing up for the first time the word 'Electricity,' and was actually forging the beginnings of the very instruments that now, in less than forty seconds, put the girdle round the globe? Again, writers on science are lost sometimes in the blaze of their own success. They raise wonder by what they do, and fall beneath it. All knowledge newly born is miracle, but by-and-by, as the knowledge becomes familiar, the miracle ceases. In this way advances in science become part of our lives, while the men who write them down cease to us. When the Leyden jar was first described, Europe was mentally as well as physically convulsed with the thing ; now a Leyden jar is a common object—we all know it ; but how few know of Mr. Cuneus, who first described this instrument of science? The whole civilised world is cognisant in this day that communication from one part of the world to the other, by telegraph, is almost child's play ; but how many have seen or heard of Mr. Cavallo's original Essay on Electricity as a means of communicating intelligence to places distant from each other? There is nothing more commonplace, in our day, than to know that a living human being can be placed in gentle sleep, and, while in blissful oblivion, can have performed on him what were once the tortures of the surgeon's art ; but how few have heard or seen Sir Humphry Davy's paper announcing to mankind this grand beneficence ! These are some of the difficulties of writers on science ; and yet there is another I must name, be it ever so lightly. I refer to the desperate struggles of the man of science who has nothing but science to carry him on in life. None but such as are placed as I am, practising as physicians in the metropolis of the world, and admitted at the same time, as men of science, into some knowledge of the subject upon which I now speak, can form a conception of the almost hopelessness of the position of the pure scholar of science. On this I say no more. I would awaken but not weary your sympathy . . . . much of the difficulty these writers have had to bear I recognise with admiration, as their truest glory ; and I see that hope for better worldly prospects is near. A profession of science is no doubt organising. The world is at last asking men of science to employ themselves in teaching the world ; and the teachers, bending to the labour, are, in their turn, willing to suspect that they are but as children, or at best youths, in the race after knowledge. This is most hopeful ; and it is hopeful also to find that men who claim to be conservators of a knowledge that was matured when science was unborn, are listening now to our scholars with an attentive ear, and are beginning to accept that the Lord of Nature, whether he reveal himself to the ancient law-giver in the burning bush that was not consumed, or to the modern astronomer in the burning glory of the omnipotent sun, is one and the same Lord. Thus there is hope, I may say certainty, in the future for the literature of science ; for its poetry, its parables, its facts, nay, even for its religion."

## *FEARFUL EARTHQUAKE IN CHINA*

THE American Minister in China, General Lowe, has just forwarded to the Secretary of State at Washington the following account of the fearful earthquake which occurred in the Bathang, in the province of Szchuen, on the 11th of April, which he has had translated from the report of the Chinese Governor General of the province in which it occurred :—"Bathang lies on a very elevated spot beyond the province about 200 miles west of Li-Tang, and about thirty post stations from the district town of Ta-tsien, on the high road to Thibet. About eleven o'clock on the morning of the 11th of April, the earth at Bathang trembled so violently that the government offices, temples, granaries, stone houses, storehouses, and fortifications, with all the common dwellings and the temple of Ting-lin, were at once overthrown and ruined ; the only exception was the hall in the temple grounds, called Ta-Chao, which stood unharmed in its isolation. A few of the troops and people escaped, but most of the inmates were crushed and killed under the falling timber and stone. Flames also suddenly burst out in four places, which strong winds drove about until the heavens were darkened with the smoke, and their roaring was mingled with the lamentations of the distressed people. On the 16th the flames were beaten down, but the rumbling noises were still heard under ground like distant thunder, as the earth rocked and rolled like a ship in a storm. The multiplied miseries of the afflicted inhabitants were increased by a thousand fears, but in about ten days matters began to grow quiet, and the motion of the earth to cease. The grain collector at Bathang says that for several days before the earthquake the water had overflowed the dykes, but after that the earth cracked in many places, and black, fœtid water spurted out in a furious manner. If one poked the earth the spurting instantly followed, just as is the case with the salt wells and fire wells in the eastern part of the province ; and this explains how it happened that fire followed the earthquake in Bathang. As nearly as can be ascertained there were destroyed two large temples, the offices of the collector of grain tax, the local magistrates' offices, the Ting-lin temple, and nearly 700 fathoms of wall around it, and 351 rooms in all inside ; six smaller temples, numbering 221 rooms, besides 1849 rooms and houses of the common people. The number of people killed by the crash, including the soldiers, was 2,298, among whom were the local magistrate and his second in office. The earthquake extended from Bathang eastward to Pang-Chahemuth, westward to Nan-Tun, on the south to Lintsah-shih, and on the north to the salt wells to Atimtoz, a circuit of over 400 miles. It occurred simultaneously over the whole of this region. In some places steep hills split and sunk into deep chasms, in others mounds on level plains became precipitous cliffs, and the roads and highways were rendered impassable by obstructions. The people were beggared and scattered like autumn leaves, and this calamity to the people of Bathang and the vicinity was really one of the most distressing and destructive that has ever occurred in China."

## *ON THE STRUCTURE OF THE EEL'S SKULL*

THE skull of the Eel is much less specialised than that of most other Osseous (*Teleostean*) fishes. I was made aware of this many years since whilst preparing skeletons of the common kind (*Anguilla acutirostris*), and of the conger (*Muræna conger*). Afterwards, when Prof. Huxley's "Croonian Lectures" (Proc. Roy. Soc.) came into my hands, the importance of the aberrant structures of this type of skull was shown to me; and since that time I have been on the watch for further opportunities for dissecting and working out both this type, and also that of the Amphibia, which it serves to illustrate. In a few weeks' time I shall be able to make myself understood with regard to those morphological changes which take place in the vertebrate skull as it passes from a low Ichthyic into the higher Amphibian type. This will be done by the illustration and description of the frog's skull in the forthcoming part of the "Philosophical Transactions," an abstract of which paper has already appeared in these columns. At present the nomenclature of the parts of the cranium and face of the fish is in a state of painful confusion. I shall not, however, trouble the student with confusing references, but continue to use those terms which he will find in my other morphological papers. I may, however, remark that these differ in some instances from those used by Professor Huxley, for instance, his "squamosal" is my "pterotic" (see "Elem. Comp. Anat.," p. 188). This is a bone called "mastoid" by Cuvier, and this term was adopted by Prof. Owen. These anatomists came much nearer the truth than my friend; but the bone only represents *part* of the human "mastoid"—its antero-superior region. Again, the terms for the palato-pterygoid arcade are very confusing; Cuvier's "internal pterygoid," also called ento-pterygoid by Owen and Huxley, does not correspond to the internal pterygoid plate of man and the mammalia generally, but to a third piece, which I call meso-pterygoid, and which occurs in a young pig's and in a young fox's skull in my collection; I have also found it in the palate of all sorts of birds, except the fowls and Struthionidæ. The true representative of the human internal pterygoid is, in fishes, called "transverse" by Cuvier; most correctly the "pterygoid" by Owen; confusingly the "ecto-pterygoid" by Huxley. I drop the frequently misapplied terms, "ecto-" and "ento-pterygoid," altogether, and call the true "transverse bone" of the reptile—never seen in fishes—the "transpalatine." Most of the other terms used by me agree with those used by Prof. Huxley in his "Elements."

There is, however, in the hyoid arch one segment which requires its name,—that given to it by Prof. Owen—to be changed; I refer to that lump of cartilage which becomes segmented off from the lower part of the hyoid cornu by a joint cavity, and which has two ossific centres. This has been called the "basi-hyal;" but it is merely a distal and not a basal bone, the key-stone being the "glosso-hyal," which passes into the basi-branchial bar. I would call it the "hypo-hyal," as it is the manifest "serial homologue" of the "hypo-branchials." All these things I hope soon to make plain in a paper now in hand, on the "Structure and Development of the Salmon's Skull." My materials at hand, from which I have studied the eel's skull, are the adult conger's skull, that of a small *Muræna* (? species), and the heads of large and small common eels. The smallest of these are the gift of Mr. F. Buckland; they measure 2 inches 8 lines in length. The cranium of the eel is long, triangular, and depressed, the nasal region being very pinched and narrow, whilst the occipital is expanded, and sends out over-hanging outgrowths, — backwardly projecting crests, which are continuous in the conger, but distinct spurs in the eel.

These crests in the eel are formed by the super-occipital at the mid-line; then a pair from the epiotics; and below, and external to these a bilobate pair, belonging largely to the pterotics, but also to the ex-occipitals.

The flat top of the skull, up to the exit of the fifth nerve, is square, the top of the cranium then narrows suddenly to half the breadth of the square part.

On each side, there is, in the broad part of the skull, a large overhanging eave, below which is the double recess which forms the glenoid cavities for the hyo-mandibular. If the large parietals which meet at the mid-line were removed, we should see the "great upper fontanelle," bounded behind by the perfected occipital arch, and laterally by the cranial and auditory elements. Indeed, the term cranio-auditory elements would be a correct term for several of these bones, the auditory capsule coalescing very early with the rising crests of the investing mass, and the subsequent ossifications enclosing both the sense-capsule and the membranous cranium. Behind, the expanded occipital region is largely indebted to the "epiotics," and "pterotics," two pairs of which bones are really primarily related to the cartilaginous auditory sac. There is no opisthotic, and the large "pro-otic" is surmounted by a part of the posterior sphenoid, which is to be found in the bird, but not in the reptile or mammal. I allude to the post-frontal, a great outstanding projection from the "ala magna," a crested, fore-turned, supero-lateral element of the primordial skull. In front of the "foramen ovale," the "ali sphenoids" wall-in the skull; they are unusually large for an osseous fish; they rest upon an inverted "saddle" of bone, with a free fore-edge. This is the fish's basi-sphenoid, and corresponds to the pre-pituitary part of the human basi-sphenoid, and to its anterior clinoid region. In high-skulled fishes this bone is Y-shaped, the descent of its long crus showing that the "meso-cephalic flexure" of the embryo is never wholly recovered from in these fishes; and its slender size showing that the connective band which brought the investing mass into union with the "trabeculæ cranii," was a feeble strip of cartilage. Behind the saddled-shaped basi-sphenoid of the eel is the open pituitary space, which, as in birds, is merely closed below by the ossification of sub-mucous fibrous tissue, in the form of the para-sphenoid. The large basi-occipital, which encloses all the retiring notochord that belongs to the skull, helps to form an elegant tri-radiate synchondrosis in the floor of the skull; for all that part of the "investing mass" from which the notochord had retired, is invested, not by a basi-sphenoidal ossification, but by the huge "pro-otics" which meet at the mid-line, behind the open pituitary space. The structures of the skull that have morphological continuity with the vertebral column cease behind the optic nerves, and even the parts surrounding the pituitary body are of a secondary or connective character, bringing the true cranial structures into fusion with parts derived from the first or pre-stomal facial arch, the trabeculæ cranii. Now there comes in a most important condition of the skull of the eel; for the anterior sphenoidal region has no cartilaginous walls whatever; the roof is formed by the narrow frontals (frontal in the conger); the side walls are membranous, and the floor is that sub-mucous bone the para-sphenoid. In young eels, 5 in. long, the trabeculæ may be traced to their union with the "investing mass" in the pre-pituitary region; but there they unite with each other, and in the anterior sphenoidal region, instead of turning upwards to form a skull floor, they grow downwards, investing the convex upper face of the para-sphenoid (see Fig. B). Over the optic region the *pterotics* overlap, in the conger they nearly reach as far as the hinder end of the bony ethmoid; and here the frontal sends out a few post-orbital snags, and sends downwards on each side a thick post-orbital process, which articulates with the ali-sphenoid. At this part the narrow skull bends downwards in a Roman-nosed manner. The solid nasal region in front is of equal length with the long membranous interorbital space; these are separated by the large, thick, ear-shaped unossified pre-frontals, or lateral ethmoids. The median ethmoid is ossified entirely by the thick, bony bar, which commences as a knife-shaped vertical plate, or parostosis; here in the eel as in the Amphibia, the distinction between parostosis and endostosis at times breaks down. The long tooth-bearing vomer splits the parasphenoid with its long style, as far back as the pituitary space; it coalesces with the ethmoid when the eel is five or six inches in length. Where the bony ethmoid and vomer unite there is a groove; along this the olfactory crus runs, protected outside by the grooved, soft, lateral, ethmoidal wing, which arose as an outgrowth of the trabecular bar; the "cornua" of the trabeculæ (Fig. C) persist as filiform prolongations, continuous with the lateral ethmoids behind, and end in blunt points near the fore part of the ethmo-vomerine bony mass. In the conger, but not in the eel, the vomer sends out a wing on each side for the lateral ethmoids to rest upon. The parasphenoid is very deeply split at both ends, both for the vomer and the basi-occipital; it has large wings in the basi-temporal region, which underlie, in a squamose manner, the lower edge of the prootics. These latter bones divide the foramen ovale; behind and below the posterior opening there is a small passage evidently the distinct foramen for the "portio dura." The vomer and parasphenoid are azygous splints applied to the under surface of the coalesced and metamorphosed trabecular bars.

When the membranous cranium dips downwards in front (mesocephalic flexure) then the trabeculæ are not only parallel with the base of the first cerebral vesicle, but also nearly so with their immediate successors, the mandibular bars; whilst thus contiguous they form a *secondary connection*, which, of course, lengthens as the trabeculæ ascend with the cranial sac, and thus enlarge the mouth cavity. This bar is well chondrified in fishes

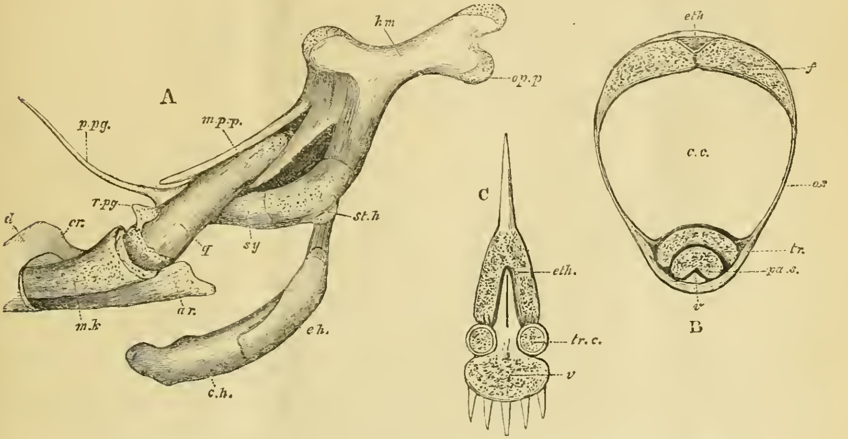



FIG. A.—INNER VIEW OF MANDIBULAR AND HYOID ARCHES OF A YOUNG EEL, 2⅞ inches long: *r.pg.* rudiment of cartilaginous pterygoid; *p.pg.* pterygo-palatine; *q.* quadratum; *ar.* articulare; *d.* dentary; *cr.* coronoid; *mk.* Meckel's cartilage; *hm.* hyo mandibular; *sy.* symplectic; *st.h.* stylo-hyal; *e.h.* epi-hyal; *c.h.* cerato-hyal; *op.p.* opercular process; *mp.p.* metapterygoid process.

FIG. B.—SECTION OF ANTERIOR SPHENOIDAL REGION IN A YOUNG EEL, 5 inches long: *eth.* ethnoid; *f.* frontal; *o.s.* orbito-sphenoidal region: *tr.* trabeculæ: *pa.s.* parasphenoid; *v.* vomer; *c. c.* cranial cavity.

FIG. C.—SECTION THROUGH THE NASAL REGION OF THE SKULL OF A YOUNG EEL, 5 inches long: *eth.* ethnoid; *v.* vomer; *tr. c.* trabecular cornu.

*All the figures are magnified about 25 diameters.*

generally, and in the tailless Amphibia. In the tailed Amphibia it is abortively developed, and no solid hyaline cartilage is found in this part in Sauropsida and Mammals. (See "On Skull of Fowl," Phil. Trans., 1869, pl. 81, figs. 1, 3, 6, 10, and 11, p. 767.)

The eel also has no solid cartilage in this bar, save a slight rudiment behind, as in the "Urodela" (Fig. A), and the three ectostal plates that invest the large cartilaginous bar in most osseous fishes—the palatine, meso-pterygoid, and pterygoid—are represented by a needle-like, solid style of bone, pointed in front, and pedate behind where it attaches itself to the inside of the front edge of the quadrate. In old eels this style becomes a flattened bar, articulating by a squamose suture with the quadrate, and loosely attached to the lateral ethmoid and the maxillary in front. This bone is the counterpart of the single plate in the Lepidosiren's mouth (see Huxley's Elem., Figs. 84 and 85, D, pp. 208, 209); but the pterygo-palatine of that fish is applied to a thick cartilaginous connective that fills in the whole sub-ocular region. As in the Lepidosiren and the Amphibia, tailed and tailless, the eel has only one ossification on the pier of the mandibular arch, and the generalised nature of the fish is shown in the partial coalescence which takes place between this and the succeeding (hyoid) pier. In the Lepidosiren, as in the Chimæra, the coalescence is entire between all but the free segmented rays of the first and second post-stomal arches; in the Urodelæ we have a similar state of things, but in the Anoura coalescence only takes place in the lower half of the pier. In all these it is cartilaginous confluence, but in the eel it is merely the anchylosis of the bony symlectic (Fig. A, *sy.*, *q.*) with the quadrate. Although there is no metapterygoid perched upon the quadrate, yet that element sends upwards a metapterygoid process which runs between and within two large denticulations of the hyo-mandibular. This latter bone (*hm.*) is very massive, and being most strongly united both by synchondrosis and deeply serrated suture to the quadrate, the suspensorium of the eel is exceedingly strong, quite as strong as, and more elastic than, the quadrate of the Sauropsida. It forms an acute angle with the basi-cranial line, as in most other fishes, but in *Muræna helena* (see Huxley, Croon. Lect., p. 34, and Osteol. Catal. Mus. Coll. Surg., vol. i., p. 14), the suspensorium is very frog-like, forming an obtuse angle. The well-developed heads of the hyo-mandibular fit into proper glenoid cavities, the foremost of which is made in the post-frontal and pro-otic, and the hinder pit is in the pterotic. The knob for the opercular is very large in the adult, and in eels five or six inches long the only sign of the separateness of the symplectic is the transverse cartilaginous tract which connects it with the hyo-mandibular; but in my youngest specimen it can be seen separate, with its own ectosteal sheath (Fig. A, *sy.*); it is very short, and the cartilaginous interspace above is very large. From the middle of that synchondrosis there arises a small semi-segmented bud of cartilage, the "stylo-hyal" (*st. h.*); this becomes ligamentous in the adult; in other Teleostei it forms a rather small cylinder, completely segmented off, and it acquires a bony sheath. The rest of the descending hyoid cornu is a thickish arcuate rod of cartilage ossified by two ectosteal sheaths, the "epi"- and "cerato-hyals." In Teleostei generally, the distal end is cut off by a joint cavity, and ossifies from two more ossicles, forming the "hypo-hyal" segment; this structure is not attained in the less specialised eel. The arch is finished by a long and stout glosso-hyal. Even the free bar of the first post-stomal arch—the mandible—has its peculiarities, for, contrary to rule, it has no angular splint—and the "coronoid," so seldom present in Teleostei, and so constant in Sauropsida, is well developed in the eel; it is very small in old individuals of the cod-fish. The dentary alone is dentigerous, and is very large and strong, with a large coronoid process; the "articulare" is short and massive. In the upper part of the face the specialised subcutaneous bones (*parostoses*) are very instructive; several belong to the lateral-line series, but, modified and broken up into two rows in the head, they form tubular mucous bones; these are the nasals and "sub"- and "pre-orbitals." Another facial series, which may run obliquely from the snout to the hinge of the lower jaw, has only two on each side,—the pre-maxillary and maxillary. Here we have, contrary to rule, the short pre-maxillary edentulous and the maxillary bearing teeth. The specialised bones of the back-face and throat are worth mention; the pre-opercular is oblong, twisted, strongly convexo-concave, and burrowed by mucous glands. The opercular fits by a deep cup to the knob on the hyo-mandibular; like the feebler sub-opercular, it is strongly falcate; the latter fits by a sliding joint to the pedate upper end of the wedge-shaped large inter-opercular. The long, thick-based, slender-pointed "branchiostegals" are eleven in number on each side in the common eel; the basal bone of this wondrously specialised series of dermal bones is the so-called "uro-hyal;" it is knife-shaped behind, and in front terminates in a massive head, facetted for the cerato-hyals. I call this bone the "basi-branchiostegal;" for the "uro-hyal" of the bird is the remnant of the basi-branchial bar. The student can easily obtain both the gigantic conger and the larger specimens of the common eel, and, having become familiar with the parts of the skull and face of such an ordinary teleostean as the cod, and of the larger amphibian types, both tailed and tailless, he will then be able to gain a much clearer idea of the fundamental harmony existing between such diverse types, if this intermediate eel-type be once well understood.

The development of the skull in the culminating amphibian, the frog, has yielded me already such satisfactory results that I am somewhat restless to know the early conditions of that of the fish: then whole groups of low vertebrate types will begin to be seen in harmonious relation.

W. K. PARKER

## NOTES

THE following is a list of the Presidents of Sections nominated by the Council of the British Association for the approaching meeting at Edinburgh:—Section A, Prof. P. G. Tait, of Edinburgh; Section B, Dr. Andrews, of Belfast; Section C, Prof. Geikie; Section D, Prof. Allen Thomson; Section E, Alex. Keith Johnston, sen.; Section F, Lord Neaves; Section G, Prof. Fleeming Jenkin. The Evening Discourses will be delivered by Prof. Abel and Mr. E. B. Tylor.

IT is stated that the labours of the Royal Commission on Coal, appointed a few years ago by Sir George Grey, are on the point of completion, and the result is the demonstration of the fact that, assuming a certain annual increase in the rate of consumption, sufficient economically gettable coal exists in Great Britain and Ireland to last from 800 to 1,000 years. We shall be very glad to see such an important fact demonstrated.

WE have to record the death of Mr. George Grote, Vice-Chancellor of the University of London, whose serious illness we mentioned a fortnight since. He died on Sunday last, after a long illness, in his seventy-seventh year. We can ill afford to lose men who have so long and so ably thrown their influence and their abilities into the cause of the higher education of all classes of the community.

WE regret to announce that Mr. Numa Edward Hartog, Senior Wrangler of the University of Cambridge in 1869, died on Monday last of smallpox. Mr. Hartog was still, in common with other Nonconformists, excluded from the substantial reward of his exertions; but in the present Session he gave important evidence before the Lords' Committee on University Tests, and it is due perhaps to the sympathy which his exclusion excited that the Lords proposed a measure which would have admitted him to a Trinity Fellowship. Before, however, he could take advantage of the passing of the University Tests Bill the man who was expected to be the first to reap its fruits had passed away.

AT the recent examination for the newly-established Diploma in State Medicine given by the University of Dublin, the first place was taken by Mr. J. W. Moore, ex-scholar Trinity College, Dublin; the second by Dr. A. W. Foot, Junior Physician to the Meath Hospital and County of Dublin Infirmary; the third by Mr. Yeo, who obtained the Junior Medical Exhibition in 1864, and the Senior Medical Exhibition in 1866; and the fourth by Mr. Todhunter, a gentleman already well known in certain circles for his literary abilities.

THE new museum and library at Clifton College were inaugurated on Saturday last by a *conversazione*. There was a good collection of objects of interest contributed by gentlemen of the neighbourhood; some music, and a speech from the Rev. Principal of the College, interested the large company, and Prof. Church delivered an address on "Colour."

THE new buildings of St. Thomas's Hospital on the southern Thames Embankment, opposite the Houses of Parliament, were opened yesterday by the Queen in person.

THE Victoria Institute concluded its fifth session on Monday. Its members are now 305 in number, seventy having joined since February; the papers for the coming session include two on subjects connected with the vegetable kingdom.

THE managers of the London Institution, in accordance with the recommendation of the annual meeting of proprietors, have resolved to afford opportunities during the ensuing season for the reading and discussion of papers on subjects of special interest in science, literature, commerce, and the arts, provided they receive such offers as will insure a succession of suitable communications. It is believed that this proposed extension of the use of the Lecture Theatre in Finsbury Circus will produce a series of attractive meetings similar in character to those of the Society of

Arts, but representing more directly the business and thought of the City. The managers do not intend to restrict the reading and discussion of papers to the proprietors of the Institution, or to limit the range of subjects otherwise than by the provisions of the charter, which precludes politics and theology.

ON Saturday last, the 17th, the Rugby School Natural History Society made an excursion through Charnwood Forest. Mr. Hambly, the manager of the Mount Sorrel granite works, conducted them over his workshops and quarries; and Mr. Ellis showed them his slate pits at Swithland. They also visited Woodhouse Eaves, the Beacon, the Monastery, and Bardon Hill. The geologists, botanists, and entomologists were alike well content with the results of a very pleasant day's excursion. The party numbered forty-one.

SIR JOHN PAKINGTON, as President of the Institution of Naval Architects, has addressed a letter to the President of the Board of Trade, in which, among other suggestions, he proposes as an additional clause in the "Prevention of Accidents Act" that in future adjusters of compasses shall be duly certified by the Board of Trade, after examination, as properly qualified.

WE are requested to state that the value of the Natural Science scholarship at Magdalen College, Oxford, will be 95*l.*, and not 80*l.* as stated last week, and that the name of the successful competitor for the Johnson Memorial Prize Essay is John G. Gamble, not James S. Gamble.

DR. MURCHISON, F.R.S., has been this week recommended by the Grand Committee for election by the Governors as Physician to St. Thomas's Hospital, Mr. Croft for election as Surgeon, Dr. John Harley and Dr. Frank Payne as Assistant Physicians, and Mr. Francis Mason and Mr. Henry Arnott as Assistant Surgeons.

DR. HOOKER reports that the upper valleys of the Atlas range are very steep and picturesque, and are thickly inhabited by a fine race of people called Shelloos. The first positive indication of ancient ice action met with was a stupendous moraine at about 6,000 feet—a perfectly unmistakeable one, but, curiously enough, with no traces above or below it, no *roches moutonnées*, no striated or grooved surfaces, and no perched blocks, except on the moraine itself. The height of the peaks of the axis is very uniform for a considerable distance, and they have very steep faces; there are no glaciers nor perpetual snow, properly so called; but snow lies all the year in steep gullies of the north face, stretching downwards for probably 5,000 feet from the summit. The vegetation is chiefly Spanish.

THE following works on Science are amongst the publishers' announcements for the next few weeks:—From Messrs. Longman—Dr. Ueberweg's "System of Logic and History of Logical Doctrines," translated by Thos. M. Lindsay; "Cooper's Dictionary of Practical Surgery and Encyclopædia of Surgical Science," new edition by S. A. Lane; in Gleig's School Series: "Animal Physiology," by Dr. E. D. Mapother; "Physical Geography," by W. Hughes. From Mr. Murray—"Rambles among the Alps, 1860—1869," by E. Whymper. From Griffith and Farran—"The Theory and Practice of the Metric System of Weights and Measures," by Prof. Leone Levi, F.S.A.; "A Compendious Grammar and Philological Handbook of the English Language," by J. Stuart Colquhoun, M.A., barrister-at-law. From W. and R. Chambers—"Class Book of Science and Literature; Zoology from do.; Botany from do.; Geology from do."; "Standard Animal Physiology," Part I. for Standard IV.; "Standard Geography," Part I. for Standard IV.; "Standard Physical Geography," for Standards IV., V., VI.; "Mackay's Arithmetical Exercises," for Standard Work, Parts I., II., III., IV.; Part V., embracing Metric System; "Standard Algebra;" "Explicit Euclid," Books I. and II. From S. Low, Son, and Co.—a complete treatise on the "Distillation and Preparation of Alcoholic Liquors," translated from the French of M. Duplais, by Dr. M. McKennie; a treatise on "The Manufacture of Vinegar," by Prof. H. Dussance. From Cassell, Petter, and Galpin—"Selected Obstetrical and Gynæcological Works of Sir J. Y. Simpson," edited by Dr. J. Watt Black; "Model Drawing," by Ellis A. Davidson, being the new volume of Cassell's Technical Manuals, with numerous illustrations and drawing copies; the "Technical History of Commerce," by Dr. Yeats, LL.D.; the "Natural History of Commerce" (second edition), by Dr. Yeats, LL.D.

WE reprint the following sentence from the recently published address of the President of the Tyneside Naturalists' Field Club, commending it to the notice of similar institutions throughout the country now that the season for excursions is commencing:—"We have no law excluding ladies from our club, but yet we have no lady members. Ladies, however, sometimes attend our meetings, and it would, I think, be an advantage to the club (may I hint also that it might be an advantage to the ladies?) if more of them came, and oftener. It is of infinite importance that mothers should be able to impart to their children an intelligent interest in Nature. They cannot do this unless they first possess that interest themselves, and in what way can it be more pleasantly developed and refreshed than by meetings such as ours? It may perhaps be objected that the length and occasionally the rugged character of our walks prove an obstacle to the presence of the weaker sex; but my impression is that this is not the case to any very serious extent, and in many of our excursions ladies have proved themseves quite equal to walks as long and as arduous as are at all desirable for our purposes. I would therefore recommend—not any new rule, which is needless—but simply that we should persuade our lady friends to join the club as members, and not as only casual visitors."

WE have received the prospectus of a proposed American *Archæological Review and Historical Register*, devoted to Archæology, Anthropology, and History, to be devoted to the rapidly increased interest displayed in these subjects in America, and designed not to meet the wants of men of science only, but of all interested in the Origin and Antiquity of Man. Its contents will include original contributions, the reports of learned societies in America and abroad, and a department of "Notes and Queries." The *Review* is intended to be published either monthly or quarterly in New York, and will be edited by Dr. Wills de Hass.

WE learn from the *American Naturalist* that Messrs. J. A. Allen and Richard Bliss, jun., of the Museum of Comparative Zoology at Cambridge, Mass., with Mr. C. W. Bennett, of Holyoke, Mass., started late in April on a six months' collecting trip to the Plains and the Rocky Mountains. The primary object of the expedition is to collect the larger mammals of the West.

THE Ohio Legislature has appropriated 21,000 dols. for continuing the survey of that State, and 18,000 dols. for publication of the results. This survey is under the direction of Prof. Newberry himself; and his corps, which has been employed for some time, will be increased by Prof. J. T. Hodge, Prof. J. H. Stevenson, and others, for the purpose of more speedily finishing the work.

MANY scientific societies have been desirous of taking advantage of the International Exhibition and of the Albert Hall to hold meetings in connection with the Exhibition, and to bestow attention on scientific visitors. The small theatres have, however, been occupied by specimens exhibited, and the Albert Hall is considered too large.

A BRILLIANT meteor of unusual form was seen at Panama on the morning of May 1 at half-past two. It was due south and about 30° above the horizon. It was of the form of a darting flame, parallel to the earth's surface from east to west. The head was of dazzling whiteness, the middle bright yellow, and the tail violet. It ended in a train of brilliant sparks of about 2° in length, and was visible about two minutes. The whole sky was of a rosy colour, and particularly in the east. The same tinge was visible in the evening at half-past seven.

A SCIENTIFIC sanitary question has arisen in India. On the ground of necessity, public offices have been supplied with anti-thermic arrangements; but the economical fit, still strong, has led to a government decree that it cannot afford such provision, and that kuskus windows and their essentials must be provided at the expense of the officials. This will afford an additional pressure on the agitation for transferring the public departments to the English towns, sanitaria, or tea plantations in the hills.

IT is stated that Assurance Companies in India have declined to accept the lives of the officers of the Geological Department there on account of the exposure to which they are subjected.

A UNITED Service Institution for India has been formed, and it is gratifying to observe that it is to be established at Simla in the Himalayas, in a healthy district instead of an unhealthy place.

THE severe earthquake of the 25th of February in Chile has called attention to the views of Mr. Darwin and Prof. Rudolph Falb of Prague. Mr. Darwin was in Chile in the great earthquake of February 20, 1835. It is observed that the recent earthquake began at the same time, 11.30 A.M. Mr. Darwin considered that the space from under which the volcanic matter was erupted in Chile was 720 miles in one line and 400 in another, and that the existence was indicated by a subterranean table of lava of the area of the Black Sea. Prof. Falb maintained that the influence of the moon is the chief cause of earthquakes, and in a letter to NATURE of the 14th of April, 1870, he explained and defended his doctrine, and referred to the earthquakes of Manilla, the volcano of Puraco in Columbia, and convulsions in Peru. His prophecies of a great earthquake in Peru, which occasioned so much alarm, were not realised. The Manilla earthquake, he says, took place two hours and a half after the culmination of the moon. It is affirmed that the late earthquake in Chile had no relation to the culmination of the moon. It is to be noted that the great earthquake in Honolulu in the Hawaian Islands took place on the night of February 19, six days before that of Chile.

AN earthquake was felt at Rawul Pindee and Murree, in the Himalayas, in April.

THE Russian Government are believed to be organising an expedition to New Guinea for the purposes of scientific research and exploration. It is, however, believed in Australia that this is only an indirect method of obtaining a foothold in that country, and it is proposed that the Government of Victoria should send an expedition to New Guinea, in order to obtain by treaty certain portions of territory for purposes of settlement. Should this design be carried into effect, it is to be hoped that every facility will be given to Naturalists to accompany the expedition into this large and comparatively unknown country.

THE *Friend of India* states that from the report on the general state of the weather in the North-West Provinces and Oudh during March, it appears that the direction of the wind, as in the preceding two months, was for nearly the whole month from the north-west in the N.W. portion of the provinces, and west elsewhere. During the first half of the month a tendency to change to the east was occasionally perceptible, and this was especially the case during the time of the barometric depression from the 13th to the 20th. The month as a whole was much drier than usual.

## *MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY*

(*Continued from page* 114)

IN geographical biology Denmark proper is of no great importance except as a connecting link, on the one hand, between the Scandinavian peninsula and Central Europe, and, on the other, as the separating barrier between the Baltic and the North Seas. Low and flat, without any great variety in its physical features, it is unfavourable for the production or maintenance of endemic organisms, and forms an inseparable portion of the region of Central Europe. But the Arctic possessions included in the kingdom, Greenland, Iceland, and the Faroe Islands, are of great interest; and Denmark itself is remarkable for the number of eminent naturalists, zoologists as well as botanists, produced by so small a state. Its reputation in this respect, established by the great names mentioned in my review of Transactions in my Address of 1865, is being well kept up by Bergh, Krabbe, Lütken, Mörch, Reinhardt, Schiödte, Steenstrup, and others in zoology; whilst Lange, Œrsted, and Warming are among the few who now devote themselves more or less to systematic botany. Their general zoological collection, when I last visited it, many years since, was not extensive, although rich in northern animals, and very well arranged under the direction of Steenstrup, and the insects in the Storm Gade Museum were very numerous; whilst at the University was deposited the typical collection of Fabricius. The Herbarium at the Botanic Garden, valuable for the types of Vahl and other early botanists, has been in modern times enriched by the extensive Mexican collection of Liebmann, the Brazilian ones of Lund and others; whilst Œrsted's Central-American and Warming's Brazilian plants are also at Copenhagen, but whether public or private property I know not. The botanical and zoological gardens are of no great importance, but the biological publications are kept up with some spirit, especially the Transactions of the Royal Society of Science, Schiödte's continuation of Kröyer's "Tidsskrift," and the "Videnskabelige Meddelelser" of the Natural History Society; and some of the authors have adopted a practice strongly recommended to those who write in languages not understood by the great mass of modern naturalists, that of giving short *résumés* of their papers in French. On the most important contributions to systematic zoology since those mentioned in my address of 1868, I have received the following memoranda:—Prof. Reinhardt, in publishing in the Transactions of the Royal Danish Academy (1869) nine posthumous plates, executed under the direction of the late Prof. Eschricht, illustrating the structure of various cetacea, has accompanied them with short explanations. Prof. Reinhardt has further published, in the "Videnskabelige Meddelelser" for 1870, a list of the birds inhabiting the Campos district of central Brazil; notes on the distribution, habits, and synonymy are copiously added; and the introductory remarks on the geographical distribution, &c., are very suggestive, and ought to be translated for the benefit of the friends of ornithology in England and elsewhere. The same "Videnskabelige Meddelelser" contains an essay by Dr. Lütken on the limits and classification of ganoid fishes, chiefly from a palæontological point of view, accompanied by a synopsis of the present condition, in systematical and geological respects, of that important branch of palæichthyology. In Mollusca, Dr. Bergh has published, in Kröyer's "Tidsskrift" for 1869, one of his elaborate, anatomical, and systematic monographs of the tribe Phillideæ, with many plates, of which a detailed notice is given in the "Zoological Record," vol. vi. p. 559. In insects, Prof. Schiödte, in the same journal for 1869, has given an elaborate essay containing new facts and views on the morphology and system of the Rhynchota, analysed in the "Zoological Record," vol. vi. p. 475. To Dr. Krabbe we owe the description of 123 species of tapeworms found in birds, an elaborate monograph accompanied by ten plates, and printed in the Transactions of the Royal Danish Society for 1869, with a French *résumé*. (Noticed in "Zoological Record," vol. vi. p. 633.) In Echinoderms, Dr. Lütken's valuable essays on various genera and species of Ophiuridæ, recent and fossil, with a Latin synopsis of Ophiuridæ and Euryalidæ, and a general French *résumé*, forming the third part of his "Additamenta ad Historiam Ophiuridarum," in the Transactions of the Royal Danish Society for 1869, have been analysed in the "Zoological Record," vol. vi. pp. 369, 462, &c. No contribution to systematic botany of much importance has appeared in Denmark since those mentioned in my Address of 1868.

There exists no general Danish Fauna; but I have a rather long list of detached works and essays from which the Danish inhabitants of the different classes of animals may be collected. Of these the most recent are Collin's Batrachia, in Kröyer's "Tidsskrift" for 1870, and Mörch's marine Mollusca, publishing in the "Videnskabelige Meddelelser" for the present year.

With regard to Iceland, the only works mentioned are Steenstrup's terrestrial mammals, or rather mammal, of Iceland, in the "Videnskabelige Meddelelser" for 1867; and Mörch's Mollusca in the same journal for 1868. C. Müller's account of the birds of Iceland and the Faroe islands dates from 1862, and Lütken's of the Echinoderms from 1857, and I find no mention of any special account of the insects of the island; whilst in botany, C. C. Babington has given us, in the eleventh volume of our Linnean journal, an excellent revision of its flora, the phænogamic portion of which may now be considered as having been very fairly investigated; and E. Rostrup, in the fourth volume of the Tidsskrift of the Botanical Society of Copenhagen, has enumerated the plants of the Faroe Islands.

The Scandinavian peninsula is, on several accounts, of great interest to the biologist. It includes a lofty and extensive mountain-tract, with a climate less severe than that of most parts of the northern belt a similar latitudes, and the uniformity of the geological formation is broken by the limestone districts of Scania. It thus forms a great centre of preservation for organic races beween the wide-spread tracts of desolation to the east and the ocean on the west, and has therefore been treated as a centre of creation, whence a Scandinavian flora and fauna has spread in various directions. As the home of Linnæus it may also be considered as classical ground for systematic biology, the pursuit of which is now being carried on with spirit, as evidenced by such names as Holmgren, Kinberg, Liljeborg, Malm, Malmgren, G. O. Sars, Stal, Toreil, and others in zoology; and Agardh, Andersson, Areschoug, Fries, Hartmann, and others in botany. Two of the academies to whose publications Linnæus contributed, those of Upsala and Stockholm, continue to issue their Transactions and Proceedings; and to these are now added the memoirs published by the University of Lund. They lost Linnæus's own collections, and the Zoological Museum at Upsala, when I saw it many years since, was poor, that of Stockholm better, and in excellent order. In the herbaria, Thunberg's and Afzelius's collections are deposited at Upsala, and Swartz's at Stockholm, where the herbarium of the Academy of Sciences has been of late years considerably increased under the care of Dr. Andersson.

The Scandinavian Fauna and Flora have been generally well investigated. The numerous Floras published of late years show considerable attention on the part of the general public. I observe that Hartmann's Handbook is at its tenth edition; Andersson has published 500 woodcut figures of the commoner plants, taken chiefly from Fitch's illustrations of my British Handbook; and my lists contain many papers on Swedish Cryptogams. The relation of the Scandinavian vegetation to that of other countries has also been specially treated of by Zetterstedt, who compared it with that of the Pyrenees, and by Areschoug, Andersson, Ch. Martins, and others, as alluded to in more detail in my Address of 1869. Many works have succeeded each other on the Vertebrate Fauna since the days of Linnæus; amongst which those of Liljeborg as to Vertebrata in general and of Sundeyall as to Birds are still in progress. The Crustacea, Mollusca, and lower animals have been the subjects of numerous papers, the marine and freshwater faunas having been more especially investigated by the late M. Sars and by G. O. Sars; and Th. Thorell, in the Upsala Transactions, has given an elaborate review of the European genera of spiders, evidently a work of great care, preceded by apposite remarks on their generic classification, and a general comparison of the Arachnoid faunæ of Scandinavia and Britain, all in the English language although published in Sweden. This work, however, does not extend to species, beyond naming a type (by which I trust is meant an example, not the type) of each genus; nor is the geographical range of the several genera given. There appears to be no general work on Scandinavian insects

The fauna and flora of Spitzbergen have specially occupied Swedish naturalists. To the accounts of the Vertebrata by Malmgren, and of the Lichens by T. M. Fries, have now been added, in recent parts of the Transactions or Proceedings of the Royal Swedish Academy, the Insects by Holmgren, the Mollusca by Mörch, the Phænogamic Flora by T. M. Fries, and the Algæ by Agardh.

An excellent and elaborate monograph of a small but widely spread genus of Plants, entitled "Prodromus Monographiæ Georum," by N. J. Scheutz, has appeared in the last part of the Transactions of the Academy of Upsala. Several interesting features in the geographical distribution of some of the species are pointed out, amongst which one of the most curious is the almost perfect identity of the *G. coccineum* from the Levant and the *G. chilense* from South Chile, the differences being such only as would scarcely have been set down as more than varieties had both come from the same country. The whole memoir is in the Latin language; the specific diagnoses are rather long, but the observations under each section and species point out the connection with and chief differences from the nearest allies.

The whole of the botanical literature published in or relating to Sweden has been regularly recorded in annual catalogues, inserted by T. O. B. N. Krok in the "Botaniske Notiser" of Stockholm.

The chief interest in the biology of Russia consists in its comparative uniformity over an enormous expanse of territory. Extending over more than 130 degrees from East to West, and above 20 degrees from South to North, without the interposition of any great geological break in mountain,* or ocean, all changes in flora or fauna, in the length and breadth of this vast area are gradual; whilst the mountains which bound it to the south and to the east, and the glacial characters of the northern shores, offer to the Russian naturalist several more or less distinct biological types, such as the Caucasian, the Central Asiatic, the Mantchurian, and the Arctic, all blending into the great Europeo-Asiatic type, and the three first-named, at least apparently, constituting great centres of preservation. By the careful discrimination of the various races which give to each of these types its distinctive character, the study of their mutual relations, of the areas which each one occupies without modification, of the complicated manner in which these several areas are interwoven, of the gradual changes which distance may produce, of the cessation of one race and the substitution of another without apparent physical cause, the Russian, even without travelling out of his own country, can contribute, more than any other observer, valuable materials for the general history of races. In botany I have on former occasions referred to Ledebour's "Flora Rossica" as the most extensive complete flora of a country which we possess, and to the numerous papers by which it has been supplemented. Several of these are still in progress, chiefly in the bulletin of the Society of Naturalists of Moscow, and I have notes of local floras and lists from various minor publications. The last received volume of the Memoirs of the Academy of Petersburgh include the botanical portion of Schmidt's travels in the Amur-land and Sachalin, in which the geographical relations of the flora are very fully treated of; and the first part of a very elaborate "Flora Caucasi" by the late F. J. Ruprecht, which may be more properly designated Commentaries on the Caucasian Plants than a flora in the ordinary sense of the word. It is an enumeration of species, with frequent observations on affinities, and a very detailed exposition of stations in the Caucasus, but without any reference to the distribution beyond that region; above 300 large 4to pages only include the Polypetalæ preceding Leguminosæ, and the lamented death of the author will probably prevent the completion of the work. N. Kaufmann, Professor of Botany at the University of Moscow, an active botanist of great promise, whose death last winter is much deplored by his colleagues, had published a Flora of Moscow in the Russian language, which had met with much success. In the zoology of Russia the most important recent work is Middendorff's "Thierwelt Sibiriens," analysed in the "Zoological Record," vi. p. 1, which, with the previously published descriptive portion and the botany of the journey by Trautvetter, Ruprecht, and others, forms a valuable exposition of the biology of N.E. Siberia, a cold and inhospitable tract of country, where organisms, animal as well as vegetable, are perhaps poorer in species and poorer in individuals than in any other region of equal extent not covered with eternal snows. Middendorff's observations on this poverty of the

* The celebrated chain of the Oural, which separates Asia from Europe is, in the greater part of its length too low, and the ascent too gradual to have much influence on the vegetation. The so-called ridge between Perm and Ekaterinburg is, according to Erman, not 1600 feet above the level of the sea, and rises from land which, for a breadth of above 120 miles, is only 700 feet lower.

fauna of Siberia, its uniformity and conformity to the European fauna, on the meaning to be given to the species, on their variability and on the multiplicity of false ones published, on the complexity of their respective geographical areas, on their extinction and replacement by others, &c., are deserving of the careful study of all naturalists. L. v. Schrenck's Mollusca of the Amur land or Mantchuria (reviewed in the "Zoological Record," iv. p. 504) is equally to be recommended for the manner in which the specific relations, the variability, affinities, and geographical distribution of Mantchurian Mollusca are treated. The publications of the first meeting of the Association of Russian Naturalists include a review of the Crustacea of the Black Sea by V. Czerniavski, an account of the Annulata Chætopoda of the Bay of Sebastopol by N. Bobretzki, and a paper on the zoology of the Lake of Onega and its neighbourhood by K. Kesslar, including a review of the fishes, Crustacea, and Annulata of the Lake of Onega, and of the Mollusca collected in and about the Lakes Onega and Ladoga, and a list of the butterflies of the Government of Olonetz. The historical and scientific memoirs published by the University of Kazan, of which several volumes have recently reached us, include a systematic enumeration and description of the birds of Orenburg (329 species), with detailed notes of their habits, &c., by the late Prof. E. A. Eversmann, edited after his death by M. N. Bogdanoff, forming an 8vo volume of 600 pages in the Russian language.

There is not in Russia at the present moment sufficient encouragement on the part of the public to induce the publication of independent biological works beyond a few popular handbooks; but the Imperial Academy of Petersburg has, on the other hand, been exceedingly liberal in the assistance it affords, and active in its issue of Transactions with excellent illustrations, as well as of its bulletin of proceedings. The volumes recently received include J. F. Brandt's "Symbolæ Sirenologicæ" and researches on the genus *Hyrax* (reviewed in "Zoological Record," v. p. 3, and vi. p. 5), A. Strauch's Synopsis of Viperidæ, with full details of their geographical distribution, E. Metschnikoff's studies on the development of Echinoderms and Nemertines, and N. Miklucho-Maclay's Memoir on Sponges of the N. Pacific and Arctic Oceans, with remarks on their extreme variability inducing the multiplication of false species. In botany, Bunge's Monograph of the Old-World species of *Astragalus* is the result of many years labour and careful investigation. The eight sub-genera and 104 sections into which this extensive genus is divided appear to be very satisfactory; but the species (971) are probably very much too numerous, and we miss that comparison with American forms which, considering the very numerous cases of identity or close affinity, is essential for the due appreciation of the N. Asiatic species. Bunge has also published a monograph of the *Heliotropia* of the Mediterraneo-Oriental region in the Bulletin of the Society of Naturalists of Moscow, which continues its annual volumes. The parts recently received continue several of the botanical enumerations already noticed, together with various smaller entomological papers.

(*To be Continued*)

## GEOLOGY

### On the Supposed Legs of the Trilobite, Asaphus platycephalus*

At the request of Mr. E. Billings, of Montreal, I have recently examined the specimen of *Asaphus platycephalus* belonging to the Canadian Geological Museum, which has been supposed to show remains of legs. Mr. Billing, while he has suspected the organs to be legs so far as to publish on the subject,† has done so with reserve, saying, in his paper, "that the first and all-important point to be decided, is whether or not the forms exhibited on its under side were truly what they appeared to be, locomotive organs." On account of his doubts, the specimen was submitted by him during the past year to the Geological Society of London; and for the same reason, notwithstanding the corroboration there received, he offered to place the specimen in my hands for examination and report.

Besides giving the specimen an examination myself, I have submitted it also to Mr. A. E. Verrill, Prof. of Zoology in Yale College, who is well versed in the Invertebrates, and to Mr. S. I. Smith, assistant in the same department, and excellent in crustaceology and entomology. We have separately and together considered the character of the specimen, and while we have reached the same conclusion, we are to be regarded as independent judges. Our opinion has been submitted to Mr. Billings, and by his request it is here published.

The conclusion to which we have come is that the organs are not legs, but the semi-calcified arches in the membrane of the ventral surface to which the foliaceous appendages or legs were attached. Just such arches exist in the ventral surface of the abdomen of the Macrura, and to them the abdominal appendages are articulated.

This conclusion is sustained by the observation that in one part of the venter three consecutive parallel arches are distinctly connected by the intervening outer membrane of the venter, showing that the arches were plainly *in the membrane*, as only a calcified portion of it, and were not members moving free above it. This being the fact, it seems to set at rest the question as to the legs. We would add, however, that there is good reason for believing the supposed legs to have been such arches in their continuing of nearly uniform width almost or quite to the lateral margin of the animal; and in the additional fact, that although curving forward in their course toward the margin, the successive arches are about equidistant or parallel, a regularity of position not to be looked for in free-moving legs. The curve in these arches, although it implies a forward ventral extension on either side of the leg-bearing segments of the body, does not appear to afford any good reason for doubting the above conclusion. It is probable that the two prominences on each arch nearest the median line of the body, which are rather marked, were points of muscular attachment for the foliaceous appendage it supported.

With the exception of these arches, the under surface of the venter must have been delicately membranous, like that of the abdomen of a lobster or other macruran. Unless the under surface were in the main fleshy, trilobites could not have rolled into a ball.

James D. Dana

* From the American *Journal of Science and Arts*, Vol. I, May, 1871.

† Q. J. Geol. Soc., No. 104, p. 479, 1870, with a plate giving a full-sized view of the under surface of the trilobite, a species that was over four inches in length.

## SCIENTIFIC SERIALS

*Annales de Chemie et de Physique.* The whole of the last part of the "Annales" is occupied by M. Berthelot's *Méthode universelle pour réduire et saturer d'hydrogène les composés organiques*, which is a *résumé* of the elaborate and exhaustive researches on the action of hydriodic acid on organic substances in which he has been engaged for the last three or four years. Most of the results have been already published from time to time in the *Bulletin de la Société Chimique de Paris*, and this classical research is now completed by the publication of the details of the methods of analysis and the thermochemical considerations involved. The author has found that any organic compound can be transformed into a saturated hydro-carbon, having, in general, the same number of atoms of carbon as the original substance, by heating it for a sufficient length of time to a temperature of 275°C., with a large excess of an aqueous solution of hydriodic acid of the specific gravity of 2.0. The proportion of the acid is varied according to the nature of the substance submitted to its action, twenty or thirty parts being sufficient to reduce an alcohol of the fatty series, whilst a member of the aromatic series and such substances as bitumen, wood charcoal, and coal, require, at least, one hundred times their weight; the large excess of acid serving the purpose of dissolving the iodine set free during the reaction, thus preventing its destructive action on the organic compound, and also in allowing the quantity of hydriodic acid necessary for the reduction of the substance, to be withdrawn from the solution without reducing its strength so far that the reaction ceases. One of the most remarkable results exhibited in the application of this method is that of the direct transformation of benzene into the saturated hydrocarbon, hexylene hydride, $C_6 H_6 + 8 H I = C_6 H_{14} + 8 I$, affording, as it does, an instance of a direct passage from the aromatic to the fatty series. When other members of the phenyl series are treated with hydriodic acid, the ultimate product is the same; but there is an intermediate step in the reaction, resulting in the formation of benzene, which, by the continued action of the acid, is transformed into the corresponding saturated hydrocarbon. The fifth and last part of the paper is of great interest from a theoretical point of view, since it comprises the results of the author's experiments on bitumen, wood charcoal, and coal. The former of these substances, under the influence of hydriodic acid, yields hexylene hydride, the saturated hydrocarbon corresponding to benzene, from which it may be inferred that bitumen is a derivative of benzene, produced by condensation and loss of hydrogen. Charcoal and coal, when treated according to M. Berthelot's method, are transformed into a mixture of various saturated hydrocarbons, identical with those found in petroleum oil. In fact *the coal is changed into petroleum oil.*

THE most important paper in the first three numbers of vol. xiii. of the *Atti della Società Italiana di Scienze Naturali* (April and November 1870, and January 1871), is a continuation of Prof. Delpino's article on "Dichogamy in the Vegetable Kingdom." In this paper the author passes in review the various modes in which the impregnation of plants is effected, with especial reference to the provisions for the impregnation of one plant by the fecundating organs of another.—M. A. Curò publishes a note on parthenogenesis among the Lepidoptera.—M. F. Sordelli contributes a note on the anatomy of the genus *Acme*, and on some of the hard parts of *Cœcilianella acicula*, illustrated with a plate; and further an anatomy of *Limax Doriæ*, Bourg., also illustrated, and including a tabular arrangement of the species of the genus *Limax*, for the elucidation of the characters of two new species, which the author describes under the names of *L. punctulatus* and *L. Bettonii*.—The Secretary of the Society, Dr. C. Marinoni, notices some remains of *Ursus spelæus* from the Cave of Adelsberg.—M. G. Bellucci gives an account of some evidences of prehistoric man in the territory of Terni.—M. L. Ricca communicates some observations on dichogamy in plants made by him upon the Alps of Val Camonica in 1870; and also a systematic catalogue of the vascular plants growing spontaneously in the olive-zone of the valleys of Diana, Marina, and Cervo, with indications of the special conditions of growth, times of flowering of each species, and occasional remarks upon their characters.—At p. 130 is the description of a supposed hybrid *Orchis*, *O. coriophoro-laxiflora*.—From M. C. Bellotti we have some observations on the disease of flaccidity, which destroys so many silkworms (*morts-flats*) in France and Italy; and from Dr. Taramelli a memoir, illustrated with an elaborate coloured plate, on the ancient glaciers of the Drave, Save, and Isonzo.

## SOCIETIES AND ACADEMIES

### LONDON

Geological Society, June 7.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Messrs. Henry Collinson and Thomas Milnes Favell were elected Fellows, and Dr. J. J. Kaup, of Darmstadt, was elected a foreign member of the society. The following communications were read:—1. "On the persistence of *Caryophyllia cylindracea* Reuss, a Cretaceous Coral, in the Coral-fauna of the Deep Sea." By Prof. P. Martin Duncan, F.R.S. The author first referred to the synonyms and geological distribution of *Caryophyllia cylindracea*, Reuss, which has hitherto been regarded as peculiar to the White Chalk, and as necessarily an extinct form, inasmuch as it belonged to a group possessing only four cycles of septa in six systems, one of the systems being generally incomplete. The distribution of the *Caryophlliæ* of this group in the Gault and the Upper Chalk, the Miocene, and the Pliocene, was noticed, and also that of the species with the incomplete cycle. The falsity of this generalisation was shown to be proved by the results of deep-sea dredging off the Havannah, under Count Pourtales, and off the Iberian peninsula under Dr. Carpenter and Mr. Gwyn Jeffreys. The former dredged up *Caryophyllia formosa* with four complete cycles, and the latter obtained, from depths between 690 and 1090 fathoms, a group of forms with four complete and incomplete cycles. This group had a Cretaceous facies; one of the forms could not be differentiated from *Caryophyllia cylindracea*, Reuss; and as a species of the genus *Bathycyathus* was found at the same time, this facies was rendered more striking. The representation of the extinct genera *Trochosmilia*, *Parasmilia*, *Synhelia*, *and Diblasus*, by the recent *Amphihelia*, *Paracyathi*, and *Caryophylliæ* was noticed, and it was considered that as the Cretaceous forms throve under the same external conditions, some of them only being persistent, there must be some law which determines the life-duration of species like that which restricts the years of the individual. It was shown that deep-sea conditions must have prevailed within the limits of the diffusion of the ova of coral polyps somewhere on the Atlantic area ever since the Cretaceous period. Mr. Gwyn Jeffreys remembered that at the spot where the coral in question was dredged up the sea-bottom was extremely uneven, varying as much as fifty fathoms within a quarter of a mile. It was also not more than forty miles from land. The species of mollusca dredged up were extremely remarkable, and many were totally different from what he had previously seen. They were, however, living or recent; none of them were Eocene or Miocene, much less Cretaceous, like *Terebratula caput-serpentis*. He quoted from Mr. Davidson other instances of the persistence of forms, especially of the genus *Lingula* from the Silurian formation. The persistence of this species of coral, as well as that of Foraminifera, from the Cretaceous to the present time, was therefore not unique, and other cases of survival from even earlier times might eventually be recognised. Dr. Carpenter, after commenting on the reductions that extended knowledge enabled naturalists to make in the number of presumed species, could not accept the mere identification of species as of the highest importance in connecting the Cretaceous fauna with that of our own day. The identity of genera was, in his opinion, of far more importance. He instanced *Echinothuria* and *Rhizocrinus* as preserving types identically the same as those of a remote period, and as illustrating the continuity of the deep-sea fauna from Cretaceous times. The chemical and organic constitution of the deep-sea bottom of the present day was also singularly analogous to that of the Chalk sea. The low temperature at the bottom of the deep sea, even in equatorial regions, was now becoming universally recognised, and this temperature must have had an important bearing on the animal life at the sea-bottom. Prof. Ramsay thought that there was some misapprehension abroad as to the views held by geologists as to continuity of conditions. They had, however, always insisted on there having been an average amount of sea and land during all time; and the fact of sea having occupied what is now the middle of the Atlantic since Cretaceous time would create no surprise among them. If, however, the bed of the Atlantic were raised, though probably many Cretaceous genera, and even species, might be found, there would on the whole be a very marked difference between these Atlantic beds and those of the Chalk. Mr. Seeley had already, in 1862, put forward views which had now been fully borne out by recent investigation. His conviction was that, from the genera having persisted for so long a time, the genera found in any formation afforded no safe guide as to its age, unless there were evidence of their having since those formations become extinct. Mr. Etheridge maintained that the species in different formations were sufficiently distinct, though the genera might be the same. Recent dredgings had not brought to light any of the characteristic molluscan forms of the Cretaceous time; and it would be of great importance to compare the results of future operations with the old Cretaceous deep-sea fauna. Prof. Rupert Jones, with reference to the supposed sudden extinction of chambered Cephalopods, remarked that Cretaceous forms had already been discovered in Tertiary beds in North America, and also that cold currents could not have destroyed them, seeing that icebergs came down to the latitude of Croydon in the Chalk sea.—2. "Note on an *Ichthyosaurus* (*I. enthekiodon*) from Kimmeridge Bay, Dorset." By J. W. Hulke, F.R.S. In this paper the author described the skeleton of an *Ichthyosaurus* from Kimmeridge Bay, agreeing in the characters of the teeth with the form for which he formerly proposed the establishment of the genus *Enthekiodon*. The specimen includes the skull, a large portion of the vertebral column, numerous ribs, the bones of the breast-girdle, and some limb-bones. The first forty-five vertebral centra have a double costal tubercle. The coracoids have an unusual form, being more elongated in the axial than in the transverse direction, and this elongation is chiefly in advance of the glenoid cavity. The articular end of the scapula is very broad. The paddles are excessively reduced in size, the anterior being larger than the posterior, as evidenced by the comparative size of the proximal bones. The species, which the author proposed to name *I. enthekiodon*, most nearly resembles the Liassic *I. tenuirostris*. The length of the preserved portion of the skeleton is about 10ft., the femur measures only 2in., and the humerus 2·7in.—3. "Note on a Fragment of a Teleosaurian Snout from Kimmeridge Bay, Dorset." By J. W. Hulke, F.R.S. In this paper the author described a fragment of the snout of a Teleosaurian obtained by Mr. J. C. Mansel, F.G.S., from Kimmeridge Bay, and which is believed to furnish the first indication of the occurrence of Telosaurians at Kimmeridge. The specimen consists of about 17in of a long and slender snout, tapering slightly towards the apex, where the præmaxillæ expand suddenly and widely. The nostril is terminal and directed obliquely forwards; the præmaxillæ ascend 2·5in. above the nostril, and terminate in an acute point; and each præmaxilla contains five alveoli. The lateral margins of the snout are slightly crenated by the alveoli of the teeth, of which the three front ones are smaller than the rest; most of the teeth have fallen out, but a few are broken off, leaving the base in the sockets. Mr. Seeley thought it likely that Mr. Hulke would eventually be led to re-establish his genus *Enthekiodon*. He remarked on the peculiar characters presented by the specimen, and referred especially to the coracoids, which were unlike those of *Ichthyosaurus*, but presented a close resemblance to those of *Plesiosaurus*. He considered that there were indications of its having been connected with a cartilaginous sternum. The scapula furnished an important character in its widening, which formed a distinct acromion process. Mr. Seeley remarked that double-headed ribs occur only in animals with a four-chambered heart; and that, considering this and other characters, there was no reason for placing *Ichthyosaurus* lower than among the highest Saurians. He considered that the Teleosaurian snout differed from all known types. Dr. Macdonald believed that what is called the coracoid has nothing to do with the shoulder-girdle, and thought it might be a part of the palate. Mr. Mansel stated, in answer to the President, that the fossils were obtained from about the middle of the Kimmeridge Clay. Mr. Etheridge suggested that it would be desirable to ascertain whether the horizon of the *Ichthyosaurus* described was the same as that of the specimens from Ely. Mr. Gwyn Jeffreys inquired as to the food and habits of the *Ichthyosaurus*. Mr. Hulke, in reply, stated that, from the presence of a stain and of numerous small scales, under the ribs, the food of the *Ichthyosaurus* probably consisted of Squids, and small fishes. He showed that the so-called coracoid was clearly a part of the shoulder-girdle.

Geologists' Association, June 2. — Rev. T. Wiltshire, president, in the chair. A paper on "Flint" was read by Mr. Hawkins Johnson, F.G.S. After stating the reasons which had induced him to pay special attention to the subject of the formation of flint, the author described the characters and mode of occurrence of nodular and tabular flint and chert. The various combinations into which silicon enters were then recapitulated, and a description of sponges introduced a statement of the theory contended for in this paper to account for the formation of chalk flints. This theory is simply that *silicon* replaced the *carbon* of the sarcode of the sponges of the Cretaceous seas. Flints are therefore merely silicified sponges. The empty shells of echinoderms were frequently used by sponges which in many cases outgrew and surrounded the shells, and these sponges afterwards becoming silicified we find the tests of echinoderms either wholly or partly embedded in chalk flints. Prof. Tennant dwelt upon the opinions of Dr. Bowerbank, and pointed out in opposition to the views of that authority that agates could not have derived their origin from sponges, since they were found in volcanic rocks. Mr. Deane, who had been associated with Dr. Mantell in his researches with respect to the origin of flints, stated that, contrary to the general belief that fossils were not found in any flints except those from the Upper Chalk, organisms had been discovered in flints from the Lower or Grey Chalk. Dr. Bedwell, who had paid great attention to this subject for a long time, denied Dr. Bowerbank's assertion that a central layer of chalk is not present in horizontal as it is in vertical tabular flint, and he could not admit the possibility of vertical walls of flint, sometimes of great height, being produced by the silicification of sponges which grew on each side of a fissure in the bed of the chalk sea, as had been contended for by Dr. Bowerbank and Mr. Johnson. Prof. Morris, after complimenting the author on the value of the paper and the knowledge of the subject which he had displayed, referred to the investigations into the orig n of flint by the observers of the last century, and gave a very interesting *résumé* of the principal facts connected with the occurrence of flint in the stratified rocks. The Professor contended for the segregation of the silica of the chalk around nuclei, in opposition to the theory advocated by Mr. Johnson, which was first propounded by Dr. Brown of Edinburgh. The amount of diffused silica in the Upper Chalk is much less than in the Lower Chalk, in which flints are rare. The tests of echinoderms are never silicified, nor are phosphatic animal substances, such as bone. He believed that the sponges of the old seas, from what we know of the sponge-gravel and the Upper Greensand, were chiefly silicious, and not ceratose; and that they were extremely abundant is shown by the fact that in the Haldon Hill Greensand masses of chert occur eight feet thick, which are quite full of the spiculæ of sponges. Mr. Johnson briefly replied, but was scarcely prepared then to combat the observations of Professor Morris. Very fine collections of flints showing peculiarities were exhibited by Messrs. Johnson, Bedwell, Evans, Leighton, Deane, and Meyer.

Mathematical Society, June 8.—Mr. W. Spottiswoode, F R.S., president, in the chair. Messrs. W. Chadwick, M.A., and J Griffiths, M.A., were elected members of the society.—Prof Cayley, V.P., stated results he had arrived at in his investigation of Plücker's models of certain oceanic surfaces. He had been able to identify eight out of the fourteen models in the Society's possession (presented by Dr. Hirst.) — W. Samuel Roberts, M.A, gave an account of his paper "On the Motion of a Plane under certain Conditions."—Prof. Henrici, V.P., exhibited cardboard models of two ellipsoids, of a hyperboloid of one sheet, and of an elliptic paraboloid, also stereograms of the models of surfaces exhibited at former meetings of the society.

Entomological Society, June 5.—Mr. J. W. Dunning, F.L.S., vice-president, in the chair.—The Secretary read a letter from the Rev L. Jenyns, of Bath, with reference to the reported showers of insects or other organisms at Bath, noticed at the last meeting. Mr. Jenyns had examined some of these organisms, and found they were *Infusoria*, probably *Vibrio undula* of Müller, many of them being congregated into spherical masses enveloped in a gelatinous substance. They fell during a heavy shower of rain.—Mr. Butler exhibited specimens of *Lepidoptera*, upon which he and Mr. Meldola had experimented with a view to ascertain the action of dyes. Many species had been subjected to aniline dyes, and all kinds of colours produced. Mr. Butler also found that when the insects were immersed in a solution of soda for the purpose of causing the dyes to be more readily taken, the colouring matter of the scales was completely discharged and collected at the bottom of the solution. Mr. Bicknell had subjected *Gonopteryx rhamni* to the action of cyanide of potassium, acting upon a suggestion made last meeting, and the yellow colour was changed to orange-red.—Mr. W. C. Boyd exhibited an example of *Ramia cratægata*, captured near London, the apical portion of one wing of which was changed to brown.—Mr. Müller exhibited the bell-shaped nest of *Aglena brunnea*, a spider; also galls of an undescribed species of *Phytophus* on *Betula*.—Mr. F. Smith exhibited three rare British species of *Hymenoptera*, captured by Mr. Dale in Dorsetshire, consisting of *Myrmecomorphus rufescens* (*Proctotrupidæ*), *Ichneumon glaucopterus*, and *Osmia pilicornis*.—Mr. Holdsworth, of Shanghai, communicated notes on the method practised by the Chinese in cultivating the silk-producing *Bombyx Pernyi*.—Mr. Butler read "Descriptions of Five New Species of Diurnal *Lepidoptera* from Shanghai."—Mr. Baly communicated "Descriptions of a new genus, and of some recently-discovered species of *Phytophaga*."—Mr. Kirby communicated "Synonymic Notes on *Lepidoptera*."

Statistical Society, June 20.—Mr. Hyde Clarke read a paper "On the Transmissibility of Intellectual Qualities in England." As one kind of test of intellectual exertion, he took the statistics of the writers of books in the Biographia; of 2,000 authors, 750 were born in country districts, and 1,250 in town districts. Examining the towns and the distribution in them, 333 were allotted to London, 73 to Edinburgh, and 53 to Dublin. The largest numbers in the tables beyond these were found in cathedral and collegiate cities. The deductions he drew were that intellectual activity is distributed unequally, but that it is more among the town or more highly educated population than among the rural populations. He pointed out that the larger the concentrated educated population, the larger is the intellectual development, and he referred to the like examples of Greece, Rome, and modern Europe, where the same law is to be traced. The great modern centres of industry in England occupy a low relative position in the list, and are scarcely to be noticed, but they are now beginning to contribute. He affirmed that the literary class was produced from the educated class, and not from the illiterate classes. While no educational effort will produce men of great genius, he inferred that literary attainments are in relation to literary culture or the culture of the educated classes, and that by extending education to other classes of the population, the intellectual capacity of the community will be extended and propagated within certain limits.

MANCHESTER

Literary and Philosophical Society, April 4.—"Notes on drift of the eastern parts of the counties of Chester and Lancaster." By E. W. Binney, F.R.S., F.G.S., president. Having in a previous paper given a short description of the higher drift found in these counties, the author now proceeds to consider the thick surface covering of the general drift, which nearly hides from our view the underlying strata, except where they are exposed in river courses or in canal or railway cuttings. This generally reaches to an elevation of about 700 feet above the sea, and does not alter much in its appearance, whether it is seen at Blackpool, Ormskirk, or Liverpool, or at Burnley, Rochdale, Glossop, or Macclesfield, except being usually more divided as it is found inland, and approaches the sides of the Pennine chain. It consists of beds of till, clay, sand, and gravel. It has been treated on by various authors, a list of whose works are given.

Annual Meeting, April 18.—Mr. E. W. Binney, F.R.S., F.G.S., president, in the chair. The report of the Council was read by one of the secretaries. The Council have the satisfaction to report that the past year has been one of steady progress for the Society. The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:—President: Mr. Edward William Binney, F.R.S., F.G.S. Vice-presidents: Mr. James Prescott Joule, D.C.L., F.R.S., F.C.S., Mr. Edward Schunck, F.R.S., F.C.S., Mr. Robert Angus Smith, F.R.S., F.C.S., Rev. William Gaskell. Secretaries: Mr. Henry Enfield Roscoe, F.R.S., F.C.S., Mr. Joseph Baxendale, F.R.A.S. Treasurer: Mr. Thomas Carrick. Librarian: Mr. Charles Bailey. Other Members of the Council: Mr. Peter Spence, F.C.S., Mr. William Leeson Dickinson, Mr. Henry Wilde, Mr. Robert Dukinfield Darbishire, F.G.S., Prof. Osborne Reynolds, and Mr. William Boyd Dawkins, F.R.S. Dr. Joule, F.R.S., drew attention to the remarkable atmospheric phenomenon which had been seen by several persons in Derbyshire and elsewhere, on the evening of Good Friday, April 7, and stated that he had witnessed a similar appearance near Glasgow on the day before it was observed in this neighbourhood. The perpendicular ray extended upwards from the sun to an altitude of 30°, and was very clearly defined. It was observed from half an hour before, until after the sun had set. The phenomenon was also witnessed, at the same time, by Prof. J. Thomson, who was sailing on the Firth of Clyde.

CAMBRIDGE

Philosophical Society, May 15.—"On Dr. Wiener's model of a cubic surface with twenty-seven real lines," Prof. Cayley, F.R.S.—"On the tides in a rotating globe covered by a Sea of depth constant at all points in the same latitude, attracted by a moon always in the plane of the equator; considered with reference to the tidal retardation of the earth's angular motion about its axis," Mr. Röhrs. "On the motion of imperfect fluid in a hollow sphere, rotating about its centre under the action of impressed external periodic forces, considered with reference to the phenomena of precession and nutation," Mr. Röhrs.

May 29.—"On an illustration of the empirical theory of vision," Mr. Coutts Trotter. "On a table of the logarithms of the first 250 Bernoullis Numbers," Mr. Glaisher.

NEW YORK

Lyceum of Natural History, Oct. 24, 1870.—"The Geological Position of the Remains of Elephant and Mastodon in North America," by Dr. J. S. Newberry. The genera *Elephas* and *Mastodon* existed on the globe during the Miocene Tertiary Epoch and were represented by various species from that time to the advent of man. The question is, when the two species *Elephas primigenius* and *Mastodon giganteus* are first met with in ascending the geological scale. In Europe it is claimed that remains of these species are found in the true Boulder Drift, and in California in the Pliocene Tertiary deposits. Whether either of these statements is strictly true remains to be decided by future investigations. In central and eastern North America the remains of elephant and mastodon are found abundantly in peat bogs and other superficial and recent deposits, also in some strata of unconsolidated material which are considered as belonging to the drift, although there has been much difference of opinion whether these beds form part of the true undisturbed drift, or whether they consist of re-arranged drift-materials or what is called "Modified Drift. The facts now offered seem to prove conclusively that the remains of elephant and mastodon are found in the true and unchanged drift, but only in the more recent of the drift deposits. The presence of these great mammals must therefore be considered as one of the incidents in the history of the drift, but as incidents belonging only to the last chapters in this long and somewhat eventful history. In order that the advent of the elephant and mastodon may be properly placed in the sequence of phenomena embraced in the Drift period, it will be necessary to make a brief review of these phenomena so far as they are known to us. The geological periods immediately antecedent to the Drift are the Cretaceous, which was a period of marked continental submergence, when the ocean covered most of the western half of the continent and reached several hundred feet higher than now over the basis of the eastern highlands; and the Tertiary with its three subdivisions Eocene, Miocene, and Pliocene. The Eocene was a period of continental progressive emergence—land area gradually expanding, climate subtropical. In the Miocene and Pliocene epochs the topography was in a general way what it is now, but in detail the surface was considerably more diversified, especially by the presence of great fresh-water lakes which occupied much of the surface on both sides of the Rocky Mountains. At this time there was probably a land connection between the northern part of North America and Europe on the one hand and Asia on the other. The climates of Alaska and Greenland were then as mild as that of Virginia now; the flora was luxuriant and varied, and was common to Europe, Iceland, Spitzbergen, Greenland, our continent, and North-eastern Asia; palms grew farther north than the Canadian line. The fauna was much richer than now, including elephant, rhinoceros, and many other animals not now living in either of the Americas, and indeed as large a number of the great mammals as are now found in Africa. The superficial boulders and gravels of the Drift are clearly the result of iceberg action. It is proved by the undisturbed condition of the clays below, that they must have been *floated* to their present resting places, just as boulders, sand, and gravel are floated from Greenland to the banks of Newfoundland, and there spread broadcast over the sea bottom. In the Drift deposits above the blue clay, remains of Elephant and Mastodon have been repeatedly found, still more frequently in the Peat-bogs of the present surface, and the much-discussed question has been, whether these mammalian remains were deposited with the upper layers of the Drift or were buried in them by subsequent shifting, as in the valleys of streams. The facts to which attention is now specially directed would seem to decide that question. It has long been known that in many parts of the valley of the Mississippi, wells penetrating twenty, thirty, or more feet, the superficial accumulations of *drifted* materials—clays and sands, with gravels and boulders brought from the far north—encounter sticks, logs, stumps, and sometimes a distinct carbonaceous soil. Combining facts of this character, of which records have been accumulating from year to year, with those brought to light by recent investigations directed specifically to this object, it is proved that over a great area at the West a sheet of buried timber, a vegetable soil, beds of peat covered with sphagnous moss, erect stumps, and in some cases standing trees, form a distinct line of demarcation between the older and newer drift deposits. In or above the horizon of this ancient soil have been found numerous animal remains: *Elephas*, *Mastodon*, *Castoroides* (the great extinct beaver) and some others.

PARIS

Academy of Sciences, June 5.—M. Faye in the chair. M. Delaunay gave some supplementary information relative to the attempts made by the Communists to set fire to the National Observatory. The director of the establishment says that M. Yvon Villarceau was somewhat incorrect in the description of the damage done to the instruments belonging to the geodesic service, of which he is the chief and superintendent. The National Observatory, which was built more than two centuries ago under the reign of Louis XIV., is a very strong building, with thick walls, and the garden itself is at an elevation of 10ft. from the level of the surrounding land. The extent of the garden is about half an acre. The Communist forces had garrisoned it, and it was only when obliged to retreat that they tried to burn it down, which attempt was defeated only by the personal exertions of the staff, their families, and well-disposed persons in the vicinity.

The spherical copper caps of the equatorials were perforated by many holes from Versaillists' rifles, and the equatorials themselves were slightly hurt. But altogether the damage done is nothing in comparison with the harm which was contemplated.—Dr. Guyot sent a paper on Dynamite, and the means of protecting storehouses from spontaneous explosion. Dynamite is known to be a mixture of sand and nitro-glycerine. When it is wrapped in a cartridge, made as usual with paper, the capillary attraction works on the nitro-glycerine, which is slowly separated from the sand, and impregnates the protecting matter. In this new form nitro-glycerine is almost as explosive as in its ordinary liquid state, which may very easily be proved.—M. Elie de Beaumont read a circular noticing that the next session for the British Association will be held this summer at Edinburgh. The learned perpetual secretary expects that many members will try to attend it, so that French science may have a fair representation, which is seldom the case on these occasions.

June 12.—M. Delaunay in the chair. The greater part of the members, who were obliged to escape from Paris, have resumed their seats. M. Leverrier was congratulated on having resumed his professorial duties at the Sorbonne, where he has opened this very morning his regular course of lectures on Mathematical Astronomy, Almost every scientific editor of the Parisian papers has returned also to his seat.—M. Serret presented a memoir on the principle of least action, economy of mechanical work by natural forces acting from certain centres by attraction. Euler and Lagrange had confined their exertions to show that the first differential was always zero. This was not sufficient, as such a differential may belong to a maximum if the second differential becomes negative, which was left to be demonstrated by Euler and Lagrange. The work was very difficult indeed.—M. Becquerel read a very long paper on atmospheric electricity. It was worked by himself as well as by his son, as the first part of a theory which can be reviewed only when completed. M. Becquerel, advocating the opinions started by Pelletier, thinks that the electricity of the upper regions is positive, and he says, moreover, that it comes from the sun, which is a focus of positive force. The electrical connection from the sun to our upper atmosphere is maintained through celestial space, which is not an absolute vacuum, but is filled with gases at a low pressure. The electricity of the earth is negative, and every thunder clap is a discharge between the earth and the upper regions through the air.—M. W. de Fonvielle sent a note reviewing the organisation of the Postal Telegraphic service in England, and showing that the French Government is wrong in maintaining two different administrations. The case of the French Government is very bad, as the two administrations were amalgamated during the war by the Tours delegation, under M. Steenackers, and ultimately separated. M. Buys Ballot, the celebrated director of the Utrecht Meteorological Observatory, asked from the Portuguese Government the establishment of a Meteorological station, or rather system, in the Azores Archipelago. This will result in the issuing of regular reports when the south-western gales are on their way to visit the British Islands and Western Europe. M. Delaunay, who read over the note at full length in the name of M. Buys Ballot, strongly advocated the proposition of his learned colleague. It is greatly to be hoped that the Portuguese Government will yield very shortly to the suggestion.

Academy of Inscriptions and Belles Letters, June 9.—The first sitting for a long period, as almost every member had been a refugee outside Paris, except a few officials. M. Haureau, director of the National Printing Office, explained that nothing was disturbed at this establishment. The Oriental Department is in excellent working order. The manuscripts of several members, which are kept there as well as valuable documents, are safe, owing to the mild rule of M. Debock, a working compositor, who was appointed a delegate by the Commune, and who protected also the National Archives, which are located in an adjoining building.—M. Alfred Maury, who had been left as Director of Archives by the Commune, had much trouble in protecting it against Communist fury, even with an order signed by Debock. M. Maury was praised for the energy exhibited and the courage shown in remaining at his post, running the risk of being taken as a hostage. He was much assisted in his work of protection by the gate-keeper, who severally turned out small parties of incendiaries coming with petroleum to execute their infamous orders. M. Leon Renier said that the stock of Borghesi works printed by the Academy, has perished at the same time as the Louvre Library. But the Borghesi manuscripts are safe. The 7th volume had not been distributed, and it will be necessary to print it again at the expense of the Academy, which had a limited credit for the whole edition. M. Leopold Delisle said that the manuscripts of the National Library, which had been concealed in the Archives, are safe. A few shelves had been slightly attacked by damp, but the real damage amounts to very little. There are seven nominations required in order to fill up the vacancies; death has removed four members, two ordinary members and one foreign associate. The Academy has adopted a proposition of M. Renan to fill up the vacancies gradually. On the 16th the Academy will appoint a commission for reporting upon the respective merits of candidates as foreign associate members. On the 23rd the Academy will examine the titles of the candidates for filling the seats of MM. Villemain and Alexander, whom the Academy lost before the Prussian siege. On the 30th the Academy will appoint a committee for reporting upon the candidates to two honorary memberships; but the nomination for the last two ordinary memberships will be postponed till next winter.

## BOOKS RECEIVED

English.—Scrambles among the Alps, 1860-1869: E. Whymper (Murray).—The Antiseptic System: Dr. A. Sansom (H. Gillman).—Introductory Text-book of Meteorology: Dr. A. Buchan (Blackwood and Sons).—Manual of Modern Geography: Rev. A. Mackay, 2nd edition (Blackwood and Sons).

Foreign.—(Through Williams and Norgate)—Die Grundzüge des graphischen Rechnens u. der graphischen Statik: K. von Ott.—Der Seideospinner des Maulbeerbaumes: F. Haberlandt.

## PAMPHLETS RECEIVED

English.—Chemical Phenomena of the Blast Furnace, Pt. II.: J. Lowthian Bell.—Annual Address by the President of the Royal Geographical Society.—Practical and Experimental Philosophy, Pt. II: R. Willis.—Report of the Winchester College Natural History Society.—Vaccination viewed politically: F. W. Newman.—An Essay on Unsolved Ethical Questions: D. Rowland.—Transactions of the Northumberland and Durham Natural History Society.—On Barometric Differences and Fluctuations: J. K. Laughton.—Thirty-eighth Annual Report of the Royal Cornwall Polytechnic Society.—A Catalogue of Hardy Perennials, &c.: W. Robinson.—British Statesman and Churchman, No. 10.—Will the Earth become a Sun-Spot?: R. Holmes.

American and Colonial.—Australasian Medical Gazette, No. 37.—Catalogue of the Iowa University, 1870-71.—Lectures delivered at the Industrial and Technological Museum, Melbourne, during the Spring Session of 1870.—Population: its Law of Increase: N. Allen.—The Physiological Laws of Human Increase: N. Allen.

Foreign.—Ueber einige Trematoden und Nemathelminther: R. von W. Sakow.—Ofversigt af konigl., V. Akademie, Forhandlingen.—Die Geographische Verbreitung der See-gräser.

## DIARY

*FRIDAY*, June 23.

Quekett Microscopical Club, at 8.

*MONDAY*, June 26.

Royal Geographical Society, at 8.30.

*WEDNESDAY*, June 28.

Society of Arts, at 8.—Anniversary Meeting.

## CONTENTS

THURSDAY, JUNE 29, 1871

## RAMBLES ROUND LONDON

*Saturday Afternoon Rambles Round London. Rural and Geological Sketches.* By Henry Walker. (London: Hodder and Stoughton. 1871.)

THE title of this book is suggestive of one of the movements of modern times, the Saturday Half-holiday, a movement in the right direction, if the recipients of the boon fully appreciate and understand how to use it beneficially. This may be done in various ways, either in the study of the subjects contained in the different museums in the Metropolis devoted to Art, Manufactures, or Natural Science; or in excursions in the vicinity, either for obtaining health or becoming acquainted with the various natural objects met with in the rambles, thus adding to the stock of intellectual and physical enjoyment, enlarging knowledge, and, perhaps, adding some new fact to the already rich stores of local observation. The institution of Field Clubs and Naturalists' Societies is not altogether new in principle, but their steady increase is a marked feature of late years; and while they have largely contributed to stimulate more systematic researches, they have at the same time exercised a social influence in bringing together persons whose tastes and pursuits have generally for their common object the investigation of the varied manifestations of creative intelligence. To those whose love of natural history leads them into the field, this small volume will be found a useful companion and guide, as it gives in a very able and pleasant manner much useful information respecting the more favoured spots easily accessible by the naturalist around London.

We can believe that these "Saturday Half-Holiday Rambles" have been the means of emancipating many young men from the gas-light hours on the Saturday to a good long afternoon of daylight. Many of them who have rural tastes, and even tastes for natural history, have never heard of the Quekett, the Geologists' Association, the South London Microscopical, and other London societies, part of whose programme consists of natural history rambles on the Saturday afternoon. So huge a place is London that there is danger of the amateur naturalist foregoing much that he might profit by, for want of such knowledge. The more we know of London life, the more do we see that this is the kind of work for young men closely engaged in the exhausting pursuits of a great city. Natural history pursuits are just the recreation they need; and the movement inaugurated by the energetic Secretary of the Saturday Half-holiday Committee of the Early Closing Association appears to fill up a gap in the previously existing modes for employing their time.

The Saturday Afternoon Rambles comprise London park and forest trees, Battersea Park, with its subalpine and tropical floras, Kew Gardens, visits to Burnham and Knockholt Beeches, Hampstead Heath and Hornsey, and autumn tours round Godalming. Interesting, however, as is the present landscape scenery of these districts, the author carries us back to the more ancient geography of the London area: the old sea-bed in Middlesex, and the subsequent changes it has undergone, and by which the present physical features have been produced.

Among the many interesting geological features to be noticed in the neighbourhood of London, the Thames valley is one, and is fully described in the work before us. Few of us are aware, except those acquainted with Mr. Prestwich's work, "The Ground beneath Us," that the familiar Thames of to-day has a pre-historic history distinct from its association with man and his fortunes, from which it is too commonly supposed to derive its sole interest and charm Still, the Thames, if we trace it from its source to the sea is replete with considerable interest, especially when we take into consideration the origin and character of the strata over which it flows in its onward course. These rocks reveal to us successive changes in the physical features and distribution of land and water during long past periods in the history of the globe; they tell us of successive oceans, or perhaps to some extent of a continuous ocean, more or less tropical in character, abounding in various forms of life adapted to the then existing conditions, which forms were successively replaced either by evolution or by new creations, coincident with the different inorganic changes which that area has undergone.

Without entering into a description of these changes (which we feel will be fully illustrated in the forthcoming work by Prof. Phillips), we will attempt to trace from the book before us the origin and condition under which the deposits in the Thames valley were accumulated, such as those which may be observed from the neighbourhood of Kew to Erith, and beyond it. The present stream, the parent of commerce and of civilised life, with its valley so rich in interesting landscapes, is but a diminished representative of a pre-historic larger river, by the agency of which, to a considerable extent, the present valley was formed. Even in the vicinity of London we have traces of older sea beds, such as the chalk, the London clay with its subordinate estuarine beds of the Woolwich series, and its overlying marine strata of middle Eocene age represented by the Bagshot sands, capping here and there the summits of the adjacent hills, and these again overlain by deposits of much later age, and indicating considerable change in the climatal conditions of the period, namely, the beds of glacial age which abut upon the northern heights of the valley, as at Highgate and elsewhere. That the present physical features of the Thames valley are of remote antiquity there can be little doubt, and many have been the opinions suggested as to its origin and age. Some have considered it partly of preglacial or glacial age, others as due to the torrential action of vast bodies of water produced by the summer thaws when the winters of England were of an arctic severity, or that the river itself was the agent by which the valley was formed. Suffice it, however, to say that from the corresponding nature of the strata on each side, which shows they were once continuous, for instance between Highgate and Norwood, it is evident, as suggested by Mr. Prestwich, that the valley of the Thames acquired its present dimensions in a period of greater atmospheric waste than the present, and of river erosion of greater intensity. Whether or not its features were partially moulded previous to the glacial period, it is probable that during the emergence of the land from the glacial sea, its present contour was more prominently determined, and it has been subsequently further modified and enlarged by the older river and its tributaries. There is even some reason for believing that its present outflow was not its former one, but that, acording to Mr. S. V. Wood, jun. (whose researches are so well known) the river probably drained southward into the Weald, being barred in by a ridge of lofty land *now* cut through by the Thames river.

The deposits of the ancient river afford memorials of considerable interest, for they tell us that along its forest-clad margins lived numerous mammalia, most of which have become extinct in the British area, although some of the genera are now restricted to the Europeo-Asiatic continent. Thus we find remains of the rhinoceros, elephant, hippopotamus, bear, and lion entombed in the valley deposits, affording a proof that at that period or previously England was joined to the Continent, over which land these animals probably migrated, so that the insular position of England is but of comparatively modern date. A further study of these remains yields to us the important evidence that in this area there were representatives of a northern and southern fauna,—the commingling of which, as the reindeer and musk ox with the hippopotamus and rhinoceros, may have arisen from the Thames area having been on the borders of two distinct zoological provinces. While, however, the majority of the Mammalia belong to extinct species, the Mollusca with which they are associated are, with two or three exceptions, still found in Britain; one shell, however, the *Cyrena fluminalis*, is at present restricted to the Nile; this assemblage in the old brick-earth deposits of the Thames valley indicating a greater tenacity of life in the molluscan than in the mammalian fauna.

## WEINHOLD'S EXPERIMENTAL PHYSICS

*Vorschule der Experimentalphysik.* Von Adolf F. Weinhold, Professor an der Königl. höheren Gewerbschule zu Chemnitz. Erster Theil. (Leipzig, 1871. London: Williams and Norgate. Pp. 208.)

THIS is by far the best school-book of Physics we have ever seen. Its leading characteristic will be understood by many readers from the statement that it is intended to be for Physics what Stöckhardt's well-known "Vorschule der Chemie" is for Chemistry. The author endeavours, as far as possible, to bring the reader into personal contact with physical experiments and phenomena. He does this by describing in detail, not only how to produce the phenomena and make the experiments of which he speaks, but also, in most cases, how to make the necessary apparatus with such materials as are to be got in almost any country-town. The result is that we cannot imagine a boy who possesses, in any degree, what is called a "mechanical turn" reading this book, without wanting to set to work at once to make experiments for himself. One of the main objects of the book is, in fact, to give a definite and useful direction to the "taste for making all sorts of things," which, as the author says, is so common amongst boys. With this intention he has made a careful choice of such experiments as, "by their pleasing nature, are adapted to awaken an interest in physical studies, but has avoided, as far as possible, the introduction of mere playthings, and has altogether excluded everything like conjuring tricks." With regard to the expenditure required for making the experiments described, the author estimates it at about fifty thalers (7*l.* 10*s.*) or a little more, but points out to those who are unwilling or unable to incur the gradual outlay of this sum, how much may be done for far less. A hammer and pair of pliers, "a small vice, a hand-vice, a few files, some sheet brass and wire, a spirit-lamp, a stock of glass tubing, and one or two retort-stands, suffice for a great deal, and should be provided before everything else."

We are glad to think that there are nowadays in this country a considerable and increasing number of schoolmasters who are anxious to introduce Experimental Physics as a regular part of school work, but who are deterred partly by the expense of the apparatus commonly thought necessary, and partly from the want of clear and full instructions as to how it should be used when they have got it. To any such we can do no greater service than to recommend them immediately to obtain Prof. Weinhold's book, and to follow implicitly the directions he gives.

We must not, however, leave it to be supposed that this work is simply a collection of practical instructions for making apparatus and experiments. Although each subject, even in the part now before us (which includes the general properties of bodies, statics, dynamics, hydrostatics, and hydrodynamics) is discussed from the point of view afforded by the particular experiments which it is intended that the reader should make for himself, the general conclusions to which the results of these experiments lead are always clearly and carefully pointed out; and a student who would work patiently through the book would lay a broad and sound foundation for a more special study of Physics, and would certainly know far more of the subject than the majority of those who have gone through in the ordinary way books of much greater pretensions. Above all, he could not fail to acquire one indispensable qualification for further progress, namely, the faculty of thinking about physical phenomena as of things which actually exist, and are just as fit subjects for the exercise of common sense as any of the facts of everyday life. This we consider is in itself no small excellence; for whoever has had any experience in teaching the elementary parts of Physics, must have become aware that very often the chief result of the *à priori* method, adopted in nearly all English books on the subject, is to make students think that the forces and motions of which Natural Philosophers talk are, if not fictions invented on purpose to puzzle them, at least so unlike anything that is ever met with in common experience, that it is useless to try to understand anything about them. In fact we believe that, except for students whose previous training has accustomed them to recognise the special cases that are included under general mathematical expression, the majority of English treatises on the fundamental parts of Physics are rather a hindrance than a help to a clear conception of the ideas they profess to explain. The true method of teaching Physics, at least to beginners, we believe to be the one adopted in this book, whereby the learner is made to acquire an actual personal acquaintance with all the most important facts of the science, through the observations which establish them having been brought within the range of his personal experience.

If this is done at all thoroughly, the modes of expressing the laws of physical phenomena in technical mathematical language may almost be left to suggest themselves when the requisite progress shall have been made in pure mathematics.

## BOOK SHELF

*The Sub-tropical Garden; or Beauty of Form in the Flower-Garden.* By W. Robinson, F.L.S. With Illustrations. (London: Murray, 1871.)

THIS volume is a sequel to the valuable works which Mr. Robinson has already given us—"The Wild Garden," and "Alpine Flowers for English Gardens." The title is a misleading one, and is thus defined by the author:—"Sub-tropical gardening means the culture of plants with large and graceful or remarkable foliage or habit, and the association of them with the usually low-growing and brilliant flowering-plants now so common in our gardens, and which frequently eradicate every trace of beauty of form therein, making the flower-garden a thing of large masses of colour only." It is a pity that Mr. Robinson has assisted to perpetuate so erroneous a designation, which conveys the idea of the culture of tender plants fitted only for our hothouses. The greater part of the volume is occupied with an alphabetical list of plants suitable for the above purpose, with description of the peculiarities of their foliage, mode of cultivation, and propagation, &c. The accompanying cut is intended to suggest the effects to be obtained from young and vigorous specimens of hardy, fine-leaved trees. In all these points Mr. Robinson

AILANTUS AND CANNAS

may be safely followed as a guide, combining great practical knowledge of gardening, an extensive acquaintance with the native habits of plants, and an artist's eye to the beauty of form and combination. The following sentence gives his idea of what gardening should be. "Nature, *in puris naturalibus*, we cannot have in our gardens, but Nature's laws should not be violated; and few human beings have contravened them more than our flower-gardeners during the past twenty years. We should compose them from Nature, as landscape artists do. We may have in our gardens, and without making wildernesses of them either, all the shade, the relief, the grace, the beauty, and nearly all the irregularity of Nature." A. W. B.

*The Meteoric Theory of Saturn's Rings, considered with Reference to the Solar Motion in Space; also a paper on the Meteoric Theory of the Sun.* By Lieut. A. M. Davies, F.R.A.S. (London: Longmans & Co.)

PROF. CLERK MAXWELL, in his remarkable essay "On the Stability of Saturn's Rings," which gained the Adams Prize in 1856, exhaustively examines the various theories of the constitution of these rings, and decides what are the impossible mechanical conditions for their maintenance and what is the possible one. He shows that they cannot be solid or rigid; he disposes of the possibility of their being continuously fluid, and he concludes that "the only system of rings which can exist is one composed of an indefinite number of unconnected particles revolving round the planets with different velocities according to their respective distances." Lieut. Davies appears not to have seen Prof. Maxwell's work, as he ascribes to the perusal of a derived exposition of it the enlistment of his interest in favour of the Satellite theory of the rings. Having espoused this theory, he has sought an explanation of Saturn's possession of a ring system in the supposition that the planet has picked up streams of meteors in its path through space; this path being a spiral resulting from the planet's orbital motion in conjunction with the proper motion of the solar system. The spirals traversed by the four planets beyond Mars are projected in accordance with Lieut. Davies's assumption of the solar motion, in order to show that Saturn is (excepting Jupiter) more favourably circumstanced than other planets for encountering wandering streams of meteors that are drawn towards the sun; while, from consideration of the masses and the distances of the two planets from the sun, it is argued that Saturn is better circumstanced than Jupiter for attaching such streams permanently to his system in the form of rings. The details of Lieut. Davies's work can only interest those who are closely concerned with cosmical hypotheses. We will merely remark that he appears to place too great faith in figures: he gives the hourly rate of the solar motion in space to a mile, and quotes the solar parallax to four places of decimals! The velocity is a very uncertain element of the solar motion, and a small alteration of the rate assumed by Lieut. Davies would greatly modify his conclusions. The book includes a paper on the meteoric theory of the sun, a theory with which the author is blindly enraptured. He claims that it "accounts for every phenomenon hitherto observed on the solar surface." He holds that the "willow leaves" are meteoric flights just falling into the sun; that the spots are spaces upon which no meteors are raining; that the periodicity of spots is due to the action of the planets in pulling "the meteoric matter outwards from the surface of the sun into larger orbits, thus temporarily delaying its precipitation," and that "the form of the spots bespeaks their origin as extraneous to the solar machinery. Were they cyclones in the atmosphere, they would invariably present a rotatory appearance . . . . This must result were the origin of the spots in a plane parallel to the tangential plane at the sun's surface; but would not do so if their origin lay in the normal to that plane, as it does in the meteoric theory. A careful study of Mr. Carrington's valuable series of observations of solar spots is decidedly unfavourable to the conclusion that they have forms of rotation." Lieut. Davies is either innocently or wilfully ignorant of the palpably cyclonic appearance which spots frequently present, and which has been frequently depicted by observers who have studied the characteristic features of individual spots. This study did not concern Mr. Carrington. The devotees of the meteoric theory of the sun's maintenance will not feel that it has been much advanced by Lieut. Davies's over-straining advocacy.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Eclipse Photographs

In his letter published in Nature on the 1st of June, and to which Mr. Brothers courteously replies in your issue of the 15th, the writer briefly touched upon four different points bearing on the value of the eclipse photographs. Those points are :—1st, The possibility in a comparatively cloudless sky of a luminosity akin to that represented under the name of the corona on page 370 of the number of Nature issued on March 9, but caused only by moisture in our atmosphere, as illustrated by his instance of what he termed a lunar halo ; 2ndly, The presence of a luminosity on what he apprehends should have been the dark disc of the moon, as represented in the photograph of the American observers at Cadiz ; 3rdly, The indifferent definition of the published photograph ; and 4thly, The evidence as presented by the photograph of the identity of the coronal rifts. Referring to the first of these points, Mr. Brothers "fails altogether to see the connection between the solar corona and a lunar halo." If the term halo, as applied to any appearance pertaining to the moon, is confined to the ring of light so frequently to be observed apparently surrounding the lunar disc, the writer would substitute the word "luminosity" for halo. The appearance he alluded to resembles Mr. Brothers's woodcuts of the corona already mentioned, more than anything else to which he can compare it, and in common phraseology may be described as a patch of light surrounding the apparent position of the moon, extending from it to a distance varying from about one degree to two or two and a-half, and having an irregular or rifted outline. The phenomenon in question was observed by the writer when the atmosphere was in such a condition that no trace of cloud whatever was visible for a distance round the moon of some thirty or forty degrees. He mentions this merely to show that even when no visible condensation of moisture is present, an appearance—attributable to nothing but atmospheric moisture, and analogous to what is termed the solar corona—is not to be regarded as out of the common, and nothing to be wondered at. Touching the second point, Mr. Brothers would seem to be of opinion that a solar corona may be seen even when the dark disc of the moon intervenes between it and the observer's eye ; for he says of the luminosity in question that if caused by our atmosphere it would extend all round and all over that disc. The point at issue here is a very simple optical question, in the discussion of which space would be merely wasted, and in reference to which the writer would simply reiterate the opinion he has already expressed, that the luminous appearance as seen extending on to the disc of the moon in the Cadiz photograph, is (if it were visible outside the camera) attributable to nothing but the influence of the terrestrial, or of a lunar atmosphere. Whilst speaking of the American observer's picture, he would remark in answer to two observations by Mr. Brothers, first, that he is not in "possession of exclusive information" concerning the circumstances of its production ; and secondly, that he does not assume that it was taken under conditions less favourable than those prevailing at Syracuse. He does, however, assume that that photograph either represents only the phenomenon to which the instrument used in its production was directed, or that it represents something in addition to that phenomenon. If its representation is confined to the phenomenon, then upon the grounds already shown, he considers that what is called the coronal light in the Cadiz picture not only may be, but most certainly is, in part at any rate, merely the result of atmospheric moisture. If, however, the American observers were unfortunate enough to represent in their photograph a luminosity not belonging to the eclipse at all, then he considers that what did belong to the eclipse is not distinguishable in their picture from what did not. In short, whether the Cadiz picture does or does not represent only what it should do, the writer is of opinion that any evidence it can afford respecting the identity of the coronal rifts must be other than *satisfactorily* conclusive.

Concerning the third point, namely the "indifferent definition," to which he directed attention, Mr. Brothers admits the validity of his remarks, so far as Syracuse picture No. 5 is concerned ; and this picture, it may be observed, Mr. Brothers would seem to consider the best of his series, inasmuch as it is the only one procurable of the opticians in Manchester, and also is the one selected for an engraved representation in the pages of Nature.

Respecting the last of the four points on which the writer has taken the liberty to remark, viz. the evidence afforded by the photographs of identity in the coronal rifts as seen at Cadiz and Syracuse, he is of opinion that he has already said sufficient to justify his observation that that evidence is not "satisfactorily conclusive." If, however, Mr. Brothers should entertain a different opinion, and the Editor of Nature think the matter worthy of further space, he will make a few other remarks, looking at the subject from an altogether different point of view.

In conclusion, the reader should be reminded that the subject under discussion is not the astronomical question—Is the sun surrounded by a medium which is illuminated by his rays and rendered visible under favourable circumstances to the eye of a terrestrial observer? but, assuming this to be the case—Is the luminosity indicated in the photographs under consideration a representation of that medium or is it not?

The writer does not doubt the existence of such a medium round the body of our great luminary (though, assuming it, ought there not to be some trace of it visible above the western horizon, immediately after sunset in a dry climate?), but he does doubt whether the patch of light depicted in the photographs of the late solar eclipse is in the main other than a phenomenon of terrestrial meteorology.

D. Winstanley

### The Solar Parallax

Mr. Proctor entirely misunderstands me if he thinks that my criticism on his account of the solar parallax had reference to any failure on his part to give prominence to the discussion between Mr. Stone and myself, or to correctly apprehend that discussion. To point out all the imperfections and inaccuracies in his account would take a whole column of Nature, and I have neither the time nor the disposition to make such a display of the accidental errors of a fellow-worker in the astronomical field. But, if Mr. Proctor desires it, I will constitute him judge in his own case in form and manner as follows : I will send him privately the list of specifications on which my criticism was founded. If, in his opinion, this list fails completely to sustain the proposition that his history of recent researches on the solar parallax is "imperfect and inaccurate in a remarkable degree," he is to publish it with any defence he chooses to make. Otherwise he may keep it for his own private use in case he brings out a second edition of his work. The kind spirit in which he has taken my remarks is highly appreciated, and I shall be happy to hear from him privately on the subject.

Simon Newcomb

### Halo in the Zenith

At 20 minutes to 6 P.M. on Saturday 10th inst., I witnessed a natural phenomenon which I understand is very unusual. It was a portion of a halo around the zenith. Take the zenith as centre, and with a radius of 20° describe an arc of 120° parallel to the horizon, and having 60° on each side of due west. This gives the position in the heavens, as nearly as I could determine, using no other means of observation except my eyes. The convex surface was red, concave blue. Other colours were also apparent, and were very distinct towards the centre of the arc of 120°, becoming much diluted at a greater distance. The horns of the arc gradually faded away amid the cirri with which the whole sky was covered, its blue colour being only partially obscured. You could tell at a glance that the zenith was the centre of the halo. The phenomenon only continued five minutes from the time I first saw it, until it disappeared altogether. It has been suggested to me that probably the azimuth of the sun and that of the centre of arc of 120° coincided. I think this is very likely, but I did not note it particularly at the time. The wind was easterly, and the sun shone brightly where I was standing. If any of your numerous correspondents would favour me with an explanation of the above I would be greatly obliged. No other halo or portion of a halo was visible at the time.

R. M. Barrington

Fassaroe, Bray, Co. Wicklow, June 26

### What is Yellow Rain?

The letter of Mr. A. Ernst in Nature, May 25, on a recent case of "yellow rain," possesses considerable interest to all who have paid any attention to early prodigies ; because his determination of the nature of that phenomenon, if true, serves to confirm the statements of others who have observed similar occurrences. The confirmation, however, only extends so far, as proving that, a yellowish substance has been seen on bodies after a fall of rain. I can mention instances where sudden appearances have taken place, and would be called *yellow* rain, without the aid of ordinary rain.

The earliest case I am acquainted with occurs in Pliny,* who says:—

"In like manner it rained iron in Lucania, the year before that in which M. Crassus was slain by the Parthians; and together with him all the Lucani, his soldiers, of whom there were many in his army. That which came down in the rain resembled in some sort sponges; and the aruspices gave warning to take heed of wounds from above."

This records two kinds of showers, viz., iron and sponges, but they were, I believe, one shower, although I have never seen any explanation of this curious passage. An account of a few "preternatural showers," I have lately come across, seems to contain exactly the information I have been seeking to clear up this difficulty. As it is novel, and was written in apparent ignorance of Pliny's record, it has at the present time some value. It says, in attempting to explain the cause of "red snow":—

"This singular phenomenon, which has been observed in the Arctic regions, seems to be owing to the presence of oxide of iron in a state of minute division, and also of a resinous vegetable principle of an orange-red colour, belonging apparently to some lichen, of which too, perhaps, the iron may form one of the immediate principles."†

This explains Pliny to a certain extent; in the common ignorance of both ascribing the redness of the snow to iron rust, or oxide of iron, and the matter that remained after the dissolution appearing of a sponge-like texture, and called a sponge or sponges by Pliny, and a resinous vegetable principle of an orange-red colour by Phillips.

This I think is the most probable meaning, and is, perhaps, the earliest "yellow" shower on record. The colour, as stated by the latter, is nearly correct, as shown by the investigation of MM. De Candolle and Prevost, who discovered, microscopically, that the red snow was due to the presence of small globules of a bright red colour, which were surrounded by a gelatinous membrane, transparent and slightly yellow, and were mixed with fragments of moss and dust. "An examination of the crimson snow found by Captain Ross in the Arctic regions by M. De Candolle proved it to be identical with the Alpine red snow; the globular bodies are of a vegetable nature, and were once thought to belong to the *Uredo*, but M. Bauer disproved this, and named the plant *Protococcus nivalis*. There are cases, however, in which the presence of animalcules gives a reddish tinge to snow."‡

Honeydew is mentioned by Pliny (Bk. XVIII. c. 28), who states that a great many of the ancients affirmed that dew burnt up by the scorching sun is the cause of honeydew on corn.

In the *Chronicum Scotorum* is the earliest direct record of a "shower of honey" I know of.§ It says: "A.D. 714 . . . . it rained a shower of honey upon Othan Bec. . . . ."

When it is known that any sudden appearance, giving a colour to the ground, or prominent places, or on trees, &c., is generally thought to have descended from above, this passage is quite intelligible. The "shower of honey" was nothing more than a "secretion of *aphides*," whose excrement has the privilege of emulating the sugar and honey in sweetness and purity."‖ Some contend that it is due solely to the exudation of the saccharine juices of trees; but, feasible as this may seem, it is not sufficient to account for this phenomenon, which often extends over very large tracts of land. If the exudation is promoted by the aphides, and the dew increased by their own excrement, then this explanation is, I believe, the true one. The former view is not to be discarded without some consideration; for one observer states that, in the course of thirty years he had attended to this subject, he had never met with any honeydew which did not seem to him to be clearly referable to aphides as its origin.¶ This view does not go counter to what I conceive to be the correct one; for exudations do take place, and the quantity of "dew" can be increased by the aphides.

* Bk. II. c. lvi.
† Lectures on Natural Philosophy, 1st series, by Montagu Lyon Phillips. London, 1839, pp 47-48.
‡ Vegetable Physiology, by Dr. W. B. Carpenter, 1858, p. 580.
§ This I take to be equivalent to "honeydew."
‖ Kirby and Spence's "Entomology," 1867, p. 119.
¶ Kirby and Spence, foot-note, p. 119.

There is a very curious account given in a now little known work of what was considered the real cause of "honeydew," but I will not trespass further upon the valuable space of this journal in quoting it. I give the title at foot.*

More can be said upon this interesting subject; and on another occasion I hope to resume the investigation, by attempting to explain the "yellow rains" of a different kind to those treated of in this letter. JOHN JEREMIAH

43, Red Lion Street

* Rohault's "System of Nat. Phil.," by John Clarke, D.D., vol. ii. p. 217. London, 1723.

### Black Rain

THE following notice of a shower of black rain, which has been sent to me by my friend Mr. G. J. A. Walker, of Norton Villa, near Worcester, though not so exact in its description as I could have wished, may call attention to the subject, and elicit a more detailed account, if in this ungenial season rain of a similar nature has fallen elsewhere. Mr. Walker's residence is about three miles south-east of Worcester, and he says, that after three or four hours of common rain on Tuesday June 6, it became suddenly dark about seven o'clock, P.M., and shortly after a rain like ink poured down for a quarter of an hour, after which light returned upon the scene. The following morning the sheep at Woodhall (an adjacent farm) appeared as if their fleeces had been dyed black; also the dog and a grey pony that Mr. Walker had out in a field close by appeared as if they had been rolling in soot or in a coal hole. The black matter brought down with the rain was of an adhesive nature, and at Littleworth, within a mile of Norton, where this rain fell into some tubs, it was observed to be as black as ink. This black rain was particularly remarked, as clear ordinary rain had been falling for some hours on the day mentioned, but had ceased an hour previously to the commencement of this black downfall. The actual rain of that evening did not extend to Worcester, but I have a note taken at my residence here at the time, that "the gloom was singular and overpowering all the evening." I regret that, going into Herefordshire the next day, I was not aware of this occurrence till some days after, and none of the black rain or the adhesive matter it brought down had been preserved for microscopical examination. EDWIN LEES

### A New View of Darwinism

I HAVE noticed that NATURE is very catholic in its sympathies, and allows all views which are not palpably absurd to be discussed in its pages, and I therefore venture to ask for some space in which to present a few of the difficulties which have been suggested by Mr. Darwin's theory of Natural Selection, and which have not, so far as I know, been as yet discussed. I have not the taste for the language nor the arguments which were used by a *Times* reviewer, and I have much too great a reverence for one of the most fearless, original, and accurate investigators of modern times, to speak of Mr. Darwin and his theory in the terms used by that very ignorant person. Approaching the subject in this spirit, and knowing how very small a section of biologists are now opposed to Mr. Darwin, I may be very rash, but hardly impertinent, in stating my difficulties.

I cannot dispute the validity and completeness of many of Mr. Darwin's proofs to account for individual cases of variation and isolated changes of form. Within the limits of these proofs it is impossible to deny his position. But when he leaves these individual and often highly artificial cases, and deduces a general law from them, it is quite competent for me to quote examples of a much wider and more general occurrence that tell the other way. In this communication I shall confine myself to Mr. Darwin's theory, and shall not trepass upon the doctrine of evolution, with which it is not to be confounded.

The theory of Natural Selection has been expressively epitomised as "the Persistence of the Stronger," "the Survival of the Stronger." Sexual selection, which Mr. Darwin adduces in his last work as the cause of many ornamental and other appendages whose use in the struggle for existence is not very obvious, is only a by-path of the main conclusion. Unless by the theory of the struggle for existence is meant the purely identical expression that those forms of life survive which are best adapted to survive, I take it that it means in five words the Persistence of the Stronger.

Among the questions which stand at the very threshold of the whole inquiry, and which I have overlooked in Mr. Darwin's books if it is to be found there, is a discussion of the causes which produce sterility and those which favour fertility in races. He no doubt discusses with ingenuity the problem of the sterility of mules and of crosses between different races, but I have nowhere met with the deeper and more important discussion of the general causes that induce or check the increase of races. The facts upon which I rely are very common-place, and are furnished by the smallest plot of garden or the narrowest experience in breeding domestic animals. The gardener who wants his plants to blossom and fruit takes care that they shall avoid a vigorous growth. He knows that this will inevitably make them sterile; that either his trees will only bear distorted flowers, that they will have no seed, or bear no blossoms at all. In order to induce flowers and fruit, the gardener checks the growth and vigour of the plant by pruning its roots or its branches, depriving it of food, &c., and if he have a stubborn pear or peach tree which has long refused to bear fruit, he adopts the hazardous, but often most successful, plan of ringing its bark. The large fleshy melons or oranges have few seeds in them. The shrivelled starvelings that grow on decaying branches are full of seed. And the rule is universally recognised among gardeners as applying to all kinds of cultivated plants, that to make them fruitful it is necessary to check their growth and to weaken them. The law is no less general among plants in a state of nature, where the individuals growing in rich soil, and which are well-conditioned and growing vigorously, have no flowers, while the starved and dying on the sandy sterile soil are scattering seed everywhere.

On turning to the animal kingdom, we find the law no less true. "Fat hens won't lay," is an old fragment of philosophy. The breeder of sheep and pigs and cattle knows very well that if his ewes and sows and cows are not kept lean they will not breed; and as a startling example I am told that to induce Alderney cows, which are bad breeders, to be fertile they are actually bled, and so reduced in condition. Mr. Doubleday, who wrote an admirable work in answer to Malthus, to which I am very much indebted, has adduced overwhelming evidence to show that what is commonly known to be true of plants and animals is especially true of man. He has shown how individuals are affected by generous diet and good living, and also how classes are so affected. For the first time, so far as I know, he showed why population is thin and the increase small in countries where flesh and strong food is the ordinary diet, and large and increasing rapidly where fish or vegetable or other weak food is in use; that everywhere the rich, luxurious, and well-fed classes are rather diminishing in numbers or stationary; while the poor, under-fed, and hard-worked are very fertile. The facts are exceedingly numerous in support of this view, and shall be quoted in your pages if the result is disputed. This was the cause of the decay of the luxurious power of Rome, and of the cities of Mesopotamia. These powers succumbed not to the exceptional vigour of the barbarians, but to the fact that their populations had diminished, and were rapidly being extinguished from internal causes, of which the chief was the growing sterility of their inhabitants.

The same cause operated to extinguish the Tasmanians and other savage tribes which have decayed and died out, when brought into contact with the luxuries of civilisation, notwithstanding every effort having been made to preserve them. In a few cases only have the weak tribes been supplanted by the strong, or weaker individuals by stronger; the decay has been internal, and of remoter origin. It has been luxury and not want; too much vigour and not too little, that has eviscerated and destroyed the race. If this law then be universal both in the vegetable and animal kingdoms, a law too, which does not operate on individuals and in isolated cases only, but universally, it is surely incumbent upon the supporters of the doctrine of Natural Selection, as propounded by Mr. Darwin, to meet and to explain it, for it seems to me to cut very deeply into the foundations of their system. If it be true that, far from the strong surviving the weak, the tendency among the strong, the well fed, and highly favoured, is to decay, become sterile, and die out, while the weak, the under-fed, and the sickly are increasing at a proportionate rate, and that the fight is going on everywhere among the individuals of every race, it seems to me that the theory of Natural Selection, that is, of the persistence of the stronger, is false, as a general law, and true only of very limited and exceptional cases. This paper deals with one difficulty only, others may follow if this is acceptable.

Derby House, Eccles HENRY H. HOWORTH

### Ocean Currents

MR. PROCTOR concludes his letter on Ocean Currents, in NATURE for June 15, with the remark that in theories respecting oceanic circulation "the vast distance separating the Polar from the Equatorial regions must not be overlooked." Will you allow me to point out to him that in the experiment he suggests, that vast distance is entirely overlooked; that, in fact, any such experiment, with whatever difference of detail it may be performed, whether in his cylinder or in Dr. Carpenter's trough, in no way illustrates the natural condition of things, and in no way tends to answer that objection to the "temperature" theory of currents which is founded on the infinitesimal nature of the thermometric gradients. The difference of temperature between Arctic and Equatorial water is about 50° F., or 1° F. in 100 miles; or, reducing it to smaller units suitable for an experiment, is $\frac{1}{500000}$ of a degree in one foot; this, if the experimental trough is five feet long, or if the cylinder is ten feet in diameter, gives an extreme difference of $\frac{1}{100000}$ of a degree of Fahrenheit's scale. Can such a difference be represented in any experiment? I think not; but no experiment which shows a much greater relative difference can be accepted as satisfactory; for it is the infinitesimal nature of the thermometric gradient existing in the ocean that constitutes the physical objection to the temperature theory. There are other objections which I will not allude to now; but it is manifestly no answer to this one objection to show that under certain other circumstances—which bear no resemblance in degree to those of nature—hot water and cold will establish a circulation. I, for one, have, for a good many years, been perfectly well aware that they will; but I doubt if it has ever been shown that a sensible motion will result from a thermometric gradient of $\frac{1}{500000}$ of a degree in a foot.

J. K. LAUGHTON

### Alpine Floras

THE fact mentioned in last week's NATURE of the absence of any Alpine flora on the Atlas Mountains, Morocco, though disappointing, is interesting. It seems to show that, during the glacial period, icebergs did not drift to the Atlas. This, however, must have been from local causes only. Mr. Wallace found a European flora on a mountain in the Eastern Archipelago—I think in Borneo—which, most probably, must have got there during the glacial period. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 19

### A Suggestion

Is it possible that the following facts may account for the presence of *Elastrus dolosus* in the Azores? At all events, I offer them as suggestive, and for the information of Messrs. Wallace, Godman, Murray, Crotch, &c.

Lawrence Almeida, son of the first Portuguese Viceroy in India, was the first European known to visit the coast of Madagascar in the year 1506. The Portuguese circumnavigated the whole island within two years, and subsequently constantly anchored at it in their voyages to the East Indies. They also established a settlement on a steep rock on the bank of the river Franchere and near the village of Hatore, in the province of Anosi (*i.e.*, at the south-eastern extremity of the island). The valuable timber, as ebony, as well as the rich dye-woods, would be well worth taking to Europe, and thus doubtless afforded a conveyance for living larval or pupal Elaters, without any rare or improbable concurrence of events, to the Portuguese islands in the Atlantic. Many of the extremely beautiful and attractive flowering shrubs and plants would also not improbably be forwarded to Europe by the same route, in which some Élateridæ might find shelter. Is the lapse of 300 years sufficient to account for change of development? S. P. OLIVER

Southsea

## HYDROUS SILICATES INFECTING THE PORES OF FOSSILS

DR. T. STERRY HUNT directed attention some time ago* to a remarkable limestone of Silurian age from Pole Hill, New Brunswick, in which I had found the

* Proceedings of the Natural History Society of Montreal.

cavities of fossil crinoids to be filled with a siliceous substance perfectly injecting their most delicate cellular structure, and which Dr. Hunt, on chemical analysis, found to be a hydrous silicate allied to jollyte. I have since, in examining with the microscope various specimens of limestone in the collection of McGill College, met with a British example of this kind of injection, to which I would wish to direct the attention of your microscopists. It is a specimen of olivaceous, imperfectly crystalline limestone, labelled Llangedoc, Wales. The only distinct fossil which it contains is a small body having the characters of the genus *Verticillopora*. It is filled, however, with crinoidal fragments and fragments of shells, and, when sliced, displays a few very minute univalves, probably of the genus *Murchisonia*, and also portions of a sponge-like organism with square meshes. The pores and cavities of many of these fossils are filled with a greenish or brownish finely crystalline silicate, which must have been introduced when the organic bodies were still recent, and which Dr. Hunt has ascertained to have the following composition :—

| | |
|---|---|
| Silica | 35·32 |
| Alumina | 22·66 |
| Protoxide of Iron | 21·42 |
| Magnesia | 6·98 |
| Potash | 1·49 |
| Soda | 0·67 |
| Water | 11·46 |
| | 100·00 |

So that this mineral is almost identical with jollyte. The fact that it fills the minute pores and cavities of the fossils can be seen in transparent slices, especially under polarised light, and also in decalcified specimens. The filling is not, however, so perfect as in the New Brunswick specimens above alluded to. The best, which I suppose to be Upper Silurian, is worthy of the attention of those who may have access to it, as presenting an interesting example of Silurian fossils preserved in the same way with the Laurentian Eozöon. It affords another palæozoic illustration of a mode of preservation of the structures of fossils, which, though perhaps more prevalent in the Laurentian and Cretaceous than in any intervening periods, is to be met with here and there throughout the geological series, and is of equal interest to the palæontologist and the chemical geologist. J. W. DAWSON

Montreal, June 8

## NEW THEORY OF SUN-SPOTS

A LATE number (1,835) of the *Astronomische Nachrichten* reproduces from the notices of the Royal Saxon Scientific Society a paper on the above subject by Professor Zöllner. The author believes that he is the first who has attempted to account for the periodicity of the spots by agencies confined to the sun itself, while he rejects the notion of planetary influence to which the phenomenon has been commonly attributed. In this, however, he is not quite correct, for in the April numbers of *Cosmos* last year there appeared a transcript of a paper read before the Belgian Academy of Sciences, by M. Bernaerts, who tries to explain the various phenomena of the sun spots without reference to any extra-solar action.

Prof. Zöllner, like M. Bernaerts, accepts the theory of a liquid forming the surface of the sun ; but while the Belgian *savant* considers the spots as perforations in the liquid layer traversed by downpouring currents of gases that had previously risen through the liquid from the gaseous nucleus, Prof. Zöllner believes the spots to be formations of slag or scoriæ caused by a certain local cooling of the liquid surface. Over this glowing liquid is a glowing atmosphere, which contains, in a vaporous state, a portion of the matter belonging to the liquid. The same as on the earth, if this atmosphere is cloudless and calm, radiation and cold are induced ; and where this occurs the slag-like products are formed, and spots become visible. But vaporous condensation is also a consequence of the cold. Clouds, therefore, are developed, the radiation is checked, the liquid surface regains its former heat, and the spots are dissolved and disappear ; so that the very cause that effects their formation also tends to their dissolution. The repetition of the same operations gives the spots the character of "intermittent phenomena ; " but their occurrence, as well as their duration, depends on such a complication of meteorological processes that those phenomena cannot be considered otherwise than as perfectly casual.

The action of a spot on the atmosphere in cooling it, and causing cloudlike condensations that oppose radiation and restore the heat, makes the presence of a great spot unfavourable to the formation of other spots, and Prof. Zöllner arrives at the conclusion that "a sun-spot exerts within a certain area, and according to its size, an influence that prevents or obstructs the formation of other sun-spots." Thus, it appears, he explains the *isolation* of the spots. But they occur also in groups over a wide extent of surface, and he infers that "the same conditions of the solar atmosphere that induce the formation of a spot in any one place, prevail in general over a larger space than that occupied by the spot, so that within the area influenced by those favourable conditions, the simultaneous production of other spots is more likely than elsewhere." The size of the spots depends plainly not on the amount of radiation alone, for the slag like products have cohesive properties like our ice-flakes.

I candidly admit that all this is by no means so very plain to me after reading the theory of the isolation of the spots ; and I would refer the reader to the original for a better understanding of the two theories relating to the isolation and the grouping, than I have been able to attain to. I would also refer to the original for the Professor's views of the oscillations of solar temperature and the periodicity of the spots, which he discusses in several paragraphs.

The appearance of the spots in certain zones on both sides of the equator he explains as the effect of currents in the liquid stratum. He asks us to imagine, in the first place, a motionless, atmosphere-enveloped globe maintained at a constant high temperature ; and, after explaining the results, he tells us to fancy such a globe with a liquid envelope heated at bottom by contact with the surface beneath it, and cooled above by radiation. The lower parts of the liquid have a tendency to rise on account of their lower specific gravity, but their ascent anywhere is impossible unless somewhere else a sinking takes place. With equal conditions everywhere prevailing, no motion in either direction could occur ; but those equal conditions do not exist on the sun, whose axial rotation diminishes the force of gravity at the equator. This therefore favours an ascent of the heated lower portions of the liquid at the equator, and a sinking of the cooler upper parts in the regions of the poles. Two streams are thus induced ; one below flowing toward the equator, and one above in a contrary direction. The former as it progresses gains in temperature by contact with the hot surface of the globe ; while the latter in its sub-aerial route loses heat by radiation. Thus the polar regions of the sun are made cooler than the equatorial, as has, in fact, been shown by Secchi's investigations.

These movements in the enveloping liquid (*flüssigen Umhüllungen*) are the cause of atmospheric disturbances, producing in certain places a lowering of temperature and condensation. The fall in temperature is favoured in two ways—by the mixing of the equatorial and polar streams in high latitudes, and by the ascent of an air-current at the equator. As this air-current cools in rising its vaporous constituents are partly condensed in the form of clouds. Yet these clouds need not at all be of so low a temperature as to appear to us like darkened areas; but, on the contrary, when we consider the high temperature of the sun, we may conceive them to be formed of matter in a glowing state, so that products of condensation such as these could scarcely, if at all, be perceived on the luminous disc of the sun. On the other hand, the author believes that in the cases of the great and still warm planets, Jupiter and Saturn, we see the sun-illumined aqueous clouds that rise in bright belts at the equator. We believe the author's object is to show that, while the visible effects of condensation appear in the atmospheres of Jupiter and Saturn, it is only on the liquid surface that they are exhibited by the sun, and thus its atmosphere remains transparent.

We have, accordingly, in the equatorial zone and in the higher latitudes distinct regions of preponderating atmospheric cloudiness, and between them, like the zones of the trade winds on the earth, lie areas of relative clearness. All this, if I rightly understand the author, is not apparent to the observer, but its effects are seen on the glowing liquid solar surface, where, beneath the unclouded areas, radiation is more induced than in other places, and the formation of sun-spots is the consequence.

To the foregoing causes of atmospheric disturbance must be added the eruptions of hydrogen that are shown by the spectroscope.

On the whole it is the *stillness* and *clearness* of certain parts of the atmosphere that induce the formation of spots; and, as the final result of his arguments, Prof. Zöllner sums up as follows:—

"The sun-spots are slag-like products of a cooling process caused by the radiation of heat from the glowing liquid surface, and they dissolve again in consequence of disturbances of equilibrium in the atmosphere which are brought on by themselves. If these disturbances are not merely local, but of more general extent, then, at the times of such general atmospheric disturbances, the formation of new spots is but little favoured, because the essential conditions of a considerable lowering of temperature are wanting, namely—*stillness* and *clearness* of the atmosphere. When the atmosphere, after the dissolution of the spots, gradually tranquillises, the process begins anew, and it assumes a *periodic* character, while the conditions of the solar surface are to be regarded as constant in the *mean* of lengthened periods. The local distribution of the spots must, according to this theory, depend on the zones of greatest atmospheric clearness, which, as has been shown, are generally coincident with the zones of the greatest development of the spots."

Such, as they appear to me, are Prof. Zöllner's views of the sun's spots; and if, as is quite possible, I have not everywhere succeeded in comprehending him, I freely admit that any misconstruction I have made may be attributable to my own shortcomings rather than to his. At the same time I cannot but regard his style as considerably difficult and diffuse, and not perfect in the logical concentration which is so necessary for the clear enunciation of a theory. In some points his conclusions seem, undoubtedly, to agree with observations—for instance, as regards the vaporous masses that are formed over the spots, and which appear sufficiently attested by their strong absorption lines in the spectrum. In the main, however, I cannot say, so far as I may venture to give an opinion, that he has been more successful than other theorists on the same subject; and among several objections which have occurred to me as affecting his views, I will venture to state the following:—

1. Regarding the establishment of currents in the liquid envelope, Prof. Zöllner affirms that in this way alone could the more heated and specifically lighter portions of the liquid at the bottom make their way to the surface; but this appears to me incorrect when we consider that in freezing water there is an interchange of the upper and lower strata until congelation begins, and this without the intervention of currents.

2. He defines the spots as scoriaceous products floating on the liquid surface. The liquid, however, is moving in a current from the equator poleward, and, if so, I would ask how is it that the spots show no tendency to be carried along with it in that direction? I do not believe that any such general tendency has been observed.

3. He makes no attempt to account for the very striking and suggestive appearances of the penumbra, which led Wilson to regard the spots as openings or depressions in the photosphere. Neither does he try to explain the distinct boundaries of the nucleus, the umbra, the penumbra, the light bridges, nor the deeper shading of the penumbra round its exterior limits.

4. The current cools in its advance poleward, and the polar regions are, as the professor tells us, the coolest parts of the sun. Then if, according to his theory, the spots are the products of cold, why do they not increase in development up to the poles? He assumes, indeed, that the cold induced in the polar regions produces clouds in the atmosphere, which are unfavourable to the production of spots; but they are so only as they check radiation and contribute to heat, and if, notwithstanding this, the polar regions are still found to be the coldest, and if cold is the cause of the spots, there seems a defect in the hypothesis. The professor points to an analogy between the spot zones and the clear zones of the trade winds of the earth. The cases are, however, very different; for our own atmosphere is subject to external influences, namely, the action of the sun, as well as those belonging to the earth itself.

Of course, anything from the eminent pen of Prof. Zöllner must be received with the utmost respect, but I conceive that his theory of the sun-spots, as I have attempted partly to show, presents many difficulties; and I cannot avoid stating my humble belief that, notwithstanding all that has been thought and written on the subject, and in spite of the modern discoveries of some of the constituent elements of the sun, we are but little nearer a true conception of its organisation or economy than the theorists of the days of Hipparchus.

J. BIRMINGHAM

## *PROFESSOR TYNDALL ON THEORIES OF DISEASES*

WE reprint this article from the *British Medical Journal*, since it shows how closely connected are the most abstract inquiries with the most practical questions:—

"The surest basis for Medicine is upon the broad foundations of exact scientific observation; and we shall all welcome such contributions as so able a physicist as Professor Tyndall can make either to our knowledge or to our facilities for testing the foundations of our beliefs. The electric beam, which has in his hands played a large part in many able investigations and demonstrative experiments, was lately brought into play to demonstrate the ubiquity of dust in the atmosphere. To some very charming experiments Mr. Tyndall joined some theories which, if capable of proof, were yet not demonstrated by anything which he said or did. The ubiquity of these air-borne particles was perfectly well known, and their illumination by the electric beam, while it has given a more complete demonstration of their presence than was otherwise obtainable, has not added anything to our knowledge of their chemical or biological relations. His experiments, however, have had the valuable effect of demonstrating the uses of cotton-wool as a filter for them, and the advantage of inspiring him with interest in a subject in which we can but be pleased that one of the most brilliant of investigators and expositors of physical science should be interested — the investigation of the origin of zymotic disease.

"In many respects the address shows a considerable advance over previous discourses by the same lecturer on this subject. The fact that the dirt or dust is in large part inorganic and in large part 'dead,' is now put prominently forward. Prof. Tyndall has, in fact, profited greatly by the lessons of Dr. Gull—we shall not venture to assume that it is by anything which we have had to say by way of comment upon his previous addresses—and does not now assume to tell us anything more about the nature of this dirt than we knew before. He proceeds only to reason upon the subject, deriving his information, however, chiefly from chance communications from various physiologists and medical correspondents. One correspondent tells him that 'blood free from dirt' will take longer to putrefy out of the body, and Von Recklinghausen's experiments are brought in reinforcement of the still more striking results and experiments of Prof. Lister; another informs him that vaccination through a bleb raised by blistering is less likely to produce secondary abscesses than by the ordinary method; and Dr. Budd assures Prof. Tyndall that, 'from the day when he first began to think of these subjects, he has never had a doubt that the specific cause of contagious fevers must be living organisms.' The last is, of course, a very interesting proof of early wisdom, but is not of the nature of a strict demonstration. The circumstance mentioned by Mr. Ellis reminds us that we have, on the other hand, seen it stated in print by one gentleman that he had to abandon vaccination by blistering because it was, in his practice, more productive than any other of suppurative and inflammatory accidents. But all this is really beside the question. The whole course of subcutaneous surgery, the whole range of Prof. Lister's experience, the daily experience of the difference in progress between simple and compound fractures, a thousand facts and observations, and the accepted and proved theories of surgical practice, have long convinced every surgeon that, in proportion as air and that which air bears are excluded from the fluids of open wounds, and from the organic fluids of the body, suppurative and putrefactive processes will be lessened and warded off. So much Prof. Tyndall might, so far as our profession is concerned, have taken for granted; and if he chooses to read, for instance, such papers as those we have published of Adams on subcutaneous osteotomy, he will see how largely this knowledge affects our practice in other directions than those to which he has referred. But, after proving to us what we know, Prof. Tyndall takes a leap, and assumes precisely those conclusions which we are desirous of his aid in testing. All these facts are as much accordant with the doctrines of Liebig and the experiments of Bastian, as with the doctrines of Schwann and the experiments of Pasteur. Granted that air-borne particles are prime agents in initiating putrefactive and fermentative change, is this by a development of pre-existent living germs, a growth of deposited ova, or by a communicated molecular motion of dead organic matter in a state of change? Is it from germs or from fermentative organic particles? We wish we could see that Prof. Tyndall had advanced our knowledge at all concerning this, the central knot of the tangle. It does not help us when he quotes certain known examples of parasitic disease, such as arises from pébrine. Because the itch is the result of the activity of the acarus, it does not follow of course that all skin-diseases are parasitic. Mr. Tyndall declares indeed, that the successful workers and profound thinkers of the medical profession are daily growing more convinced that 'contagious disease generally is of the same parasitic character' as the silk-worm disease. We cannot find on what he bases that very broad statement. Where are the works of the 'most successful workers and profound thinkers' which support that statement? It will be very kind of the lecturer to inform us whom he thus dignifies, and to what growing series of authorities he refers. Certainly not to the researches on cholera of Gull, Baly, or Cunningham and Lewes: these negative the parasitic theory. Salisbury started a parasitic theory for measles, but his observations have been generally discredited, if they were ever accepted. Hallier's observations have certainly not gained in authority by the results of many recent investigations such as those of Burdon Sanderson. We are not aware of a parasitic theory of scarlet fever being held by any one. The theory concerning typhoid fever, which Dr. Budd holds strongly and defends ably on purely logical grounds, is as distinctly controverted by Dr. Murchison.

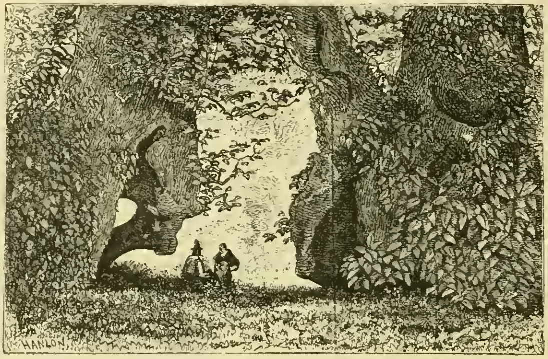
"Prof. Tyndall, however, lays just stress upon one important aspect of the question, which is precisely that which has long fascinated medical observers, and which is of the deepest importance. To it also, however, he adds nothing; and from it he draws, with admirable and unquestioning boldness, precisely the conclusions as to which we have all been debating whether they be the true and only conclusions. Small-pox and scarlatina are, to use the graphic words of Miss Nightingale, in ordinary medical experience, 'dog and cat,' so that one cannot change into the other any more than Tabby can give birth to Fido. When she says that she has seen with her own eyes one or other spring up in a corner of a room from neglected dirt, Miss Nightingale uses, of course, a purely figurative language, and her evidence must be taken *quantum valeat*. But when Mr. Tyndall declares, on the other hand, that zymotic diseases are all of primal inheritance—long descended primeval germs, never changing, never dying out, and ever passing on by lineal descent—he treads also upon ground less secure than he supposes. That this is the ordinary observed mode of extension of contagious diseases no one will dispute. That they have no other many will dispute. When he declares that, for the similarity or identity of effect of like particles acting on like fluids, we have no physical parallel, he obviously leaves out of view the whole series of phenomena of crystallisation from saturated fluids.

"To sum up: The tendency of modern research is certainly not so favourable as Mr. Tyndall believes and expects it to be to the theory of the parasitic origin of contagious disease. We should rather declare it to be unfavourable to that theory. The theory of the permanency and unrelated individuality of zymotic types of disease is not, as he assumes, an undisputed or unquestioned theory. We have to set against it, first, the theory of the correlation of zymotic diseases, which is growing into importance, and likely to attract more attention now than heretofore; second, the observations of statisticians of the complementary character of epidemics of zymotic diseases, and their apparent interchangeability in periods of decline; the theories of the spontaneous origin of zymotic disease by no contemptible observers, and in diseases as distinctly communicable as typhoid; and the observations, experiments, and reasonings of Pouchet and of Bastian, which have not yet been met, and which cannot be disposed of by a few words of philosophic doubt. We appreciate very highly the value of Prof. Tyndall's assistance in solving these questions. We entirely concur in his opinion that, as a physicist, he has a great power of usefulness in this field of investigation; and, if we refer him to the work of Gull, Baly, Cunningham and Lewes, Farr, and Murchison, it is because we are desirous that he should not be content to win easy triumphs with audiences uninstructed in the questions he discusses, or with the partisans of the theory he has adopted, but that he should enter into the heart of the question and face its real difficulties. It would be infinitely satisfactory if we could all arrive at as simple a sole theory of disease as that which Prof. Tyndall accepts entire, symmetrical, and rotund, from the supporters of the germ-theory; but we fear the solution is not yet in hand. It is satisfactory to have enlisted his sympathies, and we shall all be glad of his solid and sincere assistance."

## *THE CHESTNUT TREE OF MOUNT ETNA*

THE traveller in Sicily will recollect the little village of Giarre, about half way between Messina and Catania; and since the opening of the railway between these cities, with a station at no great distance from its principal street. On the sea-side below the town is the shipping port of Riposto, and between Riposto and Giarre lies a fertile plain, rich in olive and vine-yards. Giarre itself has not much to boast of, except perhaps it might do so of the glorious views to be seen from the slight elevation on which it stands. One long principal street, a large plain chapel, a very second-rate inn, and then there is nothing more to be said of the village. It is, however, the nearest town with an inn to the famous giant chestnut tree of Mount Etna, and as such is visited by tourists. This fine old tree grows in the Bosco or woody region close above the town and on the slope of Mount Etna. A narrow, steep road, gradually ascending, leads from Giarre to La Macchia, the broad bed of a river now (in the end of May) rolling down nought but clouds of dust, is passed, and the lava beds formed by the eruptions of 1689 and 1735 are traversed, and at last S. Alfio is reached. This village is about four and a half miles from Giarre, and from it a very fine view of Etna is obtained. The mountain, however, from this side looks low and by no means as imposing as when seen from the sea. A little beyond S. Alfio the road turns to the left still leading upwards; until all of a sudden the giant tree breaks upon the view, the road itself running through its very midst. It stands about 4,000 feet above the sea level, and it requires a good three and a half hours to walk to it from Giarre.

It has been calculated that this tree is about 1,000 years of age. It is a tree, therefore, old enough to have its early history lost in myth; but still it has its story; and this story tells us that long ago a certain Queen of Aragon was passing by this way, when, from the effects of the weather, she and her suite, which consisted of one hundred mounted persons, took shelter under the shadow of its trunk and boughs, and so to this day and from this fact it is known as the Castagno di Cento Cavalli. This story is said to be generally believed, and, at any rate, does not appear to have been much discussed. Not so the tree; and very many opinions may be quoted all more less differing as to its age and size. Some believe, or have believed, that the tree was as large as the story tells us it was, that the interior of the vast trunk has since then decayed away; leaving a number of separate pieces, each large enough to form a big tree, which pieces are covered with bark only on their outer surface. Others assert that there were here several large trees, more or less joined together, and demonstrate on the pieces of these trees still standing the barky layers surrounding the whole of their stems.



THE CHESTNUT TREE OF MOUNT ETNA

Not very long ago there were still four pieces standing, each of them of the dimensions of a very large tree. In the space surrounded by these pieces stood a hut, in which the annual crop of chestnut fruit was stored. One of these trees, or portions of the tree, has since disappeared. The hut has now been removed, and the road, sufficiently wide to allow of a carriage, runs between the remaining pieces and over the ground on which the hut was built. As you approach, one large piece of the tree is to the left-hand side of the road, and two larger pieces are to the right. It is very probable that many of the pieces believed to have belonged to the one original vast stem, were really stems themselves of independent trees, and such would appear to be the case with the large trunk to the left of the present roadway. But there is a strong probability that the two immense pieces to the right of the road were at one time united, and that they form part of the original tree. The annexed woodcut is from a photograph of these pieces. Both of them are deeply hollowed out. The base of the trunk to the right of the woodcut is very much decayed away, and several men could shelter in it; and the portions of the stems seen on looking at the picture are devoid of true bark. If these two portions once formed a single stem, then, indeed, though it might not have thrown a shadow sufficiently large to shade a hundred horsemen, yet it must have been a very giant among all the forest trees. Even now, in its decadence, the three stems are objects of sufficient interest to lead us to ask for them the reader's attention. E. P. W.

## *SCIENCE IN PLAIN ENGLISH*

II.

IN considering the importance of Technical Education with reference to the practical arts, and the claims of Science as an element of culture, we are led to study the methods of teaching.

It has been the custom in English to borrow the technical terms of Science from the so-called "learned languages," particularly Latin and Greek. To such an extent has this been carried that unless a term bears the marks of such a derivation it is hardly recognised by the public as a technical term.

The Germans, on the other hand, in teaching science, employ their own language to a large extent, and impose a definite scientific meaning upon common words. It is a remarkable evidence of the formative power of the German language, that it should have been able to produce an imitation of the systematic chemical nomenclature of the French school so complete that it is used in Germany as familiarly as the original system is in France and England. The fact that the most cultivated nation in the world, the Germans, find that they can teach science in their vernacular, deserves the most careful consideration—it seems to furnish at once an argument and an example.

In discussing this question, it is necessary to examine the capabilities of the English language with reference to the purposes of public instruction, but in the first instance we may glance at the development of scientific language in some of the countries of Europe during the last three hundred years.

When the progress of science rendered it necessary to employ new terms in order to express new ideas, two different methods were adopted by different nations. Let us compare the French and English with the German and Dutch method. When, for instance, the French wanted to express in one word the "knowledge of the stars" or the "study of the stars," they borrowed the Greek word *astronomia*, and called it *astronomie*. In like manner the English said *astronomy* but the Germans expressed the meaning of the term in their own language, calling it *Stern-kunde* or "star-knowledge;" similarly the Dutch said *Starre-kunde*, and they have continued using such words to the present day.

In English, then, it has been the custom to take Latin or Greek compounds ready made, although in many instances we might have translated them if we had chosen to do so. If we look to the literal meaning, the original difference between *sphere*, *globe*, and *ball* is that the first is Greek, the second Latin, and the third Saxon. So the Germans call a *hemi-sphere* a "half-ball," and the *globe* upon which we live the "earth-ball."

Now if, in any language, the compound words are, to a great extent, derived from other tongues, such words will be comparatively unintelligible to those who are not conversant with foreign languages. In such a case, the common people will learn the words rather by practice and association than by any exact knowledge of the original meaning; and to the same degree the learned must enjoy a privilege which the illiterate do not possess. Hence there is, in English, a broad distinction between the speech of the general public and the language of science.

Suppose that a working man, in this country, wishes to study Botany, he cannot read one of the ordinary works on that subject, without having his attention distracted by scores of new words which are either Latin or Greek, or else are derived from those languages. Thus he is often disheartened; or, if he succeeds, it is a long time before he overcomes the check which he experienced at the outset, and many a likely student is thus discouraged at the very threshold of his studies.

But the German writers, when they make books for their people, proceed upon a different plan. For if they give the Latin and Greek terms, it is only in brackets, and by way of parenthesis; while in the body of the work they use plain German words, and keep on employing such terms throughout the whole work. Hence, at the first reading, a German youth may go straight on, without paying attention to the Latin terms, and so make himself master of the facts. Afterwards, at a second or third reading, he may study the learned terms, which are repeated in brackets, from time to time, in order to catch the eye of the reader, and thus imprint themselves upon his memory.

For instance, in describing the parts of a flower, the writer does not begin by talking about a "calyx," but speaks of the *cup* (calyx), and calls the leaves of the cup *cup-leaves* (sepals). Similarly, the "corolla" is the *crown*, and its leaves are *crown-leaves* (petals). Thus, when he wishes to tell the learner that a *crown* (corolla) has several leaves, he does not tell him that the "corolla is polypetalous," but that the "crown is many-leaved," or that the "crown-leaves are many."

There is a danger that some of the terms thus employed may not be quite accurate. But the Germans are willing to risk the chance of misapprehension for the sake of making an impression on the mind of the reader, and gaining his attention. If then, half a loaf is better than no bread, it seems more advisable that an unlettered man who wishes to study science, should go through a book which is intelligible, though not absolutely accurate, rather than attempt to read a treatise which is admirably correct, but so full of hard words, that he is tempted at every line to throw down the book in despair.

We have to consider whether such a method can be carried out in English. There can be no doubt that the public would welcome any proposal for the publication of elementary scientific works written in a simple style. Some steps have already been taken in this direction, and scientific writers appear to be cautiously feeling their way. But the plan has not been carried out systematically or boldly, nor has the language of science been fully examined in reference to popular instruction.

Nor are scientific men entirely convinced that the proposed simplification is practicable, or even desirable. Some of them deny that the English language is equal to the task, because we have lost that power of making compound words which confessedly existed in Old English, and which still exists in German. Others contend that even in German the method is characterised by want of precision, and gives rise to confusion; hence they maintain that it is better to frame the scientific terms in words which are not familiar to the common ear, in order to ensure precision and to guard against error.

But in arguing this question two points may be observed. First—That men of high attainments are less averse to the proposed method than men of inferior ability. Secondly—That all are more disposed to see it tried in some other science than in the one which they themselves profess. The grammarian, or the mathematician would not greatly object to a plan for simplifying Botany; "for that," they say, "is a science of hard names." But the botanist replies, "No, you must not touch Botany; suppose you were to try Mathematics." The argument cuts both ways. It is evident that each acknowledges the value of fixed technical terms in his own science, and yet is not unwilling to see a simplification introduced in other branches of study.

The objections urged against the proposed method are of two kinds:—(1) that the system itself is misleading, and the method inaccurate; (2) that even if the plan be practicable in German it is not possible in English.

We shall, in the next article, review these objections in their order.

WM. RUSHTON

## NOTES

WE are glad to learn that steps are being taken to bring about such a general application from men of science to the Government for further deep-sea explorations as we referred to some little time ago. This is as it should be. We hear also, that, on the invitation of some of the leaders of science there, Mr. Gwyn Jeffreys will proceed to America in the middle of August, to inspect, in company with Prof. Agassiz, the collections obtained in the American dredgings. Such a proceeding will be of the utmost value to science, and no one is more fitted than Mr. Gwyn Jeffreys to perform such an important work.

As we are going to press we hear of the death of M. Claparède, one of the most distinguished naturalists whom Switzerland has produced. We hope in a future number to give an account of his labours, now unfortunately ended at an early age.

THE "Scheme of Education" Committee of the London School Board have sent in their report recommending science teaching in primary schools. We shall take an opportunity of referring to it on a future occasion.

FORTY of the teachers under the Science and Art Department are now in London for the purpose of undergoing practical training in Biology. For the last two or three years chemical courses of a similar nature have been given. These, however, have been suspended this year. It is impossible to over-estimate the good which is being done by the department in this way, and the Government deserves the best thanks, not only of every scientific man, but of every one who cares for the best interests of the country, for what they are doing. There will be similar teaching in Physics next month.

AT a meeting of the Senate of the University of London held on Monday last, Sir Edward Ryan was elected to the office of Vice-Chancellor *pro tem.*, in the place of the late Mr. George Grote.

NOTWITHSTANDING the alleged increased severity of the Matriculation examination at the University of London, and the large proportion who have failed of recent years, the number of candidates presenting themselves at the examination held during the present week is larger than in any previous year, being over six hundred.

THE Ladies' Educational Association has now been definitely connected with University College, London, where all the lectures will in future be held, an arrangement which will doubtless be of great advantage to both students and teachers. The Association has already issued its syllabus for the Michaelmas and Lent Terms of next winter session. The curriculum includes courses for ladies by the professors of Latin, Hebrew, English, French, Italian, German, Philosophy of Mind and Logic, Jurisprudence, Hygiene, History, Constitutional Law and History, Mathematics, Physics, Practical Chemistry, Geology, and Architecture. There will also be classes for Drawing and Painting in connection with the Slade School of Fine Art; and, during the Lent term, Prof. Oliver will deliver a course of ten lectures on the Structure of Plants and General Phenomena of Vegetation, with reference more especially to the general bearing of vegetation upon landscape.

DR. HOOKER and party returned last week from their visit to North Africa. The number of species of plants brought home is estimated at about 1,200, among which it is expected there will be a considerable number entirely new.

A MEETING of the friends and pupils of the late Prof. Goodsir was held in Edinburgh, in June, 1867, under the presidency of Dr. Dunsmure, and it was resolved, "That steps be taken to form a lasting memorial of Prof. Goodsir's distinguished career as an original investigator, and teacher of Anatomy and Physiology, and that the most appropriate manner of commemorating Prof. Goodsir's services, was to establish in the University of Edinburgh a Fellowship in Anatomy and Physiology, to be called the Goodsir Fellowship." A Committee was formed, and subsequently added to, to collect subscriptions, and to decide as to the conditions on which the Fellowship should be awarded. Honorary secretaries were appointed in various parts of the country, and in the Colonies. It was expected that a sufficient fund would have been collected within two years to found the Fellowship. At the present date, however, not more than 620*l.* has been subscribed. The hope of establishing an endowment in the University on the scale of a Fellowship has, therefore, been abandoned, and it is now proposed to institute a Scholarship in Anatomy and Physiology. In order to carry out this project worthily, it is necessary to raise the sum already collected to 1,000*l.*, and renewed efforts are accordingly being now made to provide this amount.

THE Harveian oration on the Progress of Therapeutics was delivered last week in the Royal College of Physicians by Dr. T. K. Chambers, after which the biennial Baly medal for the most distinguished researches in physiological science prosecuted during the past two years, was presented by the president to Dr. Lionel S. Beale.

THE Royal Agricultural Society has decided on appointing a Consulting Botanist, at a salary of 100*l.* per annum, the engagement to be an annual one. It will be the duty of the botanist to examine plants, seeds, &c., for members of the society, and to report the principal work performed from time to time for its members, and to undertake the work at fixed rates, to be arranged before his appointment, and to furnish papers to the Journal on special subjects of botanical interest. The appointment of an Entomologist to the Society has also been in contemplation.

AN International Congress, for the progress of Geographical Science, will be held at Antwerp from the 14th to the 22nd of August. A number of questions in Geography, Meteorology, Navigation, Ethnology, &c., will be submitted for discussion. An exhibition will also be held of objects connected with the purpose of the Congress, maps, plans, instruments used in navigation, &c., and prizes will be awarded for the best object exhibited in each class.

THE excursion of the Geologists' Association to Yeovil and neighbourhood on the 29th of May and three following days was a very successful one. Near Yeovil Junction station the Yellow Micaceous sands, considered by Dr. Wright to be of Upper Liassic Age, were examined, and characteristic fossils obtained. The large collection of Mesozoic fossils collected by the Rev. Edward Bower, at Closworth Rectory, was inspected, as well as that of the Rev. T. C. Maggs, of Yeovil. The next morning, under the guidance of Prof. Buckman, the party ascended the fine escarpment of Babylon Hill, where bands of concretionary sandstone contain fossils essentially Oolite and not Liassic. The characteristic fossils of the well-known "Cephalopod-bed," considered by Prof. Buckman to be at the top instead of at the base of the series, as generally supposed, were obtained at the celebrated Half-way House Quarries, and at a small but very prolific quarry on the Professor's own estate at Bradford Abbas. The following day the far-famed quarries of Ham Hill were visited, and the great bed of Inferior Oolite Freestone, which has supplied material for the churches and other buildings of the district for centuries, was carefully examined. The Middle and Upper Lias were also investigated at South Petherton; and on the fourth day the interesting and picturesque Keuper Cliffs at Seaton were examined, and the coast section followed until the "Landslip" was reached, extending for a distance of about six miles.

THE thirty-seventh Anniversary Meeting of the Statistical Society was held on Thursday, the 22nd of June, Mr. William Newmarch, F.R.S., president, in the chair. The following is the list of president, council, and officers elected to serve for the ensuing twelvemonths, viz.:—President—Dr. William Farr, F.R.S. Council—Dr. T. G. Balfour, F.R.S., R. Dudley Baxter, Samuel Brown, Dr. Hyde Clarke, L. H. Courtney, W. Fowler, M.P., F. Galton, F.R.S., Robert Giffen, Rt. Hon. W. E. Gladstone, M.P., W. A. Guy, M.B., F.R.S., Archibald Hamilton, J. T. Hammick, F. Hendriks, J. Heywood, F.R.S., F. Jourdan, Prof. Leone Levi, Sir Massey Lopes, Bart., M.P., W. G. Lumley, Q.C., J. MacClelland, Dr. F. J. Mouat, W. Newmarch, F.R.S., R. H. I. Palgrave, R. H. Patterson, F. Purdy, W. H. Smith, M.P., T. Sopwith, F.R.S., Col. W. H. Sykes, M.P., F.R.S., Ernest Seyd, W. Tayler, Prof. Jacob Waley. Treasurer—J. T. Hammick. Honorary secretaries—W. G. Lumley, Q.C., F. Purdy, Jacob Waley.

THE Winchester College Natural History Society, founded on March 12, 1870, has just issued its first Report, which includes some useful papers, and botanical, entomological, and palæontological lists of the neighbourhood. It gives promise of good and useful work to be done in future years.

WE have received the thirty-eighth annual report of the Royal Cornwall Polytechnic Society. As might be expected from the locality of the society, the majority of the papers bear on subjects connected with mining and metallurgy; though there are also some meteorological tables, and a useful list of addenda to the fauna of the county. A marked feature of the report is the number of woodcuts illustrative of various adaptations of machinery, &c., connected with the subjects of the papers.

DR. LAUDER LINDSAY has reprinted his essay on the Physiology and Pathology of Mind in the Lower Animals, in which he insists that the mind of the lower animals does not differ in kind from that of man; and that they possess the same affections, virtues, moral sense, and capacity for education, and are liable to the same kinds of mental disorders.

MR. W. ROBINSON, author of "The Wild Garden," "Alpine Flowers for English Gardens," &c., publishes a useful Catalogue of Hardy Perennials, Bulbs, Alpine Plants, Annuals, Biennials, &c., intended as a help to exchanges between cultivators of hardy plants, analogous to those that have long been common among botanists.

WE have received Nos. 203-206 of the "Bücher-Verzeichniss" of Friedländer and Son, of Berlin, comprising the following subjects—"Geology, Mineralogy, and Crystallography," "Botany," "Zoology," and "Mathematics, Physics, Astronomy, and Technology."

MR. PENGELLY has reprinted two papers read before the Devonshire Association for the Advancement of Science, Literature, and Art, "On the rainfall received at the same station by gauges at different heights above the ground," and "On the supposed influence of the moon on the rainfall," in which he thus sums up the conclusions arrived at:—"1. That under unobjectionable conditions, and at the same station, less rain will be received by a gauge high above the ground than by one nearer the surface; 2. That the total defect will increase with increase of height; 3. That the defect will not increase so rapidly as the height." And again:—"The result of my observations then may be briefly summed up thus: At Torquay, the second quarter of the moon, or that which terminates on the day before each full moon, had the least number of wet days, the heaviest average daily rate of rain, and the greatest aggregate rainfall; whilst the third quarter, or that commencing on the day of each full moon, had the greatest number of wet days, the lightest average daily rate of rain, and the second greatest aggregate rainfall. The differences are but slight; but it must be borne in mind that the moon's meteorological influence can be but slight. The results, however, do not accord with any of those mentioned by the authors so largely quoted at the commencement of this paper, yet they are such, and only such, as are calculated to induce any one to pause before giving an opinion for or against the alleged connection of the moon with our rainfall. Perhaps I cannot better conclude than by echoing the words of M. Arago, 'The subject requires to be examined afresh'"

IT is reported that about June 7, an earthquake took place on the south coast of Asia Minor, opposite Rhodes, resulting in the almost total destruction of the small town of Marmaritza.

STRONG earthquakes continue in Peru. There was one in Arequipa on April 11. The movements were from east to west, and the duration forty to fifty seconds. It is worthy of notice that on the same April 11 two slight shocks of earthquake were felt at Rangoon, in Burmah, the direction being from north to south. On the night of the 16th another earthquake was felt.

EARTHQUAKE shocks were felt on May 21st in the vicinity of Rochester and Buffalo, in the state of New York; at Augusta, in Georgia; and at Quebec, Ottawa, and other points in Canada.

THE remote island called Sunday Island, in the Pacific, has been subjected to a terrible volcanic eruption so that the inhabitants have been removed to Norfolk Island, to join the descendants of the *Bounty* Mutineers.

ON February 22 several shocks of earthquake were felt at Puno, in Peru, and on March 4 a slight earthquake of thirty seconds at Arequipa after several rainy days.

ON February 7 two distinct shocks of earthquake were felt in the department of Minititlan, in Mexico, followed by a wave rising one foot.

AMONG the late remarkable disturbances in the Pacific basin are to be numbered those affecting the waters of the ocean around the guano Islands of Guanape on the Peruvian coast, which took place on the 5th of February. During the whole of that day the sea was much agitated, though nothing particular was noted in the tides. On the morning of the 6th there was something strange about the currents, with a westerly wind freshening with dangerous force. The winds and currents ruling along the Peruvian coast are from the S.E., but on the 6th this was not so, for they veered round and came from the W. at six miles an hour (? currents). Then it was noticed that as the day grew on the currents seemed to flow in from all directions, forming numerous whirlpools, while alarm for the shipping was caused by the increasing strength of the west wind. The nights of the 6th and 7th were consequently times of alarm to the masters of the guano ships, which were dashed against each other. The phenomena had a great resemblance to those at Arica and the Chincha Islands on the 15th of August, 1868. On the 9th of February the appearances were calmer, and the wind veered round to S.E.

AT Pichicani, in Peru, an extraordinary meteor appeared on February 12. It was of a red colour, balloon-shaped, with the end or neck pointed to the earth, and exploded as it reached the surface, leaving a dark cloud on the plain, injuring the roofs of several huts, and knocking down a fence of about 500 yards belonging to a farm. Among the fragments of this meteorite were found dead fish of several species, supposed to have been lifted out of the river. Similar phenomena had been observed near Huacochullo and Atucachi.

INDIAN papers report that the tea prospects in Darjeeling this year are so favourable that up to the present time (May) the crop has been from twice to three times what it was at that date last season.

THE report of the Curator of the Natal Botanic Garden for 1870 states that there had been shipped to various public and private gardens 5 Ward's cases, 22 boxes, and 11 parcels, and that there had been received 13 Ward's cases, 9 boxes, and 22 parcels.

IT is reported from Chile that the Planchon Pass across the

Andes, the main line from Chile to Buenos Ayres, has been disturbed for about three miles by the eruption of hillocks.

AN Australian paper states that a live frog had been brought to the office that had been found three or four days before incased in the solid rock, in the drive of the Sultan mine, Barry's Reef, at a depth of 400ft. below the surface. The little animal looked bright-eyed and very lively, and was apparently none the worse for its long term of solitary imprisonment.

## SCIENCE IN AMERICA *

THE forthcoming number of the *American Journal of Science* will contain an extremely interesting announcement in regard to American palæontology, namely, the discovery by Prof. Marsh in the Cretaceous beds of the Rocky Mountain region, of a huge pterodactyl, or flying lizard. This form has long been known as characteristic of the deposits of Europe, and has always attracted much attention from its combination of the characters of the bird and reptile; but until this announcement by Prof. Marsh the family was not supposed to be represented in the New World. The addition therefore of the pterodactyl, to the list of American genera, shows a marked increase in palæontological affluence, and gives additional point to the statements made some time ago, that America, instead of being greatly inferior to the Old World in the variety of its vertebrate fossil remains, now bids fair to greatly exceed it in this respect. The name assigned to this new species is "Pterodactylus Oweni" (in honour of Prof. Richard Owen of London), and it is believed to have had an expanse between the tips of the wings of at least twenty feet.—We regret to learn that during the recent revolution on the Isthmus of Tehuantepec a large number of valuable collections in natural history, made for the Smithsonian Institution by its correspondent in that region, Prof. Sumichrast, were entirely destroyed in the course of the conflicts of the opposing parties.—The annual report of the Smithsonian Institution for 1869 has, after an unusual delay, just made its appearance from the public printing-office, and contains the customary variety of interesting matter, which has made this report so much sought after by persons of scientific tastes in the United States. Preceded by the secretary's usual report of the operations of the Institution for the year, it contains in an appendix numerous articles, partly original, and partly translations from such foreign journals as are not readily accessible to the American student. Among these may be mentioned biographies of Thomas Young, Augustus Bravais, Von Martius, and Marianni; an important original paper by Dr. Sterry Hunt on the chemistry of the earth; and one by Marey on the phenomena of flight in the animal kingdom; an extended paper by General Simpson, upon the march of Coronado in search of the seven cities of Cibola; one by Sir John Lubbock, on the social and religious condition of the lower races of man, &c. The report is in no way inferior in interest to its predecessors. —Salt Lake City has lately been the scene of considerable activity, in connection with the arrival there of several government exploring parties, for the purpose of fitting out for their summer's campaign. Among these may be mentioned Mr. Clarence King, who continues his geological and topographical exploration of the fortieth parallel eastward through Colorado; Major Powell, who renews his examination of the canons of Green River and the Colorado, and who is detained at Salt Lake City in consequence of the late melting of the mountain snows, the low stage of water preventing him from passing through the canons; and a portion of Prof. Hayden's party is also at the same place collecting animals and supplies for a visit to the Yellow Stone region.—By advices from South America we learn that on the 25th of April last Chili was visited by two of the severest earthquakes that have been experienced in the country since 1851. The first shock in Valparaiso was not preceded by any warning sound, and its suddenness and intensity created considerable alarm, the streets of the city being filled in a short time by people who rushed out from their dwellings in a state of indescribable confusion.—Many of our readers are familiar with the names of Mr. Thomas Say, of Philadelphia, and Mr. C. A. Leseur, as having been among the most prominent of our naturalists during the early part of the present century, and as having added many new species to the lists. The labours of Mr. Say were directed largely toward the invertebrata, embracing more particularly the insects, shells, and crustaceans. Many of his explorations were in the vicinity of Beesley's Point, New Jersey, where species were obtained by him that have ever sinc remained almost unknown to science. Several examinations have been more recently made on the New Jersey coast, for the express purpose of recovering these forms; and one of the most successful was prosecuted last spring, under the direction of Prof. Verrill, of Yale College, who, with several companions, spent a week at Somers Point and Beesley's Point. The results of their labours were much greater than they had anticipated, as they not only obtained a large proportion of all the missing forms, but secured quite a number of new species, and detected the occurrence, for the first time, of others previously known as belonging much farther south, among them two echinoderms, of which Cape Hatteras was the limit previously ascertained. Their "catch" for the week summed up about 175 species of marine animals—about 25 of fishes, 50 of crustaceans, 25 of worms, 50 of mollusks, and 15 of radiates and sponges.

* Communicated by the Scientific Editor of *Harper's Weekly*.

## MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY

(*Continued from page* 152)

GERMANY, or rather Central Europe from the Rhine to the Carpathians and from the Baltic to the Alps, is, as to the greater part of it, a continuation of that generally uniform but gradually changing biological region which covers the Russian empire. It is not yet affected by those peculiar western races which either stop short of the Rhine and Rhone or only here and there cross these rivers with a few stragglers; the mountains, however, on its southern border show a biological type different from either of those which limit the Russian portion, indicating in many respects, as I observed in 1869, a closer connection with the Scandinavian and high northern than with the Pyrenean to the west or the Caucasian to the east. The verifying and following up these indications gives a special interest to the study of German races, their variations and affinities. In so far as formal specific distinctions are concerned, all plants and animals, with the exception of a few of those whose minute size enables them long to escape observation, may now be considered as well known in Germany as in France and England; and in Germany especially the investigation of anatomical and physiological characters has of late years contributed much to a more correct appreciation of those distinctions and of the natural relations of organic races. But much remains still for the systematic biologist, and especially the zoologist, to accomplish. Among the very numerous floras of the country, both general and local, there are several which have been worked out with due reference to the vegetation of the immediately surrounding regions, but corresponding complete faunas do not appear to exist. A few in some branches have been commenced; but in these, as in the numerous papers on more or less extended local zoology, as far as I can perceive, animals, and especially insects, seem to be considered only in respect of the forms they assume within the region treated of, frequently with a very close critical study of variations or races of the lowest grades, but neglecting all comparison with the forms a species may assume or be represented by in adjoining or distant countries.

Germany holds a first rank amongst civilised nations in respect of her biological works in most departments; they probably exceed in bulk those of any other country. Her publishi.g scientific academies and other associations, her zoological museums and gardens, her botanical herbaria and university gardens, her zoologists and botanists of world-wide reputation, are far too numerous to be here particularised. She excels all other nations in the patient and persevering elaboration of minute details, although she must yield to the French in respect of clearness and conciseness of methodical exposition. Her speculative tendencies are well known, and the great impulse given to them since the spread of "Darwinismus" appears to have thrown systematic biology still further into the background; the sad events of the last twelvemonth have also temporarily suspended or greatly interfered with the peaceful course of science. Thus the zoological works contained in the lists I have received are almost all dated in 1868 or 1869, and have been already analysed in the reports of Wiegmann's "Archiv" and in the 5th and 6th volumes of the "Zoological Record," and the principal ones relating to exotic zoology will have to be referred to further on. In Systematic Botany also but little of importance has been published within the last ten years beyond the great "Flora Brasiliensis," which, since the death of Dr. v. Martius, has been actively proceeded with under the direction of Dr. Eichler, and to which I shall recur under the head of South America. Rohrbach has published a carefully worked-out conspectus of the difficult genus *Silene*, and, in the "Linnæa," a synopsis of Lychnideæ; and Böckeler, also in the "Linnæa," is describing the Cyperaceæ of the herbarium of Berlin, a work very unsatisfactory, considering the detail in which it is carried out, as it takes no notice whatever of the numerous published species not there represented, nor of any stations or other information relating to those described other than that what are supplied by that herbarium. It is not a monograph, but a collection of detached materials for a monograph.

Switzerland comprises the loftiest and most extensive mountain-range of which the biology has been well investigated—the Alps, which have lent their name to characterise the vegetation and other physical features of mountains generally, when attaining or approaching to the limits of eternal snows. The relations of this Alpine vegetation, both in its general character due to climatological and other physical causes, and in its geographical connection with other floras, has been frequently the subject of valuable essays, several of which I have mentioned on former occasions; and it is most desirable that the results obtained should be verified by or contrasted with those which might be derived from zoological data, and more particularly by the observation of insects and terrestrial mollusca. As a first step it is necessary that the plants and animals of the country should be accurately defined and classed in harmony with those of adjoining regions. This has been done for plants. The Swiss Flora has been well worked up both by German and by French botanists; it is included in Koch's Synopsis and some other German Floras. De Candolle and other writers on the French Flora had to introduce a large portion of the Swiss vegetation, and the compilers of the rather numerous Swiss Floras and handbooks* have generally followed either the one or the other, so that there remains but little difficulty in the identification of Swiss botanical races; but here, as elsewhere, methodical faunas of the country are much in arrear. I have the following notes from M. Humbert of what has been published in this respect during the last three years.

V. Fatio, "Faune des Vertébrés de la Suisse," 8vo, vol. i. Mammifères, 1869 (reported on in "Zoological Record," vi. p 4): the second volume, Reptiles, Batrachia, and Fishes, to appear in the course of the present year, the 3rd and 4th vols. (Birds) to follow. This fauna is the first which has been published on the Vertebrata of Switzerland. Hitherto there have only been partial and incomplete catalogues. The species are carefully described, and there are numerous notes on their distribution and habits, from the author's observations made in all the Swiss collections and in the field. There are also interesting historical details upon certain animals which have more or less completely disappeared from Swiss territory, such as the stag, the roebuck, and the wild boar, as also on the mammifers, whose remains have been found in recent deposits. G. Stierlin and V. de Gautard, "Fauna Coleopterorum Helvetica," in the Nouveaux Mémoires of the Helvetic Society, xxiii. and xxiv., a catalogue with stations and often limits in altitude, supplementing Heer's "Fauna Coleopterorum Helvetica." H. Frey's catalogues of and notes on Swiss Microlepidoptera, in the "Mittheilungen" of the Swiss Entomological Society. P. E. Müller, Note on the Cladocera of the great lakes of Switzerland, from the "Archives" of the Bibliothèque Universelle, xxxvii., April, 1870. In his excellent memoir on the Monoclea of the neighbourhood of Geneva, Jurine had only described the small crustacea of ponds and swamps. He had not investigated the species which inhabit the Lake of Geneva, and he had also neglected some very interesting forms which are only to be met with in large expanses of water, such as *Bythotrephes longimanus* and *Leptodora hyalina*. M. Müller points out the differences there are between the Cladocera of the centre of the lakes and those of the margins. The former, which float freely over the lake, have a peculiar stamp, marking also the marine crustacea of open sea; their bodies have an extreme transparency, and they show a great tendency to the development of long and rigid balancing organs. The latter, on the contrary, are little transparent, have stunted forms, and are without balancing or other elongations which might interfere with their movements amidst solid objects, such as stones and aquatic plants near the shores; most of these littoral species show, moreover, a development of some organ that assists them in moving upon solid bodies. M. Müller finds also a very great connection between the Cladoceral faunas of Switzerland and Scandinavia.

The Association zoologique du Léman, founded upon the model of the Ray Society, has for its object the publication of monographs relating to the basin of the Léman or Lake of Geneva, that is, the region comprised between Martigny and the Perte du Rhone, with the valleys of the affluents received by the Rhone in this portion of its course. It has been carried on as successfully as could have been expected from a scientific undertaking of this nature, reckoning at the present moment nearly 200 members. It has already published papers by A. Brot on the shells of the family of Naiadæ, with nine plates; by F. Chevrier on the Nyssæ (Hymenoptera); by V. Fatio on the Arvicolæ, with six plates; by H. Fourniet on the Dascillidæ (Coleoptera), with four plates; and is now issuing a more important work, the result of long and patient investigation, G. Lunel's "Histoire Naturelle des Poissons du Bassin du Léman," in folio, with twenty plates beautifully executed in chromolithography. Two parts, with eight plates, have already appeared, and the work is in rapid progress. A specimen of the plates, received from M. Humbert, lies on the tables of our library. I have also a rather long list of papers on the zoology of the same district or of the Canton de Vaud, inserted in the Bulletin of the Société Vaudoise of Natural History, and of others on the zoology of other districts, from various other Swiss Transactions, all of which are noticed in our "Zoological Record," vols. v. and vi. To these must be added J. Saratz's "Birds of the Upper Engadin," from the 2nd volume of the Bulletin of the Swiss Ornithological Society, 1870. The valley of the Upper Engadin commences at 1,860 metres above the level of the sea, and ends at 1,650 metres, where commences the Lower Engadin. The list therefore given by M. Saratz includes no point situate below that elevation. He classes the birds of this valley and of the mountains which enclose it into—1, sedentary birds; 2, birds which breed in the Upper Engadin, but do not spend the winter there; and 3, birds purely of passage. He enumerates 144 species, and gives upon every one notes of its station, times of passage, abundance or rarity, &c.

Meyer-Dür has a short note in the "Mittheilungen" of the Swiss Entomological Society (iii. 1870) on certain relations observed between the insect faunas of Central Europe and Buenos Ayres—a question worthy perhaps of some consideration in connection with the above-mentioned coincidence of a Chilian and East-Mediterranean *Geum* and a very few other curious instances of identical or closely representative species of plants in the hot dry districts of the East Mediterranean, the central Australian, and the extratropical South American regions.

Swiss naturalists continue their activity in various branches of biology. E. Claparède's very valuable memoirs on Annelida Chætopoda and on Acarina have been fully reported on in the "Zoological Record," as well as Henri de Saussure's entomological papers, which have been continued in the more recently published volumes of the Memoirs of the Société de Physique of Geneva and of the Swiss Entomological Society. In Botany, since I last noticed De Candolle's "Prodromus," the sixteenth volume has been completed by the appearance of the first part, containing two important monographs—that of Urticaceæ, by Weddell, and of Piperaceæ, by Casimir De Candolle, together with some small families by A. De Candolle and J. Müller. The social disturbances of the last twelvemonth have much delayed the preparation of the seventeenth volume, which is to close this great work; but it is hoped that it will be now shortly proceeded with. Of Boissier's "Flora Orientalis," mentioned in my address of 1868, the second volume is now in the printer's hands. Dr. G. Bernouilli, who had resided some time in Central America, has published, in the

* In the list of publications of the last three years only, sent me by M. A. De Candolle, are the following new Swiss Botanical Handbooks:—J. C. Ducommun, "Taschenbuch für den Schweizerischen Botaniker," 1 vol. 8vo of 1024 pages, with some analytical woodcuts: few details on stations. R. T. Simler, "Botanischer Taschenbegleiter der Alpenclubisten," 1 vol. 12mo, 4 plates: alpine species only. Tissière (late Canon of St. Bernard, now deceased), "Guide du Botaniste au Grand St Bernard" 1 vol. 8vo: a catalogue with detailed localities. J. Rhiner, "Prodrom der Waldstätter Gefässpflanzen," 1 vol. 8vo: a catalogue with details as to localities. Morthier, "Flore analytique de la Suisse," 1 vol 12mo: imitated from an older German "Excursions Flora fur die Schweiz," by A Gremli. A new (3rd edition of L. Fischer's "Flora von Bern" and Fischer-Ooster's "Rubi Bernenses;" the latter work, together with some contributions to the Swiss Flora of A. Gremli, adding 98 pages to the volumes of Batological literature we already possess, without advancing a step either in giving us a clear notion of what is a species of Bramble, or in facilitating our naming those we meet with, unless in the precise localities indicated by the several authors.

Memoirs of the General Helvetic Society (vol. xxiv.) a review of the genus *Theobroma*, after having compared his specimens in the herbaria of Kew, Berlin, and Geneva.

The biological interest of the Mediterranean Region, which includes Southern Europe, the north coast of Africa, and those lands vaguely termed the Levant, is in many respects the opposite of that of the great Russian empire. Extending from the Straits of Gibraltar to the foot of the Caucasus and Lebanon, over 40 to 45 degrees of longitude, by 10 to 12 degrees of latitude, from the southern declivities of the Pyrenees, of the Alps, the Scardus, and the Balkan, to the African shores, it shows, indeed, a certain uniformity of vegetation through the whole of this length and breadth; but it has evidently been the scene of great and frequent successive geological convulsions and disturbances, which, whilst they have wholly or partially destroyed some of the races most numerous in individuals, have at the same time so broken up the surface of the earth as to afford great facilities for the preservation or isolation of others represented by a comparatively small number of individuals. The consequence is that there is probably no portion of the northern hemisphere in the Old World, of equal extent, where the species altogether, and especially the endemic ones, are more numerous, none, I believe, which contains so many *dissevered* species (those which occupy several limited areas far distant from each other), and certainly none where there are so many strictly local races, species or even genera, occupying in few or numerous individuals single stations limited sometimes to less than a mile. In all these respects the Mediterranean region far exceeds, absolutely as well as relatively, the great Russian region, which has three times its length and twice its breadth; it presents, also, perhaps almost as great a contrast to a more southern tract of uniform vegetation extending across the drier portion of Africa and Arabia as far as Scinde. This diversified endemic and local character exemplified in the plants of the Mediterranean region has, as far as I can learn, been observed also in insects.

Of the three great European peninsulas which form the principal portion of the region, the Italian is the narrowest and has the least of individual character in its biology, but it is the most central one, and, including its continental base with the declivity of the Alps, may be taken as a fair type of the region generally; it is also by far the best known. Italy was the first amongst European nations to acquire a name in the pursuit of natural science after emerging from the barbarism of the middle ages; and although she has since been more devoted to art, and has allowed several of the more northern states far to outstrip her in science, she has still, amidst all her vicissitudes, produced a fair share of eminent physiologists as well as systematic zoologists and botanists; and within the last few years the cultivation of biology appears to have received a fresh impulse. It is only to be hoped that it may not be seriously checked by local and political intrigues, which appear to have succeeded, in one instance at least, in conferring an important botanical post on the least competent of the several candidates. Amongst the various publishing academies and associations mentioned in my address of 1865, the Italian Society of Natural Sciences at Milan issues a considerable number of papers on Italian zoology; and a few others in zoology and palæontology are scattered over the publications of the Academies of Turin and Venice and of the Technical Institute of Palermo. From the lists I have received, there appear to have been recent catalogues of Sicilian and Modenese birds by Doderlein in the "Palermo Journal," of Italian Araneida and Modenese fishes by Canestrini in the "Milanese Transactions," and of Italian Diptera, commenced by Rondani in the Bulletin of the Italian Entomological Society. Malacology, so peculiarly important in the study of the physical history of the Mediterranean region, has produced numerous papers, chiefly in the Milanese Transactions, and in Gentiluomo's "Bulletino Malacologica," and "Biblioteca Malacologica" published at Pisa. I also learn that at the time of the decease of the late Prof. Paolo Savi, in the beginning of April, the manuscript of his "Ornitologia Italiana" was complete, and had just been placed in the printer's hands.

In Botany, Parlatore's elaborate "Flora Italiana" has continued to make slow progress. We have received up to the second part of the fourth volume, reaching as far upward as Euphorbiaceæ, having commenced with the lower orders. The old Journal of Botany ceased with the year 1847, as I presumed to have been the case when I mentioned it in 1865, and has since been replaced by a "Nuovo Giornale Botanico Italiano," which continues, with tolerable regularity, issuing four parts in the year, the last received being the second of the third volume. The most valuable of the systematic papers it contains are Beccari's descriptions of some of his Bornean collections. Delpino, well known for his interesting dichogamic observations, as well as for some rather imaginative speculations, has also contributed to systematic botany a monograph of Marcgraviaceæ, but, unfortunately, without sufficient command of materials for the compilation of a useful history of that small but difficult group, and with a useless imposition of new names to forms which he thinks may have been already published, but has not the means of verifying. De Notaris, under the auspices of the municipality of Genoa, has published a synopsis of Italian Bryology, forming a separate octavo volume of considerable bulk.

Of the other two great European peninsulas I have little to say, notwithstanding their great comparative biological importance. The Western or Iberian Peninsula is the main centre of that remarkable Western flora to which I specially alluded in 1869, and which, more perhaps than any other, requires comparison with entomological and other faunas. But Spain is sadly in arrear in her pursuit of science. With great promise in the latter half of the last century, and certainly the country of many eminent naturalists, especially botanists, she has now for so long been subject to chronic pronunciamentos that she leaves the natural riches of her soil to be investigated by foreigners. Willkomm and Lange's "Prodromus Floræ Hispanicæ," which, when I last mentioned it, was in danger of remaining a fragment, has since been continued, and, it is hoped, will shortly be completed by the publication of one more part. I have no notes on any recent zoological papers beyond Steindachner's Reports on his Ichthyological tour in Spain and Portugal, and the Catalogues of the Zoological Museum of Lisbon publishing by the Lisbon Academy of Sciences. The Eastern Peninsula, Turkey, and Greece, with the exception of some slight attempts at Athens, has no endemic biological literature, and, with its present very unsatisfactory social state, affords little attraction to foreign visitors. The Levant, in respect of Botany at least, has has been much more fully investigated; but there, as in Turkey, much yet remains to be done; and pending the issue of Boissier's second volume already mentioned, I know of nothing of any importance in the biology of the East Mediterranean region as having been worked out within the last two or three years. As a hiatus, however, and yet a link between the Indian and the European Floras and Faunas, it will amply repay the study to be bestowed upon it by future naturalists.

(*To be continued*)

## *ASTRONOMY*

### On the Great Sun-spot of June 1843 *

ONE of the largest and most remarkable spots ever seen on the sun's disc appeared in June 1843, and continued visible to the naked eye for seven or eight days. The diameter of this spot was, according to Schwabe, 74,000 miles; so that its area was many times greater than that of the earth's surface. Now, it has been observed during a number of sun-spot cycles that the larger spots are generally found at or near the epoch of the greatest numbers. The year 1843 was, however, a *minimum* epoch of the eleven-year cycle. It would seem, therefore, that the formation of this extraordinary spot was an anomaly, and that its origin ought not to be looked for in the *general cause* of the spots of Schwabe's cycle. As having a possible bearing on the question under consideration, let us refer to a phenomenon observed at the same moment, on the 1st September, 1859, by Mr. Carrington, at Redhill, and Mr. Hodgson, at Highgate. "Mr. Carrington had directed his telescope to the sun, and was engaged in observing his spots, when suddenly two intensely luminous bodies burst into view on its surface. They moved side by side through a space of about thirty-five thousand miles, first increasing in brightness, then fading away. In five minutes they had vanished. . . . It is a remarkable circumstance that the observations at Kew show that on the very day, and at the very hour and minute of this unexpected and curious phenomenon, a moderate but marked magnetic disturbance took place, and a storm, or great disturbance of the magnetic element, occurred four hours after midnight, extending to the southern hemisphere." The opinion has been expressed by more than one astronomer that this phenomenon was produced by the fall of meteoric matter upon the sun's surface. Now the fact may be worthy of

* From the "American Journal of Science and Arts," vol. i., April 1871.

note that the comet of 1843, which had the least perihelion distance of any on record, actually grazed the solar atmosphere about three months before the appearance of the great sun-spot of the same year. The comet's least distance from the sun was about 65,000 miles. Had it approached but little nearer, the resistance of the atmosphere would probably have brought its entire mass to the solar surface. Even at its actual distance it must have produced considerable atmospheric disturbance. But the recent discovery that a number of comets are associated with meteoric matter, travelling in nearly the same orbits, suggests the inquiry whether an enormous meteorite following in the comet's train and having a somewhat less perihelion distance, may not have been precipitated upon the sun, thus producing the great disturbance observed so shortly after the comet's perihelion passage.

DANIEL KIRKWOOD

Bloomington, Indiana

## SCIENTIFIC SERIALS

OF the *Sitzungsberichte der naturwissenschaftlichen Gesellschaft Isis in Dresden* we have received the concluding part of the volume for 1869, containing the proceedings of the Society for the months of October, November, and December. Its contents are as usual of the most varied character, and we shall therefore notice only a few of the more prominent papers. In the section for prehistoric archæology Dr. Mehwald gave an interesting notice of the researches made in Norway by a young student, M. Lerange, and further a general account of ancient mining and mining implements. Under the zoological section we find an abstract by Prof. Günther of the faunistic results of recent deep-sea dredgings, founded of course chiefly upon the reports of MM. Pourtales and Agassiz, and our countrymen Messrs. Thomson, Jeffreys, and Carpenter. Under the head of mathematics, physics, and chemistry, is a paper by M. F. Otto on the calamine deposits in Upper Silesia, which would have better taken its place as a geologico-mineralogical paper. An important botanical paper is the revision by Dr. L. Rabenhorst of the Cryptogamia collected in the East (especially in Persia) by Prof. Haussknecht, in which the author catalogues a considerable number of Fungi and Lichens, and describes several new species and a new genus of the former class. The new genus *Seirosporium* belongs to the Discomycetous family Phacidiacei, and the species *S. ocellatum*, which is figured, lives upon dry stems of *Astragalus deinacanthus* Boiss. The new species described belong to the genera *Synchytrium* (2), *Ustilago* (2), *Uromyces* (1), *Puccinia* (2), *Cyathus* (1), *Montaguea* (1, figured), *Coprinus* (1), *Dothidea* (1), *Melogramma* (1), *and Rhytisma* (1).

THE fourth part of vol. xxii. of the *Zeitschrift der deutschen geologischen Gesellschaft* (1870) contains several very important memoirs. The first of these is upon new and little known Crustacea from Solenhofen by Prof. Kunth, illustrated with two plates, and includes detailed descriptions of the Stomatopod *Sculda pennata* (Münst), and of two new species of the same genus; and among the Isopods of *Urda rostrata* (Münst) forming the type of a new family *Urdaidæ*, *Reckur punctatus* (Münst), also referred to the genus *Urda*, *Naranda anomala* (Münst), and a species of *Æga*.—From M. Lemberg we find a detailed and valuable chemico-geological investigation of some calcareous deposits of the Finnish Island of Kimito, in which the author not only describes the chemical composition and mechanical condition of the rocks under consideration, but discusses at considerable length some interesting points connected with the general theories of rock-formation.—M. E. Kayser commences a series of studies of the Devonian of the district of the Rhine with a disquisition on the deposits of that age in the neighbourhood of Aix la Chapelle.—M. C. Weiss publishes an investigation of the Odontotopterides, in which he discusses the forms to be referred to that group, and comes to the conclusion that the whole may be placed under the genus *Odontopteris*, which he divides into two sections, *Xenopterides* and *Callipterides*, the former including as sub-genera, *Mixoneura*, *Xenopteris*, and *Lescuropteris*; and the latter *Callipteris*, *Anotopteris*, *Callipteridium*. He gives a list of the species referable to each of these sub-genera, with remarks upon their characters and distribution; several of them are described as new and figured, with others, in the three plates with which the memoir is illustrated.—These papers are followed by some mineralogical notices by Prof. Rammelsberg treating of the meteoric stone of Chantonnay, of the sulphide of iron of meteoric irons, the composition of Lievrite, and the Anorthite rock of the Basto.—In the concluding paper of this number M. G. Berendt notices the occurrence of Cretaceous and Tertiary deposits near Grodno on the Niemen.

## SOCIETIES AND ACADEMIES

### LONDON

Royal Society, June 15.—"On the Fossil Mammals of Australia. Part V. Genus *Nototherium* Ow." By Prof. R. Owen, F.R.S. The genus of large extinct Marsupial herbivores which forms the subject of the present paper, was founded on specimens transmitted (in 1842) to the author by the Surveyor-General of Australia, Sir Thomas Mitchell, C.B. They consisted of mutilated fossil mandibles and teeth. Subsequent specimens confirmed the distinction of *Nototherium* from *Diprotodon*, and more especially exemplified a singular and extreme modification of the cranium of the former genus. A detailed description is given of this part from specimens of portions of the skull in the British Museum, and from a cast and photographs of the entire cranium in the Australian Museum, Sydney, New South Wales. The descriptions of the mandible, and of the dentition in both upper and lower jaws, are taken from actual specimens in the British Museum, in the Museum of the Natural History at Worcester, and in the Museum at Adelaide, S. Australia, all of which have been confided to the author for this purpose. The results of comparisons of these fossils of *Nototherium* with the answerable parts in *Diprotodon*, *Macropus*, *Phascolarctos*, and *Phascolomys*, are detailed.

Characters of three species, *Nototherium Mitchelli*, *N. inerme*, and *N. Victoriæ*, are defined chiefly from modifications of the mandible and mandibular molars. A table of the localities where fossil *Nototherium* has been found, with the dates of discovery, and the names of the finders or donors is appended. The paper is illustrated by subjects for nine quarto Plates.

"On the Organisation of the Fossil Plants of the Coal-measures. Part II. *Lepidodendra* and *Sigillariæ*." By Prof. W. C. Williamson, F.R.S. The *Lepidodendron selaginoides* described by Mr. Binney, and still more recently by Mr. Carruthers, is taken as the standard of comparison for numerous other forms. It consists of a central medullary axis composed of a combination of transversely barred vessels with similarly barred cells; the vessels are arranged without any special linear order. This tissue is closely surrounded by a second and narrower ring, also of barred vessels, but of smaller size, and arranged in vertical laminæ which radiate from within outwards. These laminæ are separated by short vertical piles of cells, believed to be medullary rays. In the transverse section the intersected mouths of the vessels form radiating lines, and the whole structure is regarded as an early type of an exogenous cylinder; it is from this cylinder alone that the vascular bundles going to the leaves are given off. This woody zone is surrounded by a very thick cortical layer, which is parenchymatous at its inner part, the cells being without definite order, but externally they become prosenchymatous, and are arranged in radiating lines, which latter tendency is observed to manifest itself whenever the bark cells assume the prosenchymatous type. Outside the bark is an epidermal layer, separated from the rest of the bark by a thin bast-layer of prosenchymna, the cells of which are developed into a tubular and almost vascular form; but the vessels are never barred, being essentially of the fibrous type. Externally to this bast-layer is a more superficial epiderm of parenchyma, supporting the bases of leaves, which consist of similar parenchymatous tissue. Tangential sections of these outer cortical tissues show that the so-called "decorticated" specimens of *Lepidodendra* and of other allied plants are merely examples that have lost their epidermal layer, or had it converted into coal; this layer, strengthened by the bast-tissue of its inner surface, having remained as a hollow cylinder, when all the more internal structures had been destroyed or removed.

From this type the author proceeds upwards through a series of examples in which the *vessels* of the medullar become separated from its central *cellular* portions and retreat towards its periphery, forming an outer cylinder of medullary vessels, which are arranged without order, and enclose a defined cellular axis. At the same time the encircling ligneous zone of radiating vessels becomes yet more developed, both in the number of its vessels and in the diameter of the cylinder relatively to that of the entire stem. As these changes are produced, the medullary rays separating the laminæ of the woody wedges become more definite, some of them assuming a more composite structure, and the entire organisation gradually assuming a more exogenous type. At the same time the cortical portions retain all the essential features of the Lepidodendroid plants. We are thus brought, by the evidence of internal organisation, to the conclusion that the plants which Brongniart has divided into two distinct groups, one of which he has placed amongst the vascular Cryptogams, and the other amongst the Gymnospermous Exogens, constitute one great natural family.

*Stigmaria* is shown to have been much misunderstood, so far as the details of its structure are concerned, especially of late years. In his memoir of *Sigillaria elegans*, published in 1839, M. Brongniart gave a description of it, which, though limited to a small portion of its structure, was, as far as it went, a remarkably correct one. The plant, now well known to be a root of *Sigillaria*, possessed a cellular pith without any trace of a distinct outer zone of medullary vessels, such as is universal amongst the *Lepidodendra*. The pith is immediately surrounded by a thick and well-developed ligneous cylinder, which contains two distinct sets of primary and secondary medullary rays. The primary ones are of large size, and are arranged in regular quincuncial order. They are composed of thick masses of mural cellular tissue. A tangential section of each ray exhibits a lenticular outline, the long axis of which corresponds with that of the stem. These rays pass directly outwards from pith to bark, and separate the larger woody wedges which constitute so distinct a feature in all transverse sections of this zone, and each of which consists of aggregated laminæ of barred vessels, disposed in very regular radiating series. The smaller rays consist of vertical piles of cells, arranged in single rows, and often consisting of but one, two, or three cells in each vertical series. These latter are very numerous and intervening between all the numerous radiating laminæ of vessels that constitute the larger wedges of woody tissue. The vessels going to the rootlets are not given off from the pith, as Gœppert supposed, but from the sides of the woody wedges bounding the *upper* part of the several large lenticular medullary rays; those of the *lower* portion of the ray taking no part in the constitution of the vascular bundles. The vessels of the region in question descend vertically and parallel to each other until they come in contact with the medullary ray, when they are suddenly deflected, in large numbers, in an outward direction, and nearly at right angles to their previous course, to reach the rootlets. But only a small number reach their destination, the great majority of the deflected vessels terminating in the woody zone. A very thick bark surrounds the woody zone. Immediately in contact with the latter it consists of a thin layer of delicate vertically elongated cellular tissue, in which the mural tissues of the outer extremities of the medullary rays become merged. Externally to this structure is a thick parenchyma, which quickly assumes a more or less prosenchymatous form, and becomes arranged in thin radiating laminæ, as it extends outwards. The epidermal layer consists of cellular parenchyma with vertically elongated cells at its inner surface, which feebly represents the bast-layer of the other forms of Lepidodendroid plants. The rootlets consist of an outer layer of parenchyma, derived from the epidermal parenchyma. Within this is a cylindrical space, the tissue of which has always disappeared. In the centre is a bundle of vessels surrounded by a cylinder of very delicate cellular tissue, prolonged either from one of the medullary rays, or from the delicate innermost layer of the bark, because it always accompanies the vessels in their progress outwards through the middle and outer barks.

The facts of which the preceding is a summary lead to the conclusion that all the forms of plants described are but modifications of the Lepidodendroid type. The leaf-scars of the specimens so common in the coal-shales, represent tangential sections of the petioles of leaves when such sections are made close to the epidermal layer. The thin film of coal of which these leaf-scars consist, in specimens found both in sandstone and in shale, does not represent the entire bark as generally thought, and as is implied in the term "decorticated," usually applied to them, but is derived from the epidermal layer. In such specimens, all the more central axial structures, viz., the medulla, the wood, and the thick layer of true bark, have disappeared through decay, having been either destroyed, or in some instances detached and floated out; the bast-layer of the epiderm has arrested the destruction of the entire cylinder, and formed the mould into which inorganic materials have been introduced. On the other hand, the woody cylinder is the part most frequently preserved in *Stigmaria*; doubtless because, being subterranean, it was protected against the atmospheric action which destroyed so much of the stem.

It is evident that all these Lepidodendroid and Sigillarian plants must be included in one common family, and that the separation of the latter from the former as a group of Gymnosperms, and as suggested by M. Brongniart, must be abandoned. The remarkable development of exogenous woody structures in most members of the entire family indicates the necessity of ceasing to apply either to them, or to their living representatives, the term Acrogenous. Hence the author proposes a division of the vascular Cryptogams into an Exogenous group, containing *Lycopodiaceæ*, *Equisetaceæ*, and the fossil *Calamitaceæ*, and an Endogenous group containing the Ferns; the former uniting the Cryptogams with the Exogens through the *Cycadeæ* and other Gymnosperms, and the latter linking them with the Endogens through the *Palmaceæ*.

"Contributions to the History of the Opium Alkaloids. Part II. On the Action of Hydrobromic Acid on Codeia and its derivatives." By C. R. A. Wright, D.Sc. It has been shown in Part I. of this research * that the action of hydrobromic acid on codeia gives rise, without evolution of methyl bromide, first to bromocodide, and secondly to two other new bases termed respectively deoxycodeia and bromotetracodeia, the latter of which, under the influence of hydrochloric acid, exchanges bromine for chlorine, yielding a corresponding chlorinated base, chlorotetracodeia; when, however, the action of hydrobromic acid is prolonged, methyl bromide is evolved in some little quantity. By digesting codeia with three or four times its weight of 48 per cent acid for five or six hours in the water-bath, vapours were evolved which, condensed by the application of a freezing-mixture to a colourless mobile liquid, the boiling-point of which was found to be 10°·5 to 11°·5, and the vapour of which burnt with a yellow-edged flame, exploded violently with oxygen, forming carbonic and hydrobromic acids. It becomes, therefore, of interest to examine in detail the action of hydrobromic acid on each of the three bodies produced from codeia under its influence.

"On the Physiological Action of the foregoing Codeia derivatives." By Michael Foster, M.D. The hydrochlorate of chlorotetracodeia and the hydrobromate of bromotetramorphia, in doses of a decigramme by subcutaneous injection or by the mouth, produced in adult cats in a very few minutes a condition of great excitement, almost amounting to delirium, accompanied by a copious flow of saliva and great dilatation of the pupils. Nicturation and defæcation occurred in some instances, and vomiting was observed on two occasions with the morphia-salt, but was very slight. The excitement was very peculiar, being apparently due partly to increased sensitiveness to noises, and partly to an impulse to rush about.

The same doses of the morphia-salt given to a young kitten produced the same flow of saliva, dilatation of pupils, and excitement (without vomiting); but the stage of excitement, which in adult cats passed gradually off in a few hours, was followed by a condition marked by a want of co-ordination of muscular movements, and presenting the most grotesque resemblance to certain stages of alcoholic intoxication. This stage was followed in turn by sleepiness and stupor, in which the kitten was left at night; in the morning it was found dead.

Two observations have shown that these salts paralyse (in dogs and cats) the inhibitory fibres of the pneumogastric; they also seem to lower the internal tension, but want of material has prevented me from ascertaining how this is brought about.

On rabbits neither salt, even in doses of a decigramme, seems to have any effect, except perhaps a slight excitement. There is no dilatation of the pupils, no flow of saliva, and, if one observation can be trusted, no paralysis of the inhibitory fibres of the pneumogastric.

No marked difference was observable between the two salts, except that the morphia salts seemed rather more potent than the corresponding codeia bodies.

The salts of deoxycodeia and deoxymorphia given by mouth or by subcutaneous injection in doses of a decigramme, produced in adult cats, almost immediately after exhibition, a series of con-

* Proc. Roy. Soc. vol. xix. p. 371

vulsions much more epileptic in character than tetanic. In one case there was a distinct rotatory movement.

In a few minutes these convulsions passed away, leaving the animal exhausted and frightened. Then followed a stage of excitement with dilated pupils and flow of saliva, very similar to the effects of the tetracodeia and tetramorphia salts, but less marked.

Doses of half a decigramme given to adult cats produced the stage of excitement only, without the convulsions.

In no case, with any specimen of product, has vomiting been witnessed.

Trials with rabbits gave only negative results. Like the tetracodeia and tetramorphia products, the deoxycodeia and deoxymorphia salts appear to paralyse the inhibitory fibres of the pneumogastric.

No marked differences could be observed between the hydrochlorates and hydrobromates of deoxycodeia or deoxymorphia.

"On the Calculation of Euler's Constant." By J. W. L. Glaisher, F.R.A.S.

Zoological Society, June 20.—R. Hudson, F.R.S., vice-president, in the chair. The Secretary read a report on the additions made to the Society's Menagerie during the month of May, 1871. Amongst these particular attention was called to a Tamandua Ant-eater (*Tamandua tetradactyla*) from Santa Martha, obtained by purchase, May 29, being the first specimen of the singular Mammal ever exhibited alive in the Society's collection.—Prof. Macdonald, of the University of St. Andrew's, Scotland, exhibited and made remarks on a series of specimens illustrative of the cranial bones of Fishes.—An extract was read from a letter received from Mr. Walter J. Scott, giving notice of a living specimen of the Australian Cassowary which had been lately captured in Queensland by Mr. Haig, and which Mr. Haig was anxious to present to the Society.—Prof. Newton exhibited and made remarks on some supposed eggs of the Sanderling (*Calidris arenaria*), procured by the North German Polar Expedition.—A communication was read from the Rev. O. P. Cambridge, containing notes on the Arachnida collected by Dr. Cuthbert Collingwood during his recent travels in the Chinese seas.—A communication was read from Dr. John Anderson, Curator of the Indian Museum, Calcutta, containing notes on some rare species of Rodents collected by Mr. Forsyth during his recent expedition to Yarkand.—Messrs. Sclater and Salvin read a revised List of the species of Laridæ which have been found to occur within the limits of the Neotropical region. These were stated to be 32 in number, whereof one belonged to the sub-family Rhynchopinæ, 14 to the Sterninæ, 16 to the Larinæ, and one to the Lestridinæ.—A communication was read from Dr. J. E. Gray, F.R.S., containing notes on the Bush-bucks (*Cephalophi*) contained in the collection of the British Museum, together with the descriptions of two new species of the genus from the Gaboon.—A second communication from Dr. J. E. Gray contained some notes on the skull of a roebuck in the British Museum, originally received from the Museum of the Zoological Society of London.—Mr. Sylvanus Hanley communicated the description of a new species of *Monocondylæa* from Sarawak, Borneo, which he proposed to call *M. Walpolei*.—Mr. D. G. Elliot read a review of the genus *Ptiloris*, Sw.—Mr. D. G. Elliot also read a description of a supposed new species of Guinea-fowl from Ugogo, Central Africa, founded on a drawing made by Colonel Grant during the expedition of Messrs. Speke and Grant, which he proposed to name *Numida Granti*.—Mr. R. B. Sharpe read a paper on the Birds of Cameroons, Western Africa, based upon collections recently formed by Mr. A. Crossley in that locality. The Avi-Fauna of the country was shown to be almost identical with that of Gaboon. A species of Thrush was believed to be new to science, and was proposed to be called *Turdus Crossleyi*.—Mr. John Brazier communicated some notes on the localities of *Dolium melanostoma*, *Conus rhododendron*, and other species of land-shells found in Australia and in the adjacent islands of the Australian seas.—Mr. W. Saville Kent read a paper on two new Sponges from North Australia, the principal peculiarity of which consisted of their being arranged round a central stem or axis. These he referred to a new genus proposed to be called *Caulispongia*.—Prof. Flower communicated a paper by Mr. J. B. Perrin on the myology of the limbs of the Kinkajou (*Cercoleptes caudivolvulus*), to which were added some remarks on the myology of the limbs of the *Paradoxurus typus* and *Felis caracal*, and more particularly with reference to the chief points of difference between these animals.

BRISTOL

Observing Astronomical Society.—Observations to May 31, 1871. *The Sun.*—Mr. T. W. Backhouse writes that on March 19 at 21h. 30m. a spot on the sun's S. hemisphere had an umbra 19,000 miles long, but its greatest width was but 3,500 miles. This spot passed the centre of the sun on the 21st. On the 22nd at 3h. there was a curious curve of numerous small spots starting from it. An extensive group which passed N. of the sun's centre on the 23rd contained on the 27th at 5h. the largest spot then on the sun. Its penumbra was 29,000 miles in diameter, and its umbra 14,000 miles long; yet if it existed at all on the 24th at 21h. it must have been quite small. A spot in the sun's S. hemisphere which passed the middle of the sun on April 11, and which was not large on the 6th, on the 7th at 21h. 35m. had a penumbra 63,000 miles long. On the 9th at 21h. 15m. it was about 41,000 miles long, and its chief umbra 13,000 miles in diameter, and mostly of a light shade. On April 20 at 21h. 45h. a spot also in the southern zone had an umbra 25,500 miles long; but its *f* part was very narrow, its *p* part was very irregular. Its *f* part became broader, and on the 24th at 20h. was separated from the *p* part. The umbra had previously shortened, being only about 21,000 miles long on the 23rd; at 21h. on the 23rd it passed the same centre. On the 28th at 3h. 20m. the penumbra was 38,000 miles long. At that time there was another large solar spot also in the S. zone, which had a penumbra 33,000 miles in diameter then; but on May 4 at 5h. 15m. it was 43,000 miles long and 35,000 miles wide, and it is now (May 8) larger still. Its umbra was roundish and much mottled, and on May 4 at 4h. was 17,000 miles long and 14,500 wide. On the 5th at 21h., however, there was a very slender bridge of light across it towards the southern part, and another farther north two-thirds across it. The latter still remains (May 8, 3h. 30m.), and nearly cuts the umbra in two; but the former has disappeared. Mr. Albert P. Holden, of London, reports as follows:—"April 10, 1871. A large spot, surrounded by an extensive penumbra, has recently appeared, which I observed at 2h. this day. The chief spot was rather long and narrow, except at one end, which was considerably wide, and the narrow portion was crossed by three complete (and one partial) bridges. The penumbra was unusually pale, and the umbra of a decided light-brown hue. In the upper part of the broad portion of the umbra was a large nucleus intensely black, and so large and dark as to be visible with a very low power. Almost joining the 'yawning gulf' of the nucleus was a light triangular patch, not quite so light as the penumbra. From the great ease with which the nuclei have been seen on this and other occasions it would seem as if they increased in visibility with the approach of the maxima of the sun-spot period. When they are visible, as on the present occasion, the windward penumbra of the spot in which they occur are always unusually light in colour."—Mr. William F. Denning, of Bristol, observed the sun with his $10\frac{1}{4}$in., and 4in. reflector on May 26, but with the exception of a large scattered group the spots were neither large nor interesting.

*Jupiter.*—Mr. Albert P. Holden says: "On February 20 at 7h. 30m. I observed this planet, and found the usual equatorial belts to present a most remarkable appearance. The whole equator was covered by what appeared to be great masses of clouds stretching across the planet in four parallel, but rather irregular, rows, each row containing about four or five distinct masses of cloud. As I was using a diagonal eyepiece I thought at first the mirror had become covered with moisture, but found the phenomenon to be really on the planet's surface. With a low power the whole equator had a mottled appearance, but higher powers brought out the masses of cloud very distinctly. The clouds coming over prevented my observing whether the rotation of the planet would change the scenery of the disc at all." Edmund Neison, of London, writes with regard to Jupiter: "The only result worth mentioning is the gradual deepening of the tinge of the equatorial belts and the increase in the general orange tinge of the whole disc. In fact, on May 15 it appeared to have changed to a distinct red. This is probably due merely to the low altitude of the planet, and its immersion in the orange mists of sub-sunset."

*Mars.*—Mr. Albert P. Holden, with his 3in. refractor, has obtained some very good views of this planet. He writes: "The Kaiser Sea and Dawes Ocean come out very distinctly. This planet seems to bear magnifying much more readily than other objects, eighty to the inch of aperture giving most excellent views."

PARIS

Academie des Sciences, June 19.—M. Claude Bernard in the chair. M. Claude Bernard read a letter from Mr. Alexander Herschel, noticing the death of his father on behalf of himself and of his eldest brother now in India. The lamented Sir John Herschel was the senior foreign associate member of the Institute. The foreign associate members are only five in number; it is considered the highest honour the Academy can offer to a foreigner. The President noticed also the death of the celebrated General Probert, who was an academician of long standing, and had devoted his whole life to the study of projectiles. His memoirs are numerous in the *Comptes Rendus*, but more numerous at the War Office. He was of opinion that the Prussian steel gun should be adopted by the French artillery, but his Imperial Majesty being a great artillerist, his opinion was totally disregarded. The vacancies to be filled amongst members and associates are now six. They have never been so numerous. There were twelve correspondents to elect before the investment of Paris took place. M. Dumas presented a memoir on the reciprocal action of magnetism and electricity circulating in a vacuum. The memoir was written by M. De La Rive, a foreign associate member of the Academy, and describes experiments tried with an apparatus analogous to the magnificent instruments exhibited by M. De La Rive at the "Champ de Mars" universal exhibition.—M. Elie de Beaumont, the other perpetual secretary, has directed public attention to the extraordinary cold experienced on the 18th May and 3rd June 1871, and asked for observations relating to it. Every information must be directed to him, and will be mentioned in the *Comptes Rendus*. Several other communications are duly acknowledged, and will be printed. Some of them relate to other severe depressions of temperature witnessed late in the season in former years; hoar frost was observed as late as in July 1802, which appears to have been one of the worst years ever known for low temperature in the summer.—M. Grèmand de Lany, the senior member of the Scientific Staff of the Parisian papers, has published an interesting book on the Academy of Sciences during the siege of Paris, giving a fair idea of the amount of work executed by members resident in Paris during that eventful period of its annals. The Academy has to appoint a committee for reporting upon the memoirs sent to compete for the great prize of mathematics proposed by the government. The subject proposed belongs to the theory of elliptical functions. No qualification of nationality is required. The names of the competitors are kept sealed and opened only if successful. MM. Bertrand, Hermite, Serret, Leonville, and Bonner were appointed.—A most interesting discussion took place on a paper relating to the treatment of typhus during the Mexican campaign, showing that typhus is unquestionably contagious, as well as many other diseases of the same kind. The cold and moisture is not so much to be feared as stagnant hospital air, and treatment under canvas even in cold weather is perhaps the best that can be imagined.—M. Campion, the first assistant to M. Payen, presented a memoir on the manner of blasting rocks with dynamite. That paper is a kind of *résumé* of M. Campion's experiments during the first investment of Paris. He was closely engaged in dangerous operations, practised for protecting the town. According to every probability, he will be appointed a member to fill the chair of his professor.—Five or six other papers were read, too long to report.

VIENNA

I. R. Geological Institution, May 2.—Dr. Gümbel, of Munich, gave an account of his investigations of the different forms of Dactylopora, found chiefly in the Triassic limestones of the Alps. Notwithstanding some differences in the structure, he recognised in them a strong resemblance to living and tertiary Dactylopora. Great and constant varieties in the forms led him to distinguish a large number of different species.—Mr. F. Pick, who had visited the Isle of Milo in the month of March, made a report of the numerous earthquakes which had been observed there since the beginning of the year. From the middle of January up to the month of March they continued incessantly, and during the time between the last days of February till the 3rd March more than twenty shocks were felt daily, not seldom two or three in one hour. The St. George volcano on Santorin was seen on March 20 in continuous, but feeble activity.—M. v. Lill discovered the rare Ullmannite (Nickel-Antimon-Pyrites) at a new locality in Carinthia, the Rinkenberg, near Bleiburg, where it is imbedded in slaty schists and crystalline dolomite.—Another mineralogical discovery of interest communicated by T. Niedzwiedski is the occurrence of Trinkerite at Gams, near Hieflau, in Styria. This fossil resin, which contains more than 4 per cent. of sulphur, was first described a few months ago by Dr. Tschermak, of Carpano, in Istria, where it was found in a coal of Eocene age. At Gams it is imbedded in a dark coloured rock, which belongs to the Gasau (Upper Cretaceous) formation.—Prof. E. Suess on the Tertiary land fauna of middle Italy. The study of the rich collections of fossil mammalia in the museums in Pisa and Florence enabled the author to parallelise the different faunæ of the Upper Tertiary beds of middle Italy, which had been distinguished quite correctly by Falconer, Lartet, &c., with those of Austria. The first mammalian fauna of the Vienna Basin, the fauna of Eibiswald, with Amphicyon intermediaries, Hyotherium Sömmeringi, Palæomerix, Crocodilus, Trionyx, &c., is represented in Italy by the fauna of the lignites of Monte Bamboli. The second fauna of the Vienna Basin, the fauna of Eppelsheim with *Mastodon longirostris*, *Hippotherium gracile*, &c., is not yet known in Italy. The fauna of the Arno Valley, on the contrary, which is represented in a marvellous richness in the museum of Florence, seems to be wanting in the Vienna Basin. This third fauna is characterised by *Elephas meridionalis*, *Machairodus*, *Bos etruscus*, *Hippopotamus major*, &c.; traces of it M. Suess thinks he has recognised in some fossils from the caverns of the Karst (Istria). The fourth fauna, with *Elephas primigenius* which is to be found everywhere in our loess, has been discovered also in some localities of Tuscany in the so-called Pauchina, a clay similar to the loess.—M. Schwackhöfer exhibited a series of rocks rich in phosphoric acid, which occur in the Silurian, as well as in the Cretaceous beds of Eastern Gallicia, the discovery of which he hopes will be of great use for agricultural purposes.

## BOOKS RECEIVED

ENGLISH.—The Homing or Carrier Pigeon: W. B. Tegetmeier (Routledge).

AMERICAN.—The Monthly Reports of the Department of Agriculture for 1868-69; The Annual Report of the Commission of Agriculture, 1868; The Annual Report of the U.S. Department of Agriculture, 1862: Government Printing Office, Washington.—The Elements of Physics: Prof. Hinrichs.

FOREIGN.—Die Pflanzenstoffe, &c.: Drs. A. and Th. Husemann (Schluss).—(Through Williams and Norgate)—Discussion der während der totale Sonnenfinsterniss am August 1868 angestellten Beobachtungen und der daraus folgenden Ergebnisse: Prof. E. Weiss.—Elektrodynamische Mass-bestimmungen: W. Weber.—Physische Zusammenkünfte der Planeten: C. von Littrow.

## DIARY

*MONDAY*, JULY 3.

ENTOMOLOGICAL SOCIETY, at 7.
ROYAL INSTITUTION, at 2.—General Monthly Meeting.

*FRIDAY*, JULY 7.

GEOLOGISTS' ASSOCIATION, at 8.—On the Upper Limits of the Devonian System: J. R. Pattison.

## CONTENTS

THURSDAY, JULY 6, 1871

## SENSATION AND SCIENCE

THE morbid craving for excitement, which is characteristic of mental indolence, as well as of effete civilisation, has led to the introduction of Sensation (as it is commonly called), not merely into our newspapers and novels, but even into our pulpits. It could not be expected that our popular scientific lectures would long escape the contamination. We have watched with regret its gradual introduction and development, and have often meditated an article on the subject. But now, when a splendid opportunity has come, we feel how unfit we are for the task. None but a Spurgeon can effectively criticise a Spurgeon; nóne but a *Saturday Reviewer* could be expected to tackle with delicacy and yet with vigour the gifted author of the "Girl of the Period." So we must content ourselves with the spectacle of the Rev. Prof. Haughton as criticised by himself. We have not been able to attend his recent lectures at the Royal Institution, but we have it on excellent authority that they were racy (*i.e.* sensational) in the extreme. Happily we find in the *British Medical Journal* what is described as an authorised version of them. A few extracts from this will enable us to dispense with a great deal of comment. We shall first take the Science, and then permit the Sensation to speak for itself.

Prof. Haughton's subject is *The Principle of Least Action in Nature;* and we are told that he believes he has succeeded in discovering in this the true principle on which the Science of Animal Mechanics must be founded, and has been enabled to sketch out the broad outlines of its foundation.

Maupertuis's Principle of Least Action is indeed "well known to mathematicians," but is by no means easy of explanation to the ordinary reader. We can, therefore, sympathise with the lecturer in his repeated failures to make it intelligible. But we cannot admit any justification of the constant use of the same words, sometimes in one sense, sometimes in a totally different one. To a mathematician (Prof. Haughton speaks as at once mathematician, anatomist, medical man, natural philosopher, "expert" at shot-drill, the crank, and the treadmill, clergyman, &c., &c., and even as potential farmer and landlord-shooter!) we should have thought that, when once $x$, $y$, $z$, or whatever else, is introduced, it has and continues to have a definite meaning, until *in a new problem* it comes to be applied to something possibly quite different. How then can we account for such sentences as the following?—

"The great problem—the problem of doing a given amount of work *with a minimum of effort.*"

"Nature aims at producing a given quantity of work *with the least quantity of material.*"

"I could show that these [tendons of the legs and arms of animals] are constructed *with a wonderful economy of force* of the same kind as that with which the bee constructs its cell"

"By what force, or by what intelligence, do the limbs of animals describe their proper path? Who places the socket of each joint in the exact position (which can be calculated with unerring certainty by mathematics) which enables the muscle to perform its allotted task *with the least amount of trouble to itself?*"

"The Principle of Least Action is that the arrangement and mutual position of all muscular fibres, bones, and joints must be such as to produce the required effect *with the minimum amount of muscular tissue.*"

"Before proceeding to apply *this principle of least action or least trouble* to nature," &c.

In all these extracts the italics are ours. If the reader but glance them over, he will not require to read the lectures to see what a very Proteus is this so-called principle. There is no knowing where to have it. It is a minimum, an economy, a least quantity, and what not; sometimes of effort, sometimes of material, then of trouble, and anon of muscular tissue, or of force of the same kind as that with which the bee constructs its cell! But the most curious feature about it is that in none of its metamorphoses does it in the slightest degree resemble the least action of Maupertuis, with which it would seem throughout to be held as identical.

Even in his remarks on this perfectly definite mathematical question, Prof. Haughton commits a grave error, for he says:—

"If I take the points A and B in the planet's path, S representing the sun, I only require to know those points A and B, and the sun S, to calculate for you, from the Principle of Least Action—which I can do to the millionth part of an inch at each point of this orbit—the path that the planet must describe, on the supposition that it is a lazy, intelligent animal, trying to swim round the sun in such a manner as to give the least trouble to itself."

It seems to us that all that the principle of least action can tell us, is that, supposing the sun's attraction to vary inversely as the square of the distance, the planet will describe *some conic section or other*, whose focus is S, and which passes through A and B. Which it will be of the innumerable conics satisfying these conditions, ellipse, parabola, or hyperbola (or possibly circle) there is nothing to indicate, within quadrillions of miles—yet we are told it can be done to the millionth of an inch!! As to what a "lazy, intelligent animal" (of course, not acted on by gravity) would do in "trying to swim [in what?] round the sun," we unfortunately possess no information. But this is merely another proof that we are dealing with Sensation where we looked for Science.

Here we have caught our instructor in a palpable and inexcusable blunder, and we could easily point out many others of a similar kind in his remarks on light, &c. It is not so easy to do so, or rather to make the general reader aware that we have done so, when he leaves strictly mathematical applications, and plunges headlong into a wild sea of speculation *without previous careful definition of his terms.* These terms are, in fact, as he employs them, so elastic, that it is only by contrasting (as we did above) portions of his lectures with other portions in which the same words acquire other and different meanings, or in which different words are employed for the same meaning, that we see how excessively loose and slipshod is the whole affair. Another little group of quotations will admirably illustrate this:—

"The law of *least action* is attended to in every department of nature *down to the most minute details.* . . . . *Not even one grain of material is ever used,* when less would suffice for the purpose."

This is, no doubt, admirable, and would suit the most frantic of the mischief-making teleologists. But, alas ! like the Editor of the *Little Pedlington Observer,* "What in one line we state we retract in another." For there follows—

"We can demonstrate by mathematics that in the use of every such muscle [triangular, &c.] *there is a necessary loss of force.* . . . I have always maintained that beauty of form . . . was one of the pre-existing conditions in the mind of the Contriver of the universe, as well as economy of force."

As intermediate to these two quotations, and in itself amusing from its *bonhommie* and condescension, we may take the following :—

"Nature, according to my principle, is entitled to employ these two forms of muscles whenever she pleases."

The reader may take our word that these are but single gems, selected from among many similar and often richer ones, mainly on the Principle of Least Trouble (in copying out for press).

As to really scientific matters, occasionally referred to in these lectures, we need merely mention that the author is ignorant of, or ignores, Dr. Pettigrew's extraordinary researches on wings and other adaptations for progression ; researches which ought to be thoroughly mastered by any one who attempts to write on the subject of animal mechanics ; and that, in his remarks on the strength of the uterine muscles, he seems to have entirely forgotten to notice how thoroughly least action theories (at least as applied by him) have been upset in a late number of the *Dublin Quarterly Journal of Medical Science.*

We promised Science first and Sensation afterwards. In attempting to collect the Science we have got hold of little but Sensation : so we need give only one extract more. Would it have been considered possible (till the 23rd of last May) that a Dublin professor, an M.D., a D.C.L., an F.R S., and a clergyman of the (till lately) Established Church, should, even in jest, speak as follows in the Royal Institution in London ?—

". . . . A brilliant idea came across my mind . . . . What in the world is to hinder me from taking a farm in Westmeath, deliberately and wilfully refusing to pay my rents, and in due time shooting my landlord, and, instead of using him as a New Zealand tenant would, dissecting him at my leisure ?"

We have only to add that the *British Medical Journal,* in publishing the above, conspicuously prints the remark :—

"In reproducing the *ipsissima verba* of the lecturer, and giving them a permanent place in scientific literature, an enduring service will be rendered to Science."

Which means, we hope, that all men, scientific or otherwise, will, once for all, take warning from this terrible example. If such be the result, Prof. Haughton will, indeed, not have lectured in vain. But if the *British Medical Journal* intends its remarks to signify approval, we can say of it and of Prof. Haughton, in the language of Cervantes—

No rebuznaron en valde
El uno y el otro Alcalde.

## BASTIAN ON THE ORIGIN OF LIFE

*The Modes of Origin of Lowest Organisms : including a Discussion of the Experiments of M. Pasteur, and a Reply to some Statements by Professors Huxley and Tyndall.* By H. Charlton Bastian, M.A., M.D., F.R.S., &c. (Macmillan and Co., 1871.)

IT may be as well to state at the outset that the present volume is not Dr. Bastian's long-promised work on "The Beginnings of Life ;" and it would have been better had some title been devised to prevent the confusion that will inevitably be caused by its appearance at this juncture. We have here, however, a condensed sketch of the whole controversy on Spontaneous Generation, and a statement of some very important researches conducted by the author since the discussion which followed Prof. Huxley's Presidential Address at Liverpool last September. It will be remembered that the objections to Dr. Bastian's experiments and to the results he deduced from them were twofold. It was said that we have no proof that these minute organisms (*Bacteria,* &c.), or their germs cannot resist the heat to which they were subjected. It was also said that no proof was given that the supposed organisms found by Dr. Bastian in these boiled and hermetically sealed liquids were alive. The motions exhibited might be "Brownian" motions, and the experimenter probably found nothing in his vessels but what he put into them. The answer to these objections is now given. The test of vitality is said to be, not movement, which is admitted to be uncertain, but the *power of reproduction.* It is found that if a portion of liquid containing *Bacteria* is divided into two parts, one of which is boiled, and a drop from each of these portions is mounted as a microscopic object, under a covering glass surrounded by quickly-drying cement, the unboiled specimen exhibits a marked increase from day to day in the quantity of imprisoned *Bacteria,* while the boiled specimen continues unchanged during the same time. Making use of this test of vitality, it was next ascertained what degree of heat was fatal to these low organisms. By using a lower and lower temperature, it was found that exposure to 140° F. for ten minutes destroyed *Bacteria,* while after exposure to 131° F. for the same time they rapidly multiplied. Somewhat higher organisms—*Vibrios, Amœbæ, Monads, Vorticellæ,* &c., were, however, killed by exposure to 131° F. for five minutes. It was subsequently ascertained that a four hours' exposure to a temperature of even 127° F. destroyed *Bacteria* and *Torulæ.* It is argued that, as in all these experiments the solutions used swarmed with *Bacteria,* &c., in various stages of increase, their hypothetical "germs" cannot be supposed to have been entirely absent ; and that we may therefore conclude that the "germ" has no greater power of resisting heat than the animal itself.

Dr. Bastian also criticises many of the experiments of Pasteur, and the arguments founded on them. He maintains that the corpuscles found by the latter to exist in the atmosphere, and which "resemble" spores of fungi, have never been proved to be such ; and even if they were so proved, it would not account for the constant occurrence of *Bacteria* and other low organisms, whose "germs" are quite unknown, and which there seems no reason to believe could retain their vitality in a dry state in the atmosphere. The fact that vessels with bent necks or with plugs of cotton-wool do not produce organisms, while other vessels not so protected produce them in abundance, is shown, by numerous experiments, not to be universal. The evidence now adduced is held to prove that a variety of conditions hitherto not attended to affect the result, such as temperature, the strength of the solution, and especially the presence of particles of organic matter, other than "germs," derived from the atmosphere. A summary is given of sixty-five comparative experiments, which are believed to show, among other things, that the non-production of *Bacteria*, &c., in infusions and other suitable liquids, is so common an occurrence that the negative experiments of Pasteur and others have no weight as compared with the positive results obtained by a considerable number of observers, to whom the author refers, as well as by himself.

Some of these comparative experiments are very suggestive. Hay infusion, for instance, exposed to air, produced abundance of *Bacteria* in forty-eight hours, and these had increased considerably in sixty-eight hours. A similar infusion, sealed up after the fluid had become cold, behaved in a similar manner. The same in a flask with neck two feet long and having eight acute flexures, remained unchanged for twelve days. A similar infusion, hermetically sealed during ebullition, on the other hand, showed turbidity in forty-eight hours, which subsequently increased, and *Bacteria*, *Vibriones*, *Leptothrix*, and *Torulæ* were found in abundance. Here, then, whatever inference may be drawn from the first three experiments is entirely negatived by the fourth. Other experiments show that ammonia-tartrate solution sealed *in vacuo* at a temperature of 90° F. produced in eighty-four hours abundance of *Bacteria;* while the same solution, if boiled at 212° F. and exposed to the air in flasks covered with paper caps, remained quite clear for nine days; yet as soon as it was inoculated with living *Bacteria*, they increased rapidly and produced turbidity. These, and a number of other equally suggestive experiments, indicate that the conditions favourable to the *origin* and to the *increase* of these low forms are not always identical. Both are very complex, and we cannot avoid the conclusion that the advocates of the universal germ theory have been somewhat hasty in founding their doctrine upon insufficient data, for the most part of a negative character.

We have here, undoubtedly, an important addition to the experimental evidence by which alone the question can be decided, and we are glad to observe the unprejudiced and philosophical spirit with which Dr. Bastian discusses this most interesting and important problem.

A. R. WALLACE

## THE WORKSHOP

*The Workshop.* Edited by Prof. W. Baumer, J. Schnorr, and others. (London: J. Hagger, 67, Paternoster Row.)

EVERY year of our national progress strengthens the national appreciation of the wisdom expressed in those words of the late Prince Consort, when he told the manufacturers of Birmingham that "the introduction of Science and Art as the *conscious* regulators of productive industry is destined to play a great and important part in the future development of this nation, and the world in general." I take the liberty of italicising the word "conscious," remembering well the emphasis with which it was spoken, and being strongly impressed with the vast importance of this qualification.

Science of some sort, and art of some sort, have always regulated the operations of productive industry. The club of the savage is not uncommonly carved with much art, and shaped and poised with sound practical knowledge of the whereabouts of the weapon at which will be concentrated the whole force of the blow when it swings through the curve which the stroke of the arm will give it. The savage artisan is, however, utterly unconscious of the dynamical principles upon which the centre of oscillation or percussion is determined, and upon which his own skill depends. He follows a blind instinct but one degree higher than that which impels the bee to construct its honey-comb upon sound statical principles. The more civilised workman who merely proceeds according to the "rule of thumb" and the traditions of his trade, is in a similar intellectual condition to that of the bee and the savage. In his daily occupation his specially human faculties are scarcely exercised. The constructive instinct which he possesses in common with the beaver or the wasp is sufficient to guide his muscles in doing such work in such a manner. To talk of the "dignity of labour" when labour is thus conducted is merely to indulge in senseless and vicious phrase-mongering.

The whole life and being of the artisan becomes changed immediately his daily work is *consciously* regulated by science and art. It then becomes an elevating instead of a brutalising occupation; the "dignity of labour" is removed from the sphere of platform verbiage to that of practical fact, and the workshop becomes a school of intellectual and moral culture.

We must always remember that the character of a man is formed by the daily, hourly, and continuous habits of his life, that no quantity or excellence of mere Sunday sermons, or occasional evening meetings, can overpower these. The philanthropist who would practically influencce the character of the workman must operate upon him in and through the workshop; and it appears to me that there are no conceivable means so effectual for this purpose as the converting his bread-winning work from a mere mechanical brutal drudgery into a moral and intellectual exercise. To understand thoroughly the scientific principles involved in all the operations of any common handicraft is to know a great deal more than our greatest philosophers are yet acquainted with, and therefore the field of the consciously scientific artisan is wide enough for the intellectual effort of a life time. If, in addition to the physical science of his trade, he is conscious of his own social relations and functions, if he knows the part which he is playing in the great machinery of society, the motive to his industry will not be that of a merely sordid grubbing for wages, but the sense of duty and the chivalry of reciprocal beneficence will be introduced, and will perpetually operate as necessary results of this scientific consciousness of his own social functions.

If soldiers and sailors can be taught to glorify their work, and rise to heroism in their efforts to do their duty and serve their country, why should not the spinner, the weaver, the tailor, the agriculturist, the miller, and the baker do the same? Surely it is as noble and as glorious and as serviceable to one's country, to be engaged in clothing the naked and feeding the hungry, as in shooting and drowning our fellow creatures?

I have referred above only to the artisan, but have chosen him and the workshop merely for the sake of typical illustration; the remarks apply equally to all who are engaged in useful industry, to the distributor as well as to the producer, to the capitalist and organiser of labour as well as to the labourer himself. The grocer, for example, who should understand and take intelligent interest in the natural history of the products that cross his counter, and the social machinery that brought them there from all the corners of the earth, would be a very different being from the mere parcel-tying and change-counting machine that usually weighs our tea and coffee.

I have thus dwelt upon some of the grounds for giving special emphasis to the word "conscious," believing that the advocates of Technical Education are too apt to regard the subject from a merely technical point of view. It is of the utmost importance that we should be convinced of the perfect harmony which naturally and necessarily exists between moral and material welfare, when the best and soundest means of obtaining either one or the other are followed, especially as there does exist in the minds of a certain class, both of workers and dreamers, a foolish prejudice and misconception, leading them to regard the advocates of Technical Education as a set of cold-blooded materialists, who look upon the workman as a mere productive engine which they seek to improve only in order to get more out of him. My opportunities of learning the opinions and feelings of the better class of self-improving workmen have been rather extensive, and I have met with this idea more frequently than one might suppose were possible. Certain flashy and trashy hollow-headed writers, who are constantly babbling about "the materialistic tendencies of the age" have encouraged these ideas, and as the arts of smart writing and showy oratory are so very easily acquired, this class of sentimentalists is very numerous.

The work above-named, which has suggested these remarks, is published in shilling parts, each containing a large number of well-selected and well-executed illustrations of art workmanship, a supplementary sheet of detailed working drawings, and essays on art-industry and miscellaneous technological subjects. Most of the illustrations are representative of continental art, and the character of the whole work is essentially German, including the typography, and some clerical errors in the English. As the chief use of such a work is to supply the English manufacturer with ideas that may help to emancipate him from slavish adherence to mere trade customs and models, this feature is advantageous, provided it does not foster the too common fallacy of believing that our continental neighbours have a monopoly of artistic taste—a fallacy which is sometimes carried to the length of an extravagant prejudice.

I have little doubt that if an equal amount of industry and taste were exerted in selecting models from the English fittings, English furniture, and English ornaments of English mansions, another and retaliatory "Workshop" of equal intrinsic merit, and equally suggestive to the continental workman, might be compiled.

There are several designs for German porcelain stoves, which are especially worthy of the attention of the English manufacturer. Their value is not confined to their artistic merits; the introduction to this country of such stoves would add much to the comfort and economy of English households, by taking the place of our barbarous open fire-places which give 90 per cent. of their heat to the clouds, and with the residue roast us on one side while the other is exposed to the cold blasts that converge from all sides towards the chimney, round which we are compelled to huddle whenever we have any really cold weather, such as that of last winter. The contrast between the genial, well-diffused warmth of the sitting-rooms of a well-ordered North German household and those of English houses of a corresponding class is anything but favourable to "the Englishman's fireside;" and as reason has so little power against prejudice, it may be well to call in art to the aid of science, in order to try whether the elegant designs of some of the German fire-places may have some effect upon those who reply to all demonstrations of the inefficiency and wastefulness of the English fireplace, that they must have an open fire "to look at," or on account of its "cheerful appearance."

A work of this kind, that a man may purchase or borrow from a library, and thus deliberately study at home, has a special value over and above that of Art Museums and International Exhibitions, though of course in these he has the great advantage of seeing the objects themselves.

The great fault of the work is the want of direct connection between the letter-press and the engravings. There are essays on various branches of art-manufacture, and illustrations of these; but the illustrations are distributed at random throughout the work, which, although published in separate parts, has no part complete in itself. A re-arrangement and proper classification of the materials of this book would greatly increase its value. The publishers may possibly suppose that by devoting certain shilling parts to knockers, hinges, gates, railings, and other ironmongery illustrations, another part to jewellery, another to mantelpieces, others to cabinet-work, &c., they would be holding out an inducement to their customers to buy only isolated numbers, while by the present arrangement, which sprinkles each man's special requirements throughout the work, they compel their subscribers to take the whole series. Whatever be the motive or origin of this arrangement, or want of arrangement, the commercial result must be to prevent many practical men from purchasing it at all, who would be glad to possess those parts relating to their own trades. As a mere picture-book, the confused miscellaneous arrangement may be the most popular, it gives great variety to the contents of each number; but in reference to higher usefulness this is a serious drawback to the merits of an otherwise valuable work. W. MATTIEU WILLIAMS

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### A New View of Darwinism

I AM much obliged to Mr. Howorth for his courteous expressions towards me in the letter in your last number. If he will be so good as to look at p. 111 and p. 148, vol. ii. of my "Variation of Animals and Plants under Domestication," he will find a good many facts and a discussion on the fertility and sterility of organisms from increased food and other causes. He will see my reasons for disagreeing with Mr. Doubleday, whose work I carefully read many years ago.

CHARLES DARWIN

Down, Beckenham, Kent, July 1

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THE very ingenious manner in which Mr. Howorth first misrepresents Darwinism, and then uses an argument which is not even founded on his own misrepresentation, but on a quite distinct fallacy, may puzzle some of your readers. I therefore ask space for a few lines of criticism.

Mr. Howorth first "takes it" that the struggle for existence "means, in five words, the persistence of the stronger." This is a pure misrepresentation. Darwin says nothing of the kind. "Strength" is only one out of the many and varied powers and faculties that lead to success in the battle for life. Minute size, obscure colours, swiftness, armour, cunning, prolificness, nauseousness, or bad odour, have any one of them as much right to be put forward as the cause of "persistence." The error is so gross that it seems wonderful that any reader of Darwin could have made it, or, having made it, could put it forward deliberately as a fair foundation for a criticism. He says, moreover, that the theory of Natural Selection "has been expressively epitomised" as "the persistence of the stronger," "the survival of the stronger." By whom? I should like to know. I never saw the terms so applied in print by any Darwinian. The most curious and even ludicrous thing, however, is that, having thus laid down his premisses, Mr. Howorth makes no more use of them, but runs off to something quite different, namely, that *fatness* is prejudicial to fertility. "Fat hens won't lay," "overgrown melons have few seeds," "overfed men have small families,"—these are the *facts* by which he seeks to prove that the *strongest* will not survive and leave offspring! But what does nature tell us? That the strongest and most vigorous plants *do* produce the most flowers and seed, not the weak and sickly. That the strongest and most healthy and best fed wild animals *do* propagate more rapidly than the starved and sickly. That the strong and thoroughly well-fed backwoodsmen of America increase more rapidly than any half-starved race of Indians upon earth. No *fact*, therefore, has been adduced to show that even "the persistence of the stronger" is not true; although, if this had been done, it would not touch Natural Selection, which is the "survival of the fittest."

ALFRED R. WALLACE

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## Our Natural History Museum

IN a few days the country will be called upon to vote 30,000*l*. or 40,000*l*. towards the erection of the new Natural History Museum at Kensington. 7,000*l*. were voted last year for the purpose of drawing up estimates and preparing the site, and our present one at Bloomsbury has become such a crying evil that we can scarcely anticipate a refusal of the grant.

So liberal a sum being offered at the shrine of Science, the community at large will necessarily expect great things of her, and first among all a radical redress of all existing grievances. Yet, if rumour whispers true, the prospects of the future are scarcely so brilliant or pregnant with promises of better things to come as they should be. Plans have been drawn up and decided upon, and the chiefs of the present Natural History Departments have been subsequently consulted as to the amount of space required for the several collections under their charge.

This is itself a faulty commencement, for the building should be constructed for the requirements of the collections, and not the collections cut to the size of the building, and, as might have been anticipated, such policy already threatens to prove productive of disappointment and dissatisfaction. Some departments will profit by the change, while others, including the one mostly needing an elargement of its borders, will absolutely have less than the present amount of space awarded it. We refer to the zoological one, whose present overcrowded and semi-arranged condition is a disgrace to the nation. And yet, on the completion of the present plans, this cramming process is threatened to be still further carried out, though it is to be hoped the voice of opposition and common sense will save us yet from so unfortunate a catastrophe. We hear again that no consideration whatever has been devoted to the subject of a library for the new building, nor a single foot of space allotted to the purpose of constructing one. Such a blunder as this surpasses the first one. The scientific volumes in the present library are in constant requisition by the officers of the various departments to assist them in the determination and arrangement of the specimens. Many of these again are unique or only replaceable at a great cost, and the inconvenience and loss of advantages that will arise to the official staff on being separated from the collection of works they now have access to, cannot be over-estimated. If the Natural History collections must be removed, an edifice suitable for their thorough utilisation, and replete with every convenience for prosecuting scientific research, including efficient laboratories, should be erected.

But to commence at the root of the evil. No progress can be expected under present auspices, or so long as the chief administration of the establishment, and the appointment and promotion of all officers, is vested in the hands of some fifty or sixty trustees, out of whom not more than two can be said to take a direct interest in the promotion of Natural Science. Nor, again, so long as such little discrimination is exercised in the distribution of these officers. Curiosity has prompted inquiries which have elicited anything but satisfying discoveries. We find men with talents for one branch of natural history stationed in departments where their particular talents cannot be utilised; recent zoologists in the geological department, palæontologists in the recent botanical one, and men peculiarly gifted for literary pursuits and without the slightest taste for scientific research, in the former. Taking next the department of Recent Zoology, the inadequacy of the present staff and the ill-proportioned attention that is devoted to particular sections, to the entire neglect of the remaining ones, are painfully apparent. In the Vertebrate division, though abundant room for improvement, there is not so much cause for censure; but on descending to the lower and far more bulky one of the Invertebrates, what do we find? Of a staff of five, two are conchologists, and the remaining three entomologists, while the Crustacea, Arachnida, and the whole of the old group of the Radiates, including the Echinodermata, Molluscoida, Cœlenterata, and Protozoa, are left to shift for themselves, and make way for the necessities of the others. Have we no men in England capable of superintending the arrangement of these neglected classes? or is it that the present remuneration for scientific work, for all but those highest in authority—so slender as to necessitate their utilising every leisure hour in eking out other means of subsistence, and taxing their brains, to the detriment of the amount of work discharged in official hours—deters them from coming forward? At any rate, the evil should be attended to, and the present glaring incongruities abolished. Whether new buildings are erected at Kensington, or the existing ones enlarged, it is absolutely incumbent that the administration shall be thoroughly reorganised. A permanent committee of some dozen eminently scientific men should supply the place of the present host of uninterested trustees, and the staff of officers should be distributed in accordance with the plan adopted in the Paris and various Continental Museums. Each zoological section should have its superintendent, with a number of assistants varying according to its requirements, while one governing mind should assume the responsibility and direct the machinery of the whole; and until such reformation is accomplished, there is no hope of any practical improvements. We do not see why the two large wings of the present establishment, now occupied as residences by the superior officers, should not be converted into exhibition rooms; space enough being reserved for one official residence on either side; and if necessary, additional suitable ones might be rented in the immediate neighbourhood, and the collections thus saved the unavoidable wear and tear of removal, and at the same time preserved in their present convenient position of access to the general public. But the exodus has been decided upon, and the question itself is of secondary importance compared with that of administration. On a future occasion I would direct attention to a few other points.

BATHYBIUS

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## Steam Lifeboats

THE *Globe* of Friday last contained a report of the proceedings of the Committee of the Steam Lifeship Fund, from which it would appear that the subject of the construction of a steam lifeship is seriously contemplated. As one who has for several years given great attention to this most desirable object, perhaps you will allow me to give the results of my labours.

It has already appeared to me that the object to be attained was not the construction of a lifeship, but rather the fitting of lifeboats with steam machinery, thereby improving their efficiency and diminishing the risk of life, as a boat so constructed could be worked, and that more efficiently, by at most three men, instead of the large number now required to man them. The only means of propulsion which can be applied is, in my opinion, the hydraulic propeller, as, the turbine being enclosed, all risk of fouling pieces of wreck, weed, &c., is thereby avoided. To attempt to use a lifeboat fitted with a screw or paddle would only be courting danger and disaster. Such being the case, the boat designed by me consists of three tubes, the outer ones being circular, and the centre one in which the propeller works being semicircular, and placed underneath the platform grating connecting the two circular tubes. The three tubes would be turned up and unite at the ends, and would somewhat resemble a whaleboat. The peculiar advantage of the hydraulic propeller when applied in this manner is, that the boat could be turned round on its own centre, and sent ahead or astern by the man in charge by simply turning a handle, without issuing an order to any one, an advantage which I need hardly say is of the very greatest moment under such circumstances as those in which lifeboats are usually employed.

The system of towing lifeboats by means of steam tugs to some point as near as possible to the site of the wreck, is one attended with danger, and the lifeboat, when cast off, is deprived of its means of propulsion at the very time when engine power would be most effective in enabling it to contend with the broken water round a wreck. I remember a case at Bombay, when a lifeboat proceeding to a wreck was towed right under, and the Chinese crew swept out of the boat and nearly all drowned.

Tubular lifeboats, I need hardly say, are no novelty, and the addition of a centre tube to carry the propeller and the steam engine and boiler will certainly not diminish their efficiency.

JOHN FELLOWES

Naval and Military Club, Piccadilly, July 3

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### The Internal Stucture of the Earth

ARCHDEACON PRATT'S letter in NATURE for June 22 calls for some remarks on my part. He communicates a few marginal notes written by Mr. Hopkins on a copy of the second part of my "Researches in Terrestrial Physics," which appeared in the "Philosophical Transactions," and seems seriously to regard these curt expressions as judicial utterances beyond which there can be no appeal.

In the first place, I am accused of incorrectly stating the nature of Mr. Hopkins's hypothesis as to the non-existence of friction between the fluid nucleus and solid shell of the earth. The words quoted from my paper as incorrect immediately follow a symbolical expression presented by Mr. Hopkins as the final result of his analysis, and my remark distinctly refers to this mathematical expression, and to nothing else. Remembering that the whole of Mr. Hopkins's mathematical investigations on the internal structure of the earth culminated in the deduction of this very expression, it is well to examine what are the words he uses in the course of his investigations which refer to the existence of friction between the shell and nucleus.

In his first memoir, "Philosophical Transactions," 1839, he says, "and since there will be no friction with the assumed perfect fluidity of the interior matter," p. 386. In his second memoir, I do not recollect that anything about friction is mentioned; but in his third, which summarises the whole of his preceding labours, after presenting the formula already alluded to, he states that it was established on the supposition of "the transition being immediate from the entire solidity of the shell to the *perfect* fluidity of the mass." He afterwards gives reasons for believing that a stratum of imperfect fluid probably exists between the shell and the perfect fluid, and he further uses the words, "Consequently *the assumption made in our investigations of the absence of all tangential action between the shell and fluid* will not be accurately true," p. 43. As my remark refers to these investigations and their immediate result, it is unnecessary to say to whom the charge of inaccuracy may justly apply. In affirming the existence of friction between the shell and nucleus to such an extent as to cause both to rotate as one solid mass, friction between the particles of the fluid is clearly implied; for if no such friction existed, the film of liquid touching the shell and moving with it might slip over the remainder of the nucleus. I have, therefore, been all along at issue with Mr. Hopkins on this point, when I concluded that the rotation of the shell and nucleus must take place as if the whole were solid. Mr. Hopkins declares this conclusion to be "a mechanical impossibility." It is this "impossibility" which has been reaffirmed by M. Delaunay in stronger terms than those I used. It has been shown to be not merely possible, but rigorously true, in a particular case, by an experiment of M. Champagneur, which I have myself recently verified, and it has been further so clearly illustrated in these pages by two correspondents A. J. M. and A. H. Green (May 18, p. 45) as to require no further observation. The coincidence of the axes of instantaneous rotation of the shell and nucleus necessarily follows if the whole moves as a solid mass; and to charge me with implying the coincidence in one of my formulæ is equivalent to charging me with being strictly consistent On this point Mr. Hopkins is of course at issue with M. Delaunay as well as with myself. The next important question referred to on which I totally differ from Mr. Hopkins is that of the form of the inner surface of the shell. If the shell has been gradually formed by solidification from a fluid mass, it is evident that the rate of progressive solidification at the interior of the shell must depend on the rate of refrigeration of the surface of the nucleus. This takes place, and has probably taken place for ages, at an almost insensible rate of slowness, and therefore also the successive additions of matter to the shell's inner surface. Between the perfectly solidified and comparatively rigid part of the shell and the fluid nucleus, the matter on the point of becoming solidified is probably in a pasty or imperfectly fluid state (as Mr. Hopkins has admitted), and it is this matter which is subjected to a moulding action by the changes of shape of the nucleus, as I pointed out in the publication already alluded to. This pasty matter becoming slowly impressed with the shape of the nucleus, and freely yielding to the impression as it passes to the solid state, the more rigid part of the shell, precisely as the outer case of a mould, is saved from strain, and cannot undergo a corresponding change of figure. In the discussion which followed the reading of my communication to the French Academy of Sciences on March 6, it appears from the *Comptes Rendus* that M. Elie de Beaumont made some remarks which illustrate and support this view of the process of formation of the shell. The conclusion to which I was thus led, that the inner surface of the shell could not be less elliptical than its outer surface, was reaffirmed soon after the publication of my researches by an eminent mathematician, the late Baron Plana, of Turin. All this Mr. Hopkins considers as quite inadmissible, and very reasonably, too, in the opinion of Archdeacon Pratt, and all the results deduced therefrom are judicially pronounced to be "valueless." But my conclusion as to the interior ellipticity of the shell is only a necessary deduction flowing from the fundamental principles from which my inquiries start, a principle upon which I am as much at issue with Mr. Hopkins as upon anything referred to in his marginal notes. As this is the really vital divergence between us, a few words of explanation are desirable

The hypothesis of the entirely fluid state of the earth anterior to its present state forms the groundwork of mathematical inquiries as to the earth's figure. The problem, as hitherto treated, always involved an additional hypothesis either openly or tacitly implied, namely, that the distribution of the particles composing the earth underwent no change by the earth's transition from a completely fluid condition to its present state. While Mr. Hopkins tacitly assumed this second hypothesis throughout his investigations, I have reason to believe that it was for the first time rejected in my paper on the "Figure and Primitive Formation of the Earth," which forms the first of my "Researches in Terrestrial Physics." By this step we are at liberty to investigate, with the aid of mechanical and physical laws and the known properties of the earth's materials, the probable arrangement and laws of density of the interior strata of the shell and nucleus. In attempting to do so, I was led to conclusions as to the earth's internal structure widely differing from those of Mr. Hopkins. I have great difficulty in believing that the crude comments on my researches communicated to Archdeacon Pratt, could have been intended to meet the public eye. Long before Mr. Hopkins sent these remarks to Archdeacon Pratt, he wrote to me promising to comment publicly upon my conclusions; and since then an opportunity occurred for pointing out in his presence at a meeting of the British Association what I conceived to be the inconclusive character of his results. Mr. Hopkins promised to reply, but neither this nor his former promise was ever realised. He avoided public discussion, whil as it now appears he *privately* depreciated results incompatible with his own. To Archdeacon Pratt I am grateful for producing evidence of the kind of weapon which I had long suspected to have been employed by my distinguished adversary. HENRY HENNESSY

Dublin, July 1

### Oceanic Circulation

MR. LAUGHTON treats an experiment which was only intended to be illustrative as if it had been advanced as probative, and tests it by a doctrine of "thermometric gradients" which does not correspond to the facts of the case. A uniform reduction of the temperature of ocean-water from the Equator to the Pole would doubtless give a "thermometric gradient" of infinitesimal minuteness. But the water of the circumpolar area, on which what Sir John Herschel truly designated the intense action of polar cold is exerted, brings with it so much of equatorial heat that a very decided increase of its specific gravity must be produced by the cooling process to which it is subjected within the polar area. This increase will be adequate, as I have attempted to show, to produce a continuous downward movement of the whole mass of water subjected to the cooling process; and such a movement, however slow, will make itself perceptible in a continuous outflow of the chilled dense water along the deepest floors of the great oceanic basins, and in a continuous indraught of warmer surface water into the polar area. The proof that such is the case seems to me to be afforded by the fact that temperatures not much above 32° seem to be uniformly met with at depths exceeding 2,000 fathoms, even under the equator; a fact of which Mr. Laughton and those who think with him have not, so far as I am aware, offered any account. That there is nothing in depth, *per se*, which produces this depression is shown by the absence of it in the Mediterranean.

It would be difficult, if not impossible, to carry out a probative experiment that should represent the actual conditions of the case. Taking the distance from the pole to the equator at 6,250 miles, and the average depth to which the chilled water would descend at 2½ miles, we should require a trough having a proportion of 2,500 to 1 between its length and its depth, or (in round numbers) a length of half a mile to a depth of a foot. Let it be supposed that cold were continuously applied by a powerful freezing mixture to the surface of the water occupying one extremity of the trough as far as one-tenth of its length, and that heat were applied to the surface of the water occupying the opposite extremity to a corresponding extent, the intervening water being neither heated nor cooled artificially, would, or would not, a continuous circulation from the one end of the trough to the other come to be established? To me it seems that what Sir John Herschel calls the "common sense of the matter" teaches that the continuous descending movement given to the water at the polar end of the trough must in time propagate itself to the equatorial, provided only that the conducting power of the sides and floor of the trough were sufficiently bad to prevent the chilled stratum which falls to the bottom at one end from losing its cold before it reaches the other.

When such masters of Thermotics as Pouillet and Herschel consider that the doctrine of a general oceanic circulation sustained by differences of temperature is conformable to the facts at present known, I would suggest whether it would not be wise if those who are interested in the subject, instead of attempting to controvert their views on theoretical considerations, were to use their endeavours to collect additional data for practically testing them. By the kindness of the Hydrographer to the Admiralty I hope, in the course of the present season, to obtain some further information of a reliable kind; and I am doing my utmost to urge upon our Government a systematic inquiry into what the Secretary of the Scottish Meteorological Society has truly designated (in a recent letter to me) as "the most important problem in Terrestrial Physics."

July 3 WILLIAM B. CARPENTER

I SHOULD need Mr. Laugbton's hint if I had ever supposed that the cause of the vertical circulation of the ocean could be determined by such an experiment as I suggested. The experiment was specially intended to throw light on the easterly and westerly oceanic movements. For this purpose it is only necessary that the rate of rotation of the shallow cylinder should be duly adjusted to the observed rate of the vertical motions. But even in this respect the experiment would afford but an illustration, not a demonstration.

The subject of oceanic circulation is altogether too wide and too difficult for discussion in letters. Every point touched on by Mr. Laughton requires many columns for its full discussion. I just note that the infinitesimal nature of the thermometric gradients scarcely seems a sounder objection to the temperature theory of oceanic circulation than to the temperature theory of atmospheric circulation. In one case, as in the other, we must integrate the effects of the solar light on tropical and subtropical regions. RICHARD A. PROCTOR

### Day Auroras

LAST evening, about eight o'clock, being in the grounds belonging to the Radcliffe Observatory, I was exceedingly surprised at seeing what I *have no doubt* of being true auroral streamers, forming a little to the east of the south meridian, reaching an altitude of about 25°, and after travelling some distance in a westerly direction, vanishing. This lasted at least ten minutes, when the sky, which had been overcast nearly all day again became so. I pointed the streamers out to several people who were near me, some of whom watched them with me, as a proof of what I had before doubted, namely, that auroras are visible by daylight. JOHN LUCAS,
Assistant at Radcliffe Observatory

Radcliffe Observatory, Oxford, June 28

### The Solar Parallax

I REGRET that I have misinterpreted the severity of Prof. Newcomb's remarks respecting my chapter on the Solar Parallax. The fact is, that so far back as February 1 I was warned by an eminent astronomer that Prof. Newcomb had vowed here last November that he would annihilate all who upheld the finality or correctness of Mr. Stone's researches.

Prof. Newcomb must be sensible that his offer to supply information as to the history of inquiries into the solar parallax during the last few years is a very generous one; and that it will be immensely to my advantage to profit by his exceptional familiarity with the subject. I thank him very earnestly. I have an especial distaste for inquiries into the historical parts of scientific subjects, and shall rejoice to be saved the labour of looking up authorities, &c., in this particular matter. If I find my account requires alteration, I shall admit the fact without a particle of hesitation. It is indeed most desirable (though not, perhaps, for students of science, for whom I specially write, and who need trouble themselves little on the matter) that to each worker in the subject of the solar parallax his due proportion of credit should be assigned; and as in this case not only I, but Sir John Herschel, as well as the Council of the Astronomical Society, would seem to have done Prof. Newcomb less than justice, the sooner recantation is made the better.

Prof. Newcomb refers to "the kind spirit in which I have taken his remarks;" meaning rather, perhaps, the appreciative way in which I have spoken of his labours. His critique, regarded as a whole, was not, I take it, kindly meant; and though I by no means feel annihilated by it, I should be speaking untruly if I seemed to admit its justice. If I failed to note how I viewed his comments, it was only because I found a pleasanter subject to speak about in those important researches whereby he bas advanced astronomy. RICHARD A. PROCTOR

P.S.—I take this opportunity of noting that the remark in my former letter respecting the work of Mr. De La Rue and F. Secchi in 1860 must not be understood as implying that the account in F. Secchi's book *Le Soleil* is incorrect. On the contrary, I have no doubt it is strictly accurate. I was fortunate in securing a copy of *Le Soleil* before Paris was beleaguered, and derived considerable assistance from its perusal.

### Lee Shelter

PERHAPS it is worth noting that a lee shelter is almost as effectual as a screen to the windward. The fact may be quite well known and understood; but I did not become aware of it till I was on Bognor Pier, when a strong gale was blowing directly on the broadside. There are seats backed and covered overhead and on the sides, alternately, on the one or other side of the pier, and on this occasion all the seats to the windward were occupied, so that, wanting a rest, I had to put up with one

of those directly facing the gale. I naturally expected to have it strong in my face; but, on the contrary, I found I had almost as perfect a shelter from the wind as if I had been on the other side.
C. M. INGLEBY

Malvern Wells, July 3

## AFFINITIES OF THE SPONGES

MR. H. J. CARTER is devoting much attention at the present moment to the study of the Protozoa. In March last he published in the *Annals* and *Magazine* of *Natural History* the results of his investigations on Coccoliths and Coccospheres, stating his opinion that these minute bodies are of vegetable and not animal organisation, as hitherto supposed. Should his supposition prove correct, it will materially modify the theory of the mode of support of animal life at great depths, advocated by many recent deep-sea explorers. In the pages of the same journal for this month (July), Mr. Carter lays before us the results of his more recent researches into the ultimate structure of the marine calcareous sponges, and which entirely harmonise with those already arrived at by Prof. James Clark, of Boston, U.S. The sum total of these are that the Spongiadæ, as a group, are most closely allied to the Flagellate Infusioria; the animal portions of the genera *Leuconia*, *Grantia*, and *Clathrina* among the calcareous sponge-forms, and *Spongilla*, *Isodictya*, *Hymeniacidon*, and *Cliona* among the silicious representatives examined by Mr. Carter, being found by him to consist, for the most part, of aggregations of the same peculiar funnel-bearing ciliated cells characteristic of the new Flagellate Infusorial genera *Codosiga*, *Salpingœca*, *Bicosœca*, &c., introduced by Prof. Clark. The only point at issue between these two explorers in the same field is, whether each separate cell possesses a distinct mouth, or is capable of engulphing food, after the manner of an ordinary Rhizopod, through any portion of its body. Mr. Carter here adopts the latter view.

The most important result of Mr. Carter's investigations is, however, the additional evidence he brings forward in refutation of Ernst Haeckel's no longer tenable hypothesis, that the sponges are most closely allied to, and should even be collated in the same primary group as, the Cœlenterata. Prof. Haeckel's opinions have already been strongly opposed by myself (See *Ann.* and *Mag. Nat. Hist.* for March and September 1870); and Mr. Carter's recent investigations practically deprive Prof. Haeckel and those supporting his views of their last foot-hold. The Calcispongiæ is the group on which Ernst Haeckel and his collaborateur Mickluco-Maclay have more particularly concentrated their attention; it is the especial one, again, they have made choice of, as demonstrating in their opinion, more closely than any, the relationship they would seek to establish. Prof. Clark and Mr. Carter, however, prove beyond doubt their bond of union with the Flagellate Infusoria, the addition of a general investing sarcode layer and a spicular or horny supporting skeleton being, indeed, the only clearly defined characters that separates them from the group.

In seeking to establish other affinities, Mr. Carter is scarcely so happy. In his opinion, the Spongiadæ are more closely allied to the compound Tunicata than to the Cœlenterata, but he allows himself to be led further away here by analogous or general external resemblances than even Prof. Haeckel. To effect his purpose, he proposes that the branchial openings in the gelatinous mass of *Botryllus* "are analogous if not homologous" with the pores of the Spongiadæ, while the common cloacal cavity and fæcal orifice are respectively analogous to the excretory canal system and vent. Fascinating as these external resemblances may appear at first sight, we must penetrate a little beneath them, and before Mr. Carter can hope to substantiate the affinities he would establish, he must demonstrate to what extent the individual zooids of the Ascidian colony can be correlated with the single or aggregated ciliated cells of the sponges. In the former we have highly-organised animals, possessing a well-developed neural, hæmal, digestive, and respiratory system, while in the latter, simple uniciliated cells and undifferentiated sarcode are the only materials to be dealt with. Mr. Carter, again, would institute comparisons between the tough, gelatinous, or albuminous mass in which the Ascidian zooids are embedded, and that sarcode layer more or less generally diffused throughout all sponge structures; but in the first we have formed matter, like bone, horn, or shell, no longer possessing vital properties, while in the sarcode of the sponge we have living substance constantly altering its conditions of relationship, secreting the supporting skeleton, and contributing to the general welfare of the sponge community. Mr. Carter's inference in support of his proposition, drawn from the presence of calcareous bodies resembling spiculæ being met with in certain compound Ascidia, is but of little importance, considering that comparisons on the same grounds might be made between the sponges and the Nudibranchiate Mollusca; these latter likewise frequently secreting calcareous spiculæ in the substance of their integument.

The hiatus between the Spongiadæ and the Tunicata is far too wide to admit of such an institution of homological comparisons; the group of the Cœlenterata is evidently the nearest related to the former, but even here there are at present too many important links wanting to justify our uniting the two in one sub-kingdom, as proposed by Haeckel. *Inter se*, the sponges constitute a very natural division of the Protozoa, intimately related on the one hand through their special ciliated cells to the Flagellate Infusoria, and by the remaining sarcode layer, or skeletal secreting portion, to the simpler Rhizopoda.

In the paper here alluded to, Mr. Carter describes, under the name of *Trychogypsia*, a new calcareous sponge form differing from all others with which he is acquainted in possessing linear fusiform and no triradiate or quadriradiate spicules. The genus *Aphroceras*, described by Dr. Gray in 1858 (see Proc. Zoo. Soc., pp. 113, 114), is recognised by the same characters.

W. SAVILLE KENT

## ON RECENT MOA REMAINS IN NEW ZEALAND

IN January 1864 a remarkably perfect specimen of *Dinornis robustus*, Owen, found on the Manuherekia Plains in the interior of the Province of Otago, was transmitted to the museum at York, and formed the subject of a memoir by Prof. Owen in the Transactions of the Zoological Society for 1869. These remains were considered unique on account of the well-preserved condition of some parts of the skeleton, portions of the ligaments, skin, and feathers being still attached to some of the bones, whereas Moa bones in the condition in which they are usually found are partially fossilised, or have at least undergone a sufficient change to deprive them not only of all ligamentous appendages, but to some extent of their proper proportion of organic matter. The discovery in the following year of the unique specimen (now in the museum) of a Moa's egg containing the bones of an embryo chick and attached membranes—within twenty miles of the same locality—was recorded by me in 1867 (Proc. Zool. Soc. p. 991.) I have now to announce the acquisition of another interesting specimen from the same district, being the cervical vertebræ of a Moa, apparently of the largest size, upon the posterior aspect of which the skin, partly covered with feathers, is still attached by the shrivelled muscles and ligaments.

I saw the specimen in question in the possession of Dr. Thomson, of Clyde, who obtained it from a gold miner. It was discovered in a cave formed by an overhanging mass of mica schist, but the particulars of the locality have not yet been accurately ascertained, or whether any other parts of the bird are still to be found. Dr. Thomson has kindly undertaken to prosecute a further search, and to forward the specimen already obtained to the museum for examination.

These interesting discoveries render it probable that the inland district of Otago, at a time when its grassy plains and rolling hills were covered with a dense scrubby vegetation or a light forest growth, was where the giant wingless birds of New Zealand lingered to latest times. It is impossible to convey an idea of the profusion of bones which, only a few years ago, were found in this district, scattered on the surface on the ground or buried in the alluvial soil in the neighbourhood of streams and rivers. At the present time this area of country is particularly arid as composed with the prevalent character of New Zealand. It is perfectly treeless; nothing but the smallest-sized shrubs being found within a distance of sixty or seventy miles. The surface features comprise round-backed ranges of hills of schistose rock with swamps on the top, deeply cut by ravines that open out on basin-shaped plains formed of alluvial deposits that have been everywhere moulded into beautifully regular terraces to an altitude of 1,700 feet above the sea level. That the mountain-slopes were at one time covered with forest, the stumps and prostrate trunks of large pine trees, and the mounds and pits on the surface of the ground which mark old forest land, abundantly testify, although it is probable that the intervening plains have never supported more than a dense thicket of shrubs or were partly occupied by swamps. The greatest number of moa bones were found where rivers debouch on the plains, and that at a comparatively late period these plains were the hunting grounds of the Aborigines can be proved most incontestably. Under some overhanging rocks in the neighbourhood of the Clutha river, at a place named by the first explorers Moa Flat, from the abundance of bones which lay strewn on the surface, rude stone flakes of a kind of stone not occurring in that district were found associated with heaps of Moa bones. Forty miles further in the interior, and at the same place where the Moa's neck was recently obtained, Captain Fraser discovered in 1864 what he described to me as a manufactory for such flakes and knives of chert as could be used as rough cutting instruments in a cave formed by overhanging rocks, sheltered only from S.W. storms, as if an accumulation by a storm-stayed party of natives. With these were also associated Moa bones and other remains. Again, on the top of the Carrick Mountains, which are in the same district, but at an altitude of 5,000 feet above the sea, the same gentleman discovered a gully, in which were enormous heaps of bones, and along with them native implements of stone, among which was a well-finished cleaver of blue slate, and also a coarsely-made horn-stone cleaver, the latter of a material that must have been brought from a very great distance.

Still clearer evidence that in very recent times the natives travelled through the interior, probably following the Moas as a means of subsistence, like natives in other countries where large game abounds, was obtained in 1865-6 by Messrs. J. and W. Murison. At the Maniototo Plains, bones of several species of Dinornis, Aptornis, Apteryx, large Rails, Stringops, and other birds, are exceedingly abundant in the alluvium of a particular stream, so much so that they are turned up by the plough with facility. Attention was arrested by the occurrence on the high-ground terrace which bounds the valley of this stream, of circular heaps composed of flakes and chips of chert, of a description that occurs only in large blocks along the base of the mountains at about a mile distant. This chert is a very peculiar rock, being a "cement" or "water quartz," or sand and gravel converted into a hard quartzite by infiltration of silicious matter. The resemblance of the flakes to those they had seen described as found in the ancient Kitchen-Middens, and a desire to account for the great profusion of Moa bones on a lower terrace-shelf nearer the margin of the stream, led the Messrs. Murison to explore the ground carefully, and by excavating in likely spots, they found a series of circular pits partly lined with stones, and containing, intermixed with charcoal, abundance of Moa bones and egg-shells, together with bones of the dog, the egg-shells being in such quantities that they consider that hundreds of eggs must have been cooked in each hole. Along with these were stone implements of various kinds, and of several other varieties of rock besides the chert which lies on the surface. The form and contents of these cooking-ovens correspond exactly with those described by Mantell in 1847 as occurring on the sea-coast, and among the stone implements which Mantell found in them, he remembers some to have been of the same chert, which occurs *in situ* at this locality fifty miles in the interior. The greater part of these chert specimens found on the coast are with the rest of the collection in the British Museum. There are other circumstances which incidentally support the view that while the Moas still existed in great numbers, the country was open and regularly traversed by the natives engaged in hunting. Near the old Maori ovens on the coast Mantell discovered a very curious dish made of steatite, a mineral occurring in New Zealand only on the west coast, rudely carved on the back in Maori fashion, measuring twelve by eight inches, and very shallow. The natives at the time recognised this dish by tradition, and said there should be two of them. It is very remarkable that since then the fellow dish has been discovered by some gold diggers in the Manuherikia Plain, and was in use on a hotel counter at the Dunstan township as a match-box, till lately, when it was sent to England, and, as I am informed, placed in a public museum in Liverpool.

The manner in which the Maoris use their cooking ovens suggests to me an explanation of the mode in which these flakes of chert came to be found in such profusion, while only a few of them show any signs of having been trimmed in order to fit them for implements. The native method of cooking is to heat the hardest stones they can find in the fire, and then placing the food to be cooked on top, to cover the whole with leaves and earth, and through an opening pour in water, which coming in contact with the hot stones, causes the formation of steam by which the food is cooked. If masses of the white chert be heated and quenched with water in the manner described, the result is the formation of flakes of every variety of shape with sharp cutting edges. It is natural to suppose that when one of these flakes was found of shape convenient for a particular purpose, such as a knife, cleaver, or spear-head, it was trimmed and dressed in the manner of a gun-flint, when the edge became defective, rather than thrown away, and favourite forms might be preserved and carried even as far as the coast. This suggested explanation of how a race advanced probably far beyond the period of such rude-looking implements might yet find it convenient to manufacture and use these, is supported by the circumstances that along with the trimmed chert flakes the Messrs. Murison found finished adzes of aphanite and even jade, which shows that the hunting natives had the same implements as those which are so common among the natives at the present day, though their use is now superseded by iron.

In the ovens on the coast, besides flakes and rough knives of chert and flint, are found flake-knives of obsidian, a rock which only occurs in the volcanic district of the North Island, and also adzes and stone axes of every degree of finish and variety of material. Although there is no positive evidence in the latter case that more highly finished implements were in use by a people contemporaneous with the Moa, whose remains, collected by human agency, are so abundant in the same place, nevertheless the fact of a similar association occurring far in the interior affords strong presumptive evidence on this point, as the finely-finished implements must have been carried inland and to the same spots where the Moa remains occur, to be used at native feasts, of which these bones are the only other existing evidences.

So far I have been dealing with evidence gathered in the South Island of New Zealand of the recent co-existence of Man and the Moa, but in the North Island there is no lack of similar proofs. During the summer of 1866, H.E. Sir George Grey, K.C.B., made a fine collection at Waingongoro on the west coast of this island, being the same locality from which Mantell gathered the magnificent series of bones which he forwarded to Europe in 1847. At this place, along with the bones of the Moa and other extinct birds, were found those of dogs, seals, and many species of birds that are common at the present day, such as the albatross, penguin, nestor, and porphyris, and notably the notornis, a gigantic rail, which till a comparatively recent date, was supposed, like the Moa, to be extinct, and of which as yet only two living examples have been obtained. Associated with these remains Sir George Grey obtained artificially formed stone flakes of a very peculiar kind, being chips from rolled boulders of hard crystalline sandstone, produced by a single blow, probably when the stone was heated and quenched in water. The stones from which these chips were obtained had evidently been used, in the first instance, for cooking, as the ancient *Umus*, or cooking-ovens, are chiefly formed of them; and, indeed, in the sandy tracts on the west coast, where stones are rare, the identical stones that in former days were used for cooking Moas are still in use by the natives of the district for cooking pigs and shell-fish. Here again we find that the same necessity and circumstance which suggested the use of the chert flakes in the South, gave origin to a similar adaptation of the chips from the sandstone boulders. It is of some interest to find that native tradition points to the sandy flat at Waingongora, called Te Rangatapu, as the spot where the first Maori immigrants to the district originally settled; and there appears to be nothing in the abundant traces which they have left of these great feasts, which we must refer to that period, that would indicate any difference in their domestic habits from those of the Maoris now existing, and who, no doubt, are their direct descendants.

What has been advanced affords strong presumptive evidence that the Moas, although belonging probably to a race that was expiring from natural causes, was finally exterminated through human agency; and on this subject Mr. Murison has suggested how infallibly the wholesale consumption of the eggs, which were evidently highly prized as an article of food, must have led to their rapid extinction, without its being necessary that the birds themselves should have been actually destroyed. That wide-spreading fires contributed, in some instances, to the destruction of these wingless birds, is rendered probable from the occurrence of little heaps of bones, in spots where flocks of them would be overtaken when fleeing before the destroying element. At the south-west extremity of a triangular plain, by the side of the Wakatipu Lake, in 1862, I counted thirty-seven of such distinct skeleton-heaps, where the steep rocky slope of the mountain, covered with fallen blocks and tangled shrubs, meets the lake, and would, therefore, stop the progress of the fugitives in that direction. From what we know of habits of birds akin to the Moas, we may fairly infer that they did not frequent heavily-timbered country, but roamed over coppice-covered plains and mountain slopes. This view is supported by the comparative rarity of Moa remains in forests, the few exceptions being easily accounted for.

The whole of the eastern district of the South Island of New Zealand back to the Southern Alps was completely surveyed and mapped as early as 1862, and had been thoroughly explored at least ten years before that date, without any of these gigantic birds being met with; but there is a large area of rugged mountainous country, especially in the south-west district of Otago, which even to the present time is only imperfectly known. The mountain sides in this region are clothed with open forest, in which Kiwis, Kakapos, and other expiring forms of apterous birds are still to be found in comparative abundance, but where we could scarcely expect to meet with the larger species. Nevertheless, owing to the lofty tabular configuration of this district, the mountains afford very extensive areas above the forest limit—which are covered with Alpine shrubs and grasses—where it is not impossible that a remnant of this giant race may have remained to very recent times. The exploration, however, to which the country has been subjected during the last few years, by parties of diggers prospecting for gold, forbids any reasonable hope that any still exist. I may here mention that on one of the flat-topped mountains near Jackson's Bay, visited in January 1863, I observed, at an altitude of 4,000 feet, numerous well-beaten tracks about sixteen inches wide intersecting the dense scrub in all directions, and which, owing to the height of the scrub (two to four feet) could only been formed in the first instance by the frequent passage of a much larger bird than either the Kiwi or Kakapo, which, judging from the droppings, were the only birds that now resorted to them. On the sides of the tracks, especially near the upper confines of the forest, are shallow excavations, 2ft. to 3ft. in diameter, that have much the appearance of having been scraped for nests. No pigs or any other introduced animals having penetrated to this part of the country, it appears manifest that these were the tracks of some large indigenous animal, but from the nature of the vegetation it is probable that such tracks may have been for a very long period in disuse, except by the smaller ground birds, without becoming obliterated.

The above facts and arguments in support of the view that the Moa survived to very recent times, are similar to those advanced at an early period after the settlement of the colony, by Walter Mantell, who had the advantage of direct information on the subject from a generation of natives that has now passed away. As the first explorer of the artificial Moa beds, his opinion is entitled to great weight. Similar conclusions were also drawn by Butler, who is personally familiar with the facts derived from the North Islands, in an article which appeared in the *Zoologist* for 1864. The fresh discovery therefore of well-preserved remains of the Moa only tends to confirm and establish these views; and it would have been unnecessary to enlarge on the subject by the publication of the foregoing notes, which for the most part were written several years ago, but for the entirely opposite conclusions advanced by Dr. Haast in a recent address, which, from the large amount of interesting and novel matter it contains, will doubtless have a wide circulation.

JAMES HECTOR

## *ON THE GASEOUS AND LIQUID STATES OF MATTER*

A DISCOURSE was delivered on Friday evening, June 2, at the Royal Institution in Albemarle Street, by Dr. Andrews on the "Gaseous and Liquid States of Matter," from which we make the following extracts:—"The liquid state of matter forms a link between the solid and gaseous states. This link is, however, often suppressed, and the solid passes directly into the gaseous or vaporous form. In the intense cold of an arctic winter, hard ice will gradually change into transparent vapour without previously assuming the form of water. Carbonic acid snow passes rapidly into gas when exposed to the air, and can with difficulty be liquefied in open tubes. Its boiling point, as Faraday has shown, presents the apparent anomaly of being lower in the thermometric scale than its melting point, a statement less paradoxical than it may at first appear, if we remember that water can exist as vapour at temperatures far lower than those at which it can exist as liquid. Whether the transition be directly from solid to gaseous, or from solid to liquid and from liquid to gaseous, a marked change of physical properties occurs at each step or break, and heat is absorbed, as was proved long ago by Black, without producing elevation of temperature. Many solids and liquids will for this reason maintain a low temperature, even when surrounded by a white hot atmosphere, and the remarkable experiment of solidifying water and even mercury on a red hot plate, finds thus an easy explanation. The term spheroidal state, when applied to water floating on a cushion of vapour over a red hot plate, is, however, apt to mislead. The water is not here in any peculiar state. It is simply water evaporating rapidly at a few degrees below its boiling point, and all its properties, even those of capillarity, are the properties of ordinary water at 96°·5C. The interesting phenomena exhibited under these conditions are due to other causes, and not to any new or peculiar state of the liquid itself. The fine researches of Dalton upon vapours, and the memorable discovery by Faraday of the liquefaction of gases by pressure alone, finished the work which Black had begun. Our knowledge of the conditions under which matter passes abruptly from the gaseous to the liquid and from the liquid to the solid state may now be regarded as almost complete.

"In 1822 Cagniard de la Tour made some remarkable experiments, which still bear his name, and which may be regarded as the starting point of the investigations which form the chief subject of this address. Cagniard de la Tour's first experiments were made in a small Papin's digester constructed from the thick end of a gun barrel, into which he introduced a little alcohol and also a small quartz ball, and firmly closed the whole. On heating the gun barrel with its contents over an open fire, and observing from time to time the sound produced by the ball when the apparatus was shaken, he inferred that after a certain temperature was attained the liquid had disappeared. He afterwards succeeded in repeating the experiment in glass tubes, and arrived at the following results. An hermetically sealed glass tube, containing sufficient alcohol to occupy two-fifths of its capacity, was gradually heated, when the liquid was seen to dilate, and its mobility at the same time to become gradually greater. After attaining to nearly twice its original volume, the liquid completely disappeared, and was converted into a vapour so transparent that the tube appeared to be quite empty. On allowing the tube to cool, a very thick cloud was formed, after which the liquid reappeared in its former state.

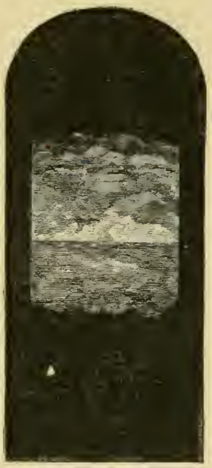

FIG. 1—Cloud below critical point

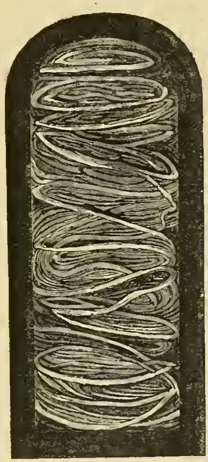

FIG. 2—Striæ above critical point

"It is singular that in this otherwise accurate description Cagniard de la Tour should have overlooked the most remarkable phenomenon of all—the moving or flickering striæ which fill the tube, when, after heating it above the *critical point*, the temperature is quickly lowered. This phenomenon was first observed by the lecturer in 1863, when experimenting with carbonic acid, and may be admirably seen by heating such liquids as ether or sulphurous acid in hermetically sealed tubes, of which when cold they occupy about one-third of the capacity. The appearances exhibited by the ascending and descending sheets of matter of unequal density are most remarkable, but it is difficult to give an adequate description of them in words or even to delineate them.

"These striæ arise from the great changes of density which slight variations of temperature or pressure produce when liquids are heated in a confined space above the critical point already referred to; but they are not formed if the temperature and pressure are kept steady. When seen they are always a proof that the matter in the tube is homogeneous, and that we have not liquid and gas in presence of one another. They are, in short, an extraordinary development of the movements observed in ordinary liquids and gases when heated from below. The fact that at a temperature 0°·2 above its critical point carbonic acid diminishes to one-half its volume from an increase of only $\frac{1}{87}$ of the entire pressure is sufficient to account for the marked characters they exhibit.

"If the temperature is allowed to fall a little below the critical point, the formation of cloud shows that we have now heterogeneous matter in the tube, minute drops of liquid in presence of a gas. From the midst of this cloud (as shown in Fig. 1) a faint surface of demarcation appears, constituting the boundary between liquid and gas, but at first wholly devoid of curvature. We must, however, take care not to suppose that a cloud necessarily precedes the formation of true liquid. If the pressure be sufficiently great, no cloud of any kind will form."

After describing the results obtained by the lecturer with carbonic acid under varied conditions of temperature and pressure, of which a full account has already appeared in NATURE,* Dr. Andrews remarked that it would be erroneous to say that between liquid and gas there exists one intermediate state of matter, but that it is correct to say that between ordinary liquid and ordinary gas there is an infinite number of intermediate conditions of matter, establishing perfect continuity between the two states. Under great pressures the passage from the liquid to the gaseous state is effected on the application of heat without any break or breach of continuity. A solid model, constructed by Prof. J. Thomson, from the data furnished by the experiments of the lecturer, exhibited very clearly the different paths which connect the liquid and gaseous states, showing the ordinary passage by break from the liquid, as well as the continuous passages above the critical point.

After referring to the experiments of Frankland on the change produced by pressure in the spectrum of hydrogen, and to those of the same able chemist and Lockyer on the spectrum of the spark in compressed gases, Dr. Andrews described the remarkable change from a translucent to an opaque body, which occurs when bromine is heated above the critical point; and then drew attention to the general fact that when the critical point is reached, the density of the liquid and gas become identical.

In order to establish the continuity of the solid and liquid states, it would be necessary in like manner, by the combined action of heat and pressure, to obtain the solid and liquid of the same density and of like physical properties. To accomplish this result would probably require pressures far beyond any which can be reached in transparent tubes, but future experiment may show that the solid and liquid can be made to approach to the required conditions.

* See NATURE, vol. ii. p. 278.

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## *ON AN ADDITIONAL TRUE RIB IN THE HUMAN SUBJECT*

THE almost absolute rule that there are seven true ribs in the human subject has, like every other rule, its exceptions. Occasionally instances are met with in which there are eight sternal ribs on one or both sides. But Nature does not effect her evolutions by *per saltum* transitions between extreme points, but steadily makes progress by degrees almost imperceptible to human intelligence. So in the matter of rib transition, there are various grades met with between the presence of a complete eighth sternal rib on the one hand, and its absence on the other. In the sternum of a female subject recently dissected at the Royal College of Surgeons, the right seventh and eighth rib cartilages blended together about a quarter of an inch distant from the mesosternum. On the left side the eighth rib cartilage was arrested about an inch and a half from the mesosternum. The latter was free at its sternal end. In another subject—a moderately muscular male—the eighth rib cartilage on the right side extended within an inch of the mesosternum, its extremity being free. On the left it was aborted at the distance of two and a half inches from the mesosternum. In the latter subject the sternum was exceedingly large; all the rib cartilages, especially the seventh on the left side, were well developed, and the xiphisternum was very much elongated, spatulate, and curved in an anterior direction. Occasionally specimens are met with in which the sixth rib cartilage is implanted upon the distal extremity of the mesosternum (rather than upon its distal lateral aspect), lying in front of the xiphisternum, and separated from its fellow of the opposite side by a small interval. In the receding angle formed by their divergence, the seventh sternal ribs are placed, lying directly upon the xiphisternum, and articulating with it, barely attaining an attachment to the mesosternum. This closely simulates the arrangement met with when the eighth sternal rib is present.

In another adult male skeleton, I found a complete specimen of an eighth sternal rib, but only on the right side. It articulated with the xiphisternum, and not with the mesosternum. On the left side the seventh sternal rib cartilage was larger than the corresponding one on the right side, and articulated with both the mesosternum and xiphisternum.*

On examining the skeletons (human) in the Hunterian Museum, I noticed another instance of an eighth sternal rib in an adult male African negro, occurring on the right side only. It was in every respect similar to the preceding. This is the only instance out of the fifteen skeletons (human) contained in the museum which deviated from the average standard number of seven true ribs. It is just possible that it may be more frequently present and remain undetected. In maceration the cartilages are very frequently removed, and articulators prepare artificial ones in their place corresponding to the average seven.

On examining the higher quadrumana, &c., I noticed that this additional true rib was present only in one young chimpanzee, but not in the gorillas and orangs. It was present in the gibbon and silvery gibbon, the pig-tailed monkey, *Macacus Rhesus*, Galeopithecus, and Indri. The aye-aye, the slender lemur, and the squirrel monkey, have each nine true ribs. The grand galago, the awantilo, the slender loris, the douroucouli, and the potto, have each ten true ribs. Prof. Flower very kindly called my attention to a paper on the axial skeleton of the Primates by Mr. St. George Mivart,† in which these rib variations are described as follows:—"In the highest forms of the Primates, the number of true ribs is seven, but in Hylobates there are sometimes eight pairs. In Semnopithecus and Colobus there are generally seven, but sometimes eight pairs of true ribs. In the Cynopithecinæ the normal number is eight. In the Cebidæ there are generally seven or eight pairs, but in Ateles sometimes nine. In Hapale there are sometimes as few as six, sometimes as many as eight; seven or eight in Galago, Lemur, and Indris; nine in Cheiromys. The highest number, as might be expected, is found in the Nycticebinæ, there being as many as ten pairs of true ribs in Perodicticus and Loris."

Professor Flower remarks ‡ that "in the higher Simiina the ribs do not differ very notably from those of man, except in number; but in the lower forms, and especially in the Lemurina, they more resemble those of the Carnivora."

In the Carnivora the number of nine sternal ribs is fairly constant. There are some exceptions, however, *e.g.* the Esquimaux dog—the Arctic wolf and Proteles have only eight true ribs. The common badger (*Meles taxus*) has ten true ribs—the tenth rib being implanted on the apex of the xiphisternum. The ninth rib in all these animals is more or less intimately associated with the xiphisternum, but rarely forming so decided an articulation with it as in the badger.

In a dog's sternum lately in my possession, the xiphisternum had the ninth rib articulated directly with it.

* This does not obviate the rule laid down by Prof. Flower in his recent admirable book on the Osteology of the Mammalia, that the xiphisternum never carries any true ribs. This is the average rule. But variations are frequent, although they cannot be considered in a text-book on average, and not irregular, Osteology.

† Proceedings of the Zoological Society of London June 27, 1865.

‡ Osteology of the Mammalia, p. 89.

The former (xiphisternum) was bifid through its whole length.

The scientific value of this additional sternal rib—in a Darwinian sense—is simply great. It evinces in a clear and forcible manner a latent disposition in the human subject, either to revert to an original and lower condition, or to retain traces of that previous condition. We have already seen that some of the lowest forms of Primates have ten true ribs, others have nine, some eight, and others again seven, as in the human subject. But it is interesting, indeed, to find that the conflict between the major number ten and the minor seven takes place in the lower Primates. As we pass up to the higher Primates, there seems to be a decided tendency towards fixity at the number of seven true ribs. But yet a few solitary examples—besides the human subject—illustrate the lower type, as in the chimpanzee already mentioned. The number of ribs in the lower forms of monkeys seems to be a repetition of that in the Carnivora, and subject to the same fluctuations between seven and ten true ribs. Although the few specimens which I have examined of the higher Primates show a decided tendency towards fixity at the number of seven, yet I believe that in a very large number of skeletons of each of the higher species, various transitional grades would be met with closely according with those in the human subject. It is somewhat remarkable that each of the variations of the eighth rib in the human subject which I have described should all be on the right side.

From the preceding facts it may be decidedly inferred that the tenth, ninth, and eighth true ribs are gradually lost in the transition from the lower to the higher Primates, except in a few isolated examples. The recurrence of the eighth true rib in the human subject cannot be looked upon as an accident, any more than the presence of a distinct peroneus quartus, and a moderately large extensor primi internodii hallucis coming from the tibialis anticus, exactly as in the chimpanzee, in the same individual whose sternum, with an almost complete eighth rib, has been described.

J. BESWICK-PERRIN

## *NOTES*

THE men of the North do not seem disposed to let grass grow under their feet in respect to their proposed College of Physical Science, at Newcastle-upon-Tyne. Of the 35,000*l*. required, in addition to to the Durham University endowment, to carry out their plans, upwards of 23,600*l*. has been already subscribed. Three of the professorial chairs have now been filled, viz.:—Experimental Physics: A. S. Herschel, M.A. Chemistry: A. Freire-Marreco, M.A. Geology: David Page, LL.D., F.R.S.E. No decision has yet been made public in respect to the professorship of Mathematics. This appointment, together with the chair of Experimental Physics, is in the hands of the Dean and Chapter of Durham. These selections will give general satisfaction, and are sufficient assurance of the desire of the Committee to obtain the services of the men within reach, without reference to local influence or predilections. Indeed it seems to us just possible that the claims of one eminent local geologist may have suffered somewhat through the fear of a charge of partiality. Few family names stand higher in the scientific world than that of Herschel, and its present representative is well known as a teacher of experimental philosophy. M. Freire-Marreco has long served the University of Durham as its reader in chemistry and the Newcastle College of Medicine as its lecturer. Apart from his acquirements as a chemist and his ability as a teacher, there is perhaps no one who is so thoroughly versed in the chemical technology of the industries of the North of England. Dr. Page's elementary works on geology are widely appreciated, and if one may judge of his capacity as a lecturer by his power of interesting a general audience, he is eminently fitted to instruct the rising generation of mining engineers. We learn that the opening of the College is fixed for October 7, and shall watch with pleasure the progress of the undertaking, bidding it heartily "God speed."

AT a meeting of the Council of University College, London, held on Saturday last, a scheme for the establishment of a Sharpey Physiological Scholarship in the College was adopted. It is expected that the annual value of the Scholarship will be about 100*l*.

SIR DOMINIC CORRIGAN, Bart., M.D., M.P., has been appointed Vice-Chancellor of the Queen's University in Ireland, in the room of the late Sir Maziere Brady, Bart.

ONLY one gentleman has this year obtained the degree of D.Sc. of the University of London, Mr. W. A. Tilden, in Chemistry.

M. H. SAINTE-CLAIR-DEVILLE, one of the most learned and popular members of the Institute, was a candidate at the recent French election on the moderate Republican ticket. M. Broca, the celebrated anthropologist, who will soon be a member of the Institute, was also a candidate on the same ticket, as was also M. Wolowsky, a member of the Académie des Sciences Morales et Politiques. We learn that M. Wolowski has been returned at the head of the poll, and that MM. Scheurer-Restner and Laboulaye have also been elected for Paris. The Institute is fast becoming, not actually a political body, but a body more closely connected with politics than it was formerly. For some time past a *résumé* of the sittings of the Academy has been inserted regularly in the *Journal Officiel*, which is becoming every day more scientific in its character. The National Society of Men of Letters recently held its ordinary meeting, when it was proposed to erase from its list of members MM. Victor Hugo, Pyat, and Rochefort, who are being prosecuted for their deeds during the Commune. But the meeting rejected the motion.

THE *Revue des Cours Scientifiques* commences with July 1 a new series, with the new title *La Revue Scientifique*, under the old editorship of MM. Yung and Alglave. The first number of the new series contains a sketch of the labours of the late M. Claparède, translations of Profs. Huxley and Tyndall's addresses at the Liverpool meeting of the British Association, and some fresh notes by Prof. Van Beneden on Commensalism in the Animal Kingdom.

THE weekly journal, *L'Institut*, has just entered on the fortieth year of its existence.

HERR RÜMKER has communicated to the *Astronomische Nachrichten* the following ephemeris of a new comet discovered by Temple on the 14th ult.:

$0^h$ BERLIN MEAN TIME.

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AT the Anniversary Meeting of the Meteorological Society, held June 21, C. W. Walker, Esq., F.R.S., F.R.A.S., the president, in the chair, the following officers were elected:—President: Dr. John W. Tripe. Vice-Presidents: N. Beardmore, C. O. F. Cator, G. J. Symons, C. V. Walker, F.R.S. Treasurer: H. Perigal. Trustees: Sir Antonio Brady, S. W. Silver. Secretaries: Chas. Brooke, F.R.S., Jas. Glaisher, F.R.S. Foreign Secretary: Lieut.-Col. Alexander Strange, F.R.S.

Council: Arthur Brewin, Geo. Dines, F. W. Doggett, H. S. Eaton, Fred. Gaster, Rev. C. H. Griffith, Dr. R. J. Mann, W. W. Saunders, F.R.S., R. H. Scott, F.R.S., Thos. Sopwith, F.R.S., S. C. Whitbread, F.R.S., E. O. W. Whitehouse.

THE annual distribution of prizes at Owens College, Manchester, was held on June 23, when the chairman, Mr. A. Neild, stated that the report of the Principal exhibits a very satisfactory amount of work done during the session, and a considerable increase in the number of students. The quality of the work has also not in any degree fallen off. The session was opened by a lecture from Dr. Balfour Stewart, on his appointment to the chair of Natural Philosophy; but his work was interrupted soon afterwards by a terrible accident which occurred to him at Harrow. He was glad, however, to be enabled to say that Prof. Stewart had so far recovered that he would be able to resume work at the commencement of the next session. A great deal had been done of late in the North of England in the way of increasing the teaching of Natural Science. He thought that so far as the means at their disposal enabled them, the managers of Owens College had made the institution a great school of Natural Science. At the same time, he hoped they should never fall into the opposite error of neglecting classical study and all that belonged to it. There is every reason to anticipate that Owens College will enter on its new premises in the course of session 1872-73. In addition to the prizes in the various classes, the following scholarships, &c., were then awarded:—Shuttleworth Scholarship (Political Economy), value 50*l.* per annum, tenable for two years: James Parkinson; Dalton Chemical Scholarship, value 50*l.* per annum, tenable for two years: William Robert Jekyll; Dalton Senior Mathematical Scholarship, value 25*l.* per annum, tenable for one year: John Henry Poynting; Dalton Junior Mathematical Scholarship, value 25*l.* per annum, tenable for one year: Arthur Walton Fuller; Ashbury Scholarship (Engineering), value 25*l.* per annum, tenable for two years: Edgar S. Cobbold; Dalton Natural History Prize, value 15*l.*: Charles Henry Wade; Engineering Essay Prize, Books of the value of 5*l.*: John Alfred Griffiths.

THE *Glasgow Star* says that the trustees of Anderson's University have been informed by their president—Mr. Young, of Kelly—that a gentleman had of his own accord made an offer of 2,000*l.* towards founding a chair of Applied Physics. Among other things, the trustees agreed to record their hearty approval of the scheme for establishing a College of Technology in Glasgow.

THE conditions necessary to the completion of the Brown Trust by the University of London have now been fulfilled. The University has been placed in possession of an excellent site, and abundant funds are forthcoming to carry out the objects of the Trust by founding an institution for the reception and treatment of sick and diseased domestic animals, which will afford invaluable opportunities for the advance of our knowledge of their diseases and their relation to those of man—a subject, says the *British Medical Journal*, of scientific and national importance.

DR T. BUCHANAN WHITE, President of the Perthshire Society of Natural Science, and editor of the *Scottish Naturalist*, publishes a first contribution towards a knowledge of the animals inhabiting Perthshire, in the form of a list of the Lepidoptera of the county.

THE last number of Petermann's "Mittheilungen" contains an admirable map of the Diamond Fields of Natal and the Orange River.

THE York School Natural History Society has issued its thirty-seventh Annual Report, from which we are glad to learn that the members show no lack of interest in the various branches of Natural Science. The collections exhibited at the close of last year were as follows: in Botany, three, varying from 257 to 96 species; of Lepidoptera, five, ranging from 105 to 72 species; of Coleoptera, six, containing from 212 to 64 species, and one illustrative of Insects generally. Three Natural History diaries were exhibited, and three recording astronomical observations, the latter especially being the result of much care and labour, and the observatory has been very diligently used. We trust that the society will long continue to exercise its useful influence, and that the members will profit in after life by the opportunities which have been afforded them.

THE Eastbourne Natural History Society, although only established in 1867, has already done a useful work in compiling for a new "Guide" to the neighbourhood a provisional list of the Fauna and Flora of the district. The space at their disposal being necessarily limited, it was impossible to give more than an enumeration of the animals and plants of the neighbourhood: but the attempt is worthy of note as a step in the right direction. The mammalia and reptilia are arranged according to Bell, the birds and fishes after Yarrell; for marine mollusca "Forbes' Handbook," and for land and freshwater species "Jeffreys' British Conchology," are followed; while the butterflies and moths follow respectively Morris and Newman. The flowering plants and ferns are arranged according to the "London Catalogue;" the mosses and algæ after Wilson and Gray; there is also a list of fungi. The secretary of the society, the Rev. A. K. Cherrill, will be glad to receive any additional information. A valuable museum, chiefly geological, has been bequeathed to the town, and is to be placed under the care of the society, so soon as a suitable building can be provided for its accommodation.

AT a meeting of the Royal Bavarian Academy of Sciences, on the 28th of March, Baron Liebig spoke thus of the future relations between Germany and France:—"The Academy seizes this moment to declare openly that there exists no national hatred between the German and Latin races. The peculiar character of the Germans, their knowledge of languages, their acquaintance with foreign people, the past and present state of their civilisation, all tend to make them just toward other peoples, even at the risk of often becoming unjust toward their own; and thus it is that we recognise how much we owe to the great philosophers, mathematicians, and naturalists of France, who have been in so many departments our masters and our models. I went forty-eight years ago to Paris to study chemistry; a fortuitous circumstance drew upon me the attention of Alexander Von Humboldt, and a single word of recommendation from him caused M. Gay-Lussac, one of the greatest chemists and physicists of his time, to make to me, a young man of twenty, the proposal to continue and finish with his co-operation an analysis which I had commenced; he introduced me as a pupil into his laboratory; my career was fixed after that. Never shall I forget the kindness with which Arago and Thenard received the German student; and how many compatriots, physicians and others, could I not name who, like myself, gratefully remember the efficacious assistance afforded to them by French men of science in finishing their studies. An ardent sympathy for all that is noble and grand, as well as a disinterested hospitality, form some of the most noble traits of the French character."

THE *British Medical Journal* states that a person named G. M. Raufer puffs and sells for three shillings, under the name of "lemonade for strengthening the memory," a fluid mixture of about 30 grammes, containing 15 parts of phosphoric acid, 15 of glycerine, and 70 of water. This is sold in Vienna.

IT is stated in the *British Medical Journal* that the Emperor of All the Russias has intimated to the University of Helsingfors, through the Senate of Finland, his willingness to permit women to attend the medical lectures at that University, in furtherance of the expressed wishes of His Majesty's Finnish subjects.

AT the last Calcutta University Convocation the novelty was the presence of eight native Brahme ladies.

GOLD is reported in New Caledonia, near the Scot River.

SUCH is the ease with which scientific intelligence is now propagated that the experiments of Dr. Fayrer, in India, on snake-bites, have attracted attention in the *Panama Herald*. It is there stated that an efficacious native Indian remedy for snake-bites has long been employed in many parts of the interior, and more successfully than ammonia, codron, cuaco, and other substances. The composition referred to is made by adding to a bottle of alcohol, as strong as can be got, and of at least 35°, the contents of the gall-bladders of every poisonous snake that can be got at. The dose is a thimble-full internally and the like externally.

THE *Mechanics' Magazine* for June 9 and 16 contains a full and interesting report of the recent *conversazione* of the Institution of Civil Engineers.

## *FIRST REPORT OF THE SCHEME OF EDUCATION COMMITTEE OF THE LONDON SCHOOL BOARD*

THE questions referred to us appear to fall under two chief divisions :—(1) The nature of the schools which it is desirable that the School Board should provide ; and (2) the methods of instruction which should be adopted in such schools ; and we shall therefore group our recommendations under these two heads.

Before proceeding to state these recommendations, it is important to observe that they need not be considered to apply, unreservedly, to those already existing schools which may now, or hereafter, be taken over by the Board.

The nature of the schools to be provided by the School Board will, as a general rule, be determined by the conditions under which grants of public money are made to schools by the Education Department.

Under the new code grants are made to public elementary schools of two kinds—those in which the instruction is given in the daytime, and those in which it is given in the evening. Under the regulations of the Science and Art Department, payments are made to teachers of science and art classes upon the results of examinations passed by the scholars. It will be desirable, in the first place, to deal with the two kinds of schools, viz., public elementary day schools, and public elementary evening schools, which it is the immediate duty of the Board to provide ; and, subsequently, to consider the classes of the Science and Art Department, in relation to these schools.

### I. PUBLIC ELEMENTARY DAY SCHOOLS

Public elementary day schools are conveniently classified into infant schools, for children below seven years of age ; junior schools, for children seven and ten years of age ; and senior schools, for older children.

Some of the recommendations we have to make are general, or hold good for all three classes of schools, while others apply only to one or two of them.

#### *General Recommendations*

*a.* MIXED OR SEPARATE SCHOOLS.—By mixed schools, we understand schools in which male and female children are taught in the same classes ; by separate schools, those in which boys and girls are taught in separate rooms.

It is universally agreed that infant schools may be mixed, not only without detriment, but with positive advantage to the children.

We therefore recommend that infant schools be mixed.

With respect to junior schools, so much depends upon the previous training of the children, and upon local circumstances, that we do not think it advisable to lay down any general rule regarding them.

On the other hand, while evidence has been brought before us tending to show that, under certain conditions, senior schools may be mixed, we are decidedly of opinion, and we recommend, that the senior schools provided by the School Board of London should be separate.

*b.* LARGE OR SMALL SCHOOLS.—A Board school should contain, under one management, an infant school or schools, a junior school, a senior boys' school, and a senior girls' school.

Large junior and senior schools of 500 children and upwards, can be worked with much greater economy and efficiency than small schools ; and we have no hesitation in recommending that large schools be established wherever it is practicable to do so. But we are of opinion that the number of children in average attendance in any infant school, or infant department of a school, under one principal teacher, should not exceed 250 to 300.

*c.* THE PROPORTION OF TEACHERS TO SCHOLARS.—Efficient and economical teaching, other things being alike, depends upon two conditions : the first, the regularity of the attendance of the scholars ; the second, the due proportion of the teaching power to the number of the scholars.

We are of opinion that the minimum number of teachers for a junior or senior school of 500 children should be 16—namely, 1 principal teacher, 4 assistant certificated teachers, and 11 pupil teachers ; and that the teaching staff should be increased by 1 assistant certificated teacher and 3 pupil teachers for every additional 120 children.

*d.* THE EMPLOYMENT OF FEMALE TEACHERS.—In infant and girls' schools, as a general rule, we recommend the employment of female teachers only ; and we are of opinion that, in many cases, women may advantageously take charge of mixed junior schools. We do not think it advisable that female teachers should be employed in senior boys' schools.

*e.* HOURS OF INSTRUCTION.—We recommend that the period during which the children are under actual instruction in school should be five hours daily for five days in the week.

We recommend that arrangements should be made by which, during the time of religious teaching, any children withdrawn from such teaching shall receive separate instruction in secular subjects.

*f.* CORPORAL PUNISHMENT.—While we consider that the frequent use of corporal punishment is a mark of incompetency on the part of the teacher, we by no means deny the necessity of the occasional and exceptional employment of such punishment. But we recommend that every occurrence of corporal punishment be formally recorded in a book kept for the purpose ; that the pupil teachers be absolutely prohibited from inflicting such punishment ; and that the head teacher be held directly responsible for every punishment of the kind.

*g.* MUSIC AND DRILL.—On the 1st of February, 1871, the Board resolved—"That it is highly desirable that means should be provided for physical training, exercise, and drill, in public elementary schools established under the authority of this Board :" and on the 22nd of March the Board passed another resolution—"That the art and practice of singing be taught, as far as may be possible, in the Board schools, as a branch of elementary education."

The new code of the Education Department encourages drill, by providing that attendance at drill, under a competent instructor, "for not more than two hours a week and twenty weeks in the year," may be counted as school attendance ; and although it does not make the teaching of vocal music compulsory, it inflicts a fine at the rate of one shilling per scholar in average attendance upon all schools in which vocal music is not taught.

We recommend that music and drill be taught in every school during the period devoted to actual instruction.

*h.* MORAL AND RELIGIOUS INSTRUCTION.—On the 8th March, 1871, the Board resolved—That in the schools provided by the Board, the Bible shall be read, and there shall be given such explanations and such instruction therefrom in the principles of morality and religion, as are suited to the capacities of children ; provided always—

1. That in such explanations and instruction the provisions of the Act, in Sections VII. and XIV., be strictly observed, both in letter and spirit, and that no attempts be made in any such schools to attach children to any particular denomination.

2. That in regard of any particular school, the Board shall consider and determine upon any application by managers, parents, or ratepayers of the district, who may show special cause for exception of the school from the operation of this resolution, in whole or in part.

We recommend, therefore, that provision should be made for giving effect to this resolution.

#### 2. *Particular Recommendations*

INFANT SCHOOLS.—We cannot too strongly insist upon the importance of schools for children under seven years of age. In a properly conducted infant school, children are not only withdrawn from evil and corrupting influences, and disciplined in habits of order, attention and cleanliness, but they receive such an amount of positive instruction as greatly facilitates their progress in the more advanced schools. There appears to be no doubt that by regular attendance in an infant school, provided with efficient teachers, a large proportion of ordinary children of six or seven years of age may be enabled to pass in the first standard of the new code.

The inducements which lead parents to keep older children from school are almost wholly absent in the cases of those under seven years of age, who are able to earn little or nothing, and are of no use in the house. And the fact that the younger children are taken care of in an infant school will often remove one of the chief difficulties in the way of securing the regular attendance of the elder girls of a family at the junior and senior schools.

The subjects in which we recommend that instruction should be given in infant schools are :—

*a.* Morality and religion.

*b.* Reading, writing, and arithmetic.

*c.* Object lessons of a simple character, with some such exercise of the hands and eyes as is given in the "Kinder-Garten" system.

In addition, the general recommendations respecting music and drill apply to infant schools, in which singing and physical exercises, adapted to the tender years of the children, are of paramount importance.

JUNIOR AND SENIOR SCHOOLS.—We recommend that certain kinds of instruction shall form an essential part of the teaching of every elementary school; while others may or may not be added to them, at the discretion of the managers of individual schools, or by the special direction of the Board.

A. ESSENTIAL SUBJECTS.

*a.* Morality and religion.

*b.* Reading, writing, and arithmetic: English grammar in senior schools; with mensuration in senior boys' schools.

*c.* Systematised object lessons, embracing in the six school years a course of elementary instruction in physical science, and serving as an introduction to the science examinations which are conducted by the Science and Art Department.

*d.* The History of Britain.

*e.* Elementary geography.

*f.* Elementary social economy.

*g.* Elementary drawing, leading up to the examinations in mechanical drawing, and to the art teaching of the Science and Art Department.

*h.* In girls' schools, plain needlework and cutting out.

B. DISCRETIONARY SUBJECTS, which may be taught to advanced scholars.

*a.* Algebra and geometry.

*b.* Latin or a modern language.

II.—PUBLIC ELEMENTARY EVENING SCHOOLS

Evening Schools are of great importance, partly as a means of providing elementary education for those who, for various reasons, fail to obtain sufficient instruction in elementary day schools; and, partly, because it is easy to connect with such schools special classes in which a higher kind of instruction than that contemplated by the Sixth Standard can be given to the more intelligent and older scholars. In this manner the advantages of further instruction may be secured by those scholars who are unable or unwilling to go into secondary schools, but who are both able and willing to pay for instruction of a more advanced kind than that given in primary schools.

We recommend that the course of instruction in these evening schools shall be of the same general character as that already recommended for the junior and senior elementary day schools. Elementary evening schools should, in all cases, be *separate*, and the General Recommendation (*a*) respecting moral and religious instruction applies to them. In all other respects we recommend that the managers should be left free to adapt the instruction given in the schools to local requirements.

According to the New Code, the scholars in evening schools must be not under 12, nor above 18, years of age, and no attendance is reckoned unless the scholar has been under instruction in secular subjects for one hour and a half.

III.—SCIENCE AND ART CLASSES

Numerous classes for instruction in Science and Art are already in existence; their current expenses, and the remuneration of teachers, being defrayed, in part, by the grants paid upon the result of the annual examinations, and, in part, by pupils' fees.

These classes are usually held in the evening, and are frequently connected with evening schools.

The Science and Art Department comes into relation with these classes, and with the examination of the scholars taught in them, through the agency of Committees who voluntarily charge themselves with the responsibility of seeing that the regulations of the Department are carried out. The establishment of Science and Art Classes in connection with Public Elementary Evening Schools, therefore, would not involve the Board either in trouble or expense.

We recommend that the formation of such classes be encouraged and facilitated.

The Elementary Education Act does not confer upon a School Board the power of providing secondary schools, and it is silent as to the mode by which a connection may be established between the elementary and the secondary schools of the country. But it is of such importance to the efficiency of popular education that means should be provided by which scholars of more than average merit shall be enabled to pass from elementary into secondary schools that we feel it our duty to offer some suggestions upon the subject

The practical difficulty in the way of the passage of boys and girls from an elementary into a secondary school, is the cost of their maintenance; and the best way of meeting that difficulty appears to be to establish exhibitions equivalent to the earnings of boys and girls of from 13 to 16 years of age, tenable for the period during which they remain under instruction in the secondary schools. The funds out of which such exhibitions may be created already exist, and the machinery for distributing them has been provided by the Legislature in the Endowed Schools Act.

The Endowed Schools Commissioners have fully recognised the claims of scholars in public elementary schools to share the advantages of the endowed schools. We recommend, therefore, that the Board enter into official communication with the Endowed Schools Commissioners, and agree with them upon some scheme by which the children in public elementary schools shall be enabled to obtain their rightful share of the benefits of those endowments with which the Commissioners are empowered to deal.

T. H. HUXLEY (*Chairman*).
JOSEPH ANGUS
ALFRED BARRY
EDM. HAY CURRIE
EMILY DAVIES
LAWRENCE
BENJN. LUCRAFT
J. MACGREGOR
CHARLES REED
JAMES H. RIGG
WILLIAM ROGERS
EDW. J. TABRUM

JOHN G. CROMWELL—

Except that, looking to the three concluding clauses of the Report and to the sixth recommendation founded thereon, I feel unable to join in recommending that "Latin or a modern language may be taught to advanced scholars" in schools provided by the Board.

J. ALLANSON PICTON—

Except the application of General Recommendation (*h*) to public elementary evening schools.

SANDON—

Except that I object to the teaching of Latin or a modern language in primary public elementary schools; and that I also object to pronouncing any opinion in this Report upon the appropriation of existing endowments to the public elementary schools of London, as I do not consider that it is competent to a committee appointed by the School Board "to consider and report upon the scheme of education to be adopted in the public elementary schools" to consider, or make recommendations upon, this important subject.

## *MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY*

(*Concluded from page* 172)

FRANCE, without any special endemic character, unites within her limits portions of several biological regions, thus requiring from her naturalists the study of all the European Floras and

Faunas in order rightly to understand her own. The greater part of her surface constitutes the western extremity of that great Russo-European tract I have above commented upon, its flora, and probably also its fauna, here blending with the West European type, which spreads more or less over it from the Iberian peninsula. To the south-east she has an end of the Swiss Alps, connected to a certain degree with the Pyrenees to the south-west by the chain of the Cevennes, but at an elevation too low, and which has probably always been too low, for the interchange of the truly alpine forms of those two lofty ranges. South of the Cevennes she includes a portion of the great Mediterranean region; and the marine productions of her coasts are those of three different aquatic regions—the North Sea, the Atlantic, and the Mediterranean. The few endemic or local races she may possess appear to be on those southern declivities which bound the Mediterranean region; and if the volcanic elevations of Central France have a special interest, it is more from the absence of many species common at similar altitudes in the mountains to the east or to the south-west, than from the presence of peculiar races not of the lowest grades, with the exception, perhaps, of a very few species now rare, and which may prove to be the lingering remains of expiring races.

With so many natural advantages, French science, represented during the last two centuries by as great, if not a greater, number of eminent men than any other country, has long felt the necessity of a thorough investigation of the biological productions of her territory. The French Floras, both general and local, are now numerous, and some of them excellent. The geographical distribution of plants in France has also been the subject of various essays as well as separate works. It is only to be regretted that in the Floras themselves the instructive practice of indicating under each species its extra-Gallican distribution has not yet been adopted. In Zoology no general fauna has been attempted, since De Blainville's, which was never completed, and none is believed to be even in contemplation; but I have a long list of partial Faunas and Memoirs on the animals of various classes of several French departments; and Rey and Mulsant are publishing, in the Transactions of two Lyons Societies, detailed monographs of all French Coleoptera.

The progress of French naturalists in Biology in general up to 1867 has been fully detailed as to Zoology by Milne-Edwards, in his "Rapport sur les Progrès de la Zoologie en France;" and as to Systematic Botany by Ad. Brongniart in his "Rapport sur les Progrès de la Botanique Phytographique." The recent progress as to both branches, as well as in regard to other natural sciences, has also been reviewed by M. Emile Blanchard in his annual addresses to the meetings of the delegates of French scientific societies, held every April at the Sorbonne from 1865 to 1870. The Société Botanique de France had also up to that time been active, and the publication of its proceedings brought down nearly to the latest meetings. I am compelled, however, for want of time, to defer some details I had contemplated relating to the recent labours of French biologists; but I cannot refrain from inserting the following note on a work mentioned only, but not analysed, in the last volume of the "Zoological Record," obligingly communicated to me with other memoranda by Prof. Deshayes, whilst slowly recovering from a severe illness contracted during the German siege:—

"In Mollusca we have also to regret that we have no complete work embracing the whole of this important branch of the animal kingdom. It is true that we make use of numerous works published in England, amongst which several are excellent, such as those of Forbes and Hanley, Gwyn Jeffreys, &c. Nevertheless I have to point out to you an excellent work published in 1869 by M. Petit de la Saussaye. The author, a very able and scientific conchologist, is unfortunately just dead. He has had the advantage of preparing a general catalogue of Testaceous Mollusca of the European Seas, possessing in his own collection nearly the whole of the species inserted, and of having received direct from the authors named specimens of the species foreign to the French coasts. This work is divided into two parts. The first is devoted to the methodical and synonymical catalogue of the species amounting to 1,150. In the second part, these species are distributed geographically into seven zones, starting from the most northern and ending with the hot regions of the Mediterranean. These zones are thus distinguished:—1, the Polar zone; 2, the Boreal zone; 3, the British zone; 4, the Celtic zone; 5, the Lusitanian zone; 6, the Mediterranean zone; and 7, the Algerian zone. Some years since it would have been impossible for M. Petit to have established the fifth zone, for that nothing, literally nothing, was known of the malacological fauna of Spain. Its seas were until 1867 less known than those of New Holland or California. It was only in that year that Hidalgo published a well drawn up synonymic catalogue in Crosse and Fischer's "Journal de Conchyliologie."

The British Isles have less even than France of an endemic character in respect of biology. They form, as it were, an outlying portion of regions already mentioned, the greater part, as in the case of France, belonging to the extreme end of the great Russo-European tract. Like France, also, they partake, although in a reduced degree, of that Western type which extends upwards from the Iberian Peninsula. They are, however, completely severed from the Mediterranean as from the Alpine regions; their mountain vegetation, and, as far as I can learn, their mountain zoology, is Scandinavian; and if it shows any connection with southern ranges, it is rather with the Pyrenees than with the Alps. The chief distinctive character of Britain is derived from her insular position, which acts as a check upon the passive immigration of races, and is one cause of the comparative poverty of her Fauna and Flora; the isolation, on the other hand, may not be ancient enough or complete enough for the production and preservation of endemic forms. As far as we know, there is not in phænogamic botany, nor in any of the orders of animals in which the question has been sufficiently considered, a single endemic British race of a grade high enough to be qualified as a species in the Linnæan sense. How far that may be the case with the lower cryptogams cannot at present be determined; there is still much difficulty in establishing species upon natural affinities, and in some Lichens and Fungi, for instance, much confusion between phases of individual life and real genera and species remains to be cleared up. The study of our neighbours' Faunas and Floras is therefore necessary to make us fully acquainted with the animals and plants we have, and useful in showing us what we have not, but should have had, were it not for causes which require investigation; such, for instance, as plants like *Salvia pratensis*, a common European species to be met with in abundance the moment we cross the Channel, but either absent from or confined to single localities in England.

There is no country, however, in which the native Flora and Fauna has been so long and so steadily the subject of close investigation as our own, nor where it continues to be worked out in detail by so numerous a staff of observers. To the Floras we possess, a valuable addition has been made within the last twelvemonth in J. D Hooker's "Student's Flora of the British Isles;" the best we have for the purposes of the teacher, and in which the careful notation of the general distribution of each species is a great improvement on our older standard classbooks. H. C. Watson's recently completed "Compendium of the Cybele Britannica" treats of the geographical relations of our plants with that accuracy of detail which characterises all his works. In Zoology, although we may not have compact synoptical Faunas corresponding with our Floras in all branches of the animal kingdom, the series of works on British Vertebrata published by Van Voorst are a better and more complete account of our indigenous races than any Continental State can boast of; and I observe with much pleasure that in the new edition announced of the "British Birds," Mr. Newton proposes specially to follow out the determination of their geographical range, upon which Mr. Yarrell had bestowed so much pains. With regard to our Mollusca, we have been very fortunate. Forbes and Hanley's costly work, published by the Ray Society, has been followed by Gwyn Jeffreys's "British Conchology," the great merits of which as a Malacological Fauna of Britain have been fully acknowledged abroad as well as at home. The present geographical as well as the fossil range of the species is specially attended to, and the only thing missed is perhaps a general synoptical view of the characters of the classes, families, and genera into which the species are distributed. The Ray Society series comprises also several most valuable works on the lower orders of British animals; but the entomological fauna of our country, especially in relation to the insects of the adjoining Continent, notwithstanding the numerous able naturalists who devote themselves to its study, appears to be somewhat in arrear. In answer to my query as to works where our Insects are compared with those of other countries, I received from our Secretary, Mr. Stainton, the following reply:—"The questions you have put to me with reference to our entomological literature are very important; they, however, painfully call my attention to the necessarily unsatisfactory nature of my replies. Wollaston's 'Coleoptera Hesperidum'* is the only separate

* Referred to in my Address of 1869.

work to which I can direct your attention as giving the fauna of a particular district, with the geographical range of such of the species as are likewise found elsewhere. R. M'Lachlan, who in 1865 had published (Trans. Ent. Soc. ser. 3, v.) a Monograph of the British Caddis-flies, gave in 1868 (Trans. Ent. Soc. for 1868) a Monograph of the British Neuroptera Planipenna, but little is there said of the European range of our species. In 1867 (Entom. Monthly Mag. iii.) Mr. M'Lachlan, who is one of our most philosophical writers, gave a Monograph of the British Psocidæ, and he there says with reference even to their distribution in our own country, 'As a rule, I have not mentioned special localities; these insects have been so little collected that an enumeration here of known or recorded localities would probably appear ridiculous in a few years.' The Rev. T. A. Marshall has given (Entom. Monthly Mag. i. to iii.) an essay towards a knowledge of the British Homoptera, in which occasionally allusion is made to the European distribution of our British species.

"The position of the Insect-fauna of Britain may be thus stated: the late J. F. Stephens commenced in 1827 a systematic descriptive work of all the orders of British Insects as 'Illustrations of British Entomology;' it ceased to appear after 1835, until a supplementary volume came out in 1846. The Lepidoptera, Coleoptera, Orthoptera, Neuroptera were wholly, the Hymenoptera partly, done, the Hemiptera and Diptera altogether left out. In 1839 Mr. Stephens published, in a more compendious form, a 'Manual of British Beetles.' In 1849 an attempt was made to supply the gaps in the British Entomology left by Stephens, and a scheme of a series of volumes called 'Insecta Britannica' was elaborated, in which Mr. F. Walker was to undertake the Diptera, Mr. W. S. Dallas the Hemiptera, and great progress having been made in our knowledge of the smaller moths since 1835, I undertook to write a volume on the Tineina. This scheme was so far carried out, that three volumes on the British Diptera by Mr. F. Walker (assisted by the late A. H. Haliday) appeared in 1851, 1852, and 1856, and my volume on the British Tineina in 1854. In 1859 another great group of the smaller moths was described by S. J. Wilkinson in a volume entitled 'The British Tortrices.' The British Hemiptera, not having been done by Mr. Dallas, were undertaken by Messrs. Douglas and Scott for the Ray Society; and in 1865 a 4to volume was issued, containing the Hemiptera, Heteroptera, leaving the Homoptera for a second volume, still in progress. Even in this elaborate work little or nothing is said of the geographical distribution out of Britain of our British species. The same will apply to the late J. F. Dawson's 'Geodephaga Britannica,' published in 1854; to Westwood's "Butterflies of Great Britain,' published in 1855; and to E. Newman's 'Illustrated Natural History of British Moths,' published in 1869.

"I believe I do not at all exaggerate if I say that for many years Entomology was pursued in this country with an insularity and a narrow-mindedness of which a botanist can scarcely form a conception. The system of only collecting British Insects was pursued to such an extent, that it was almost a crime to have a non-British insect in one's possession; if accidentally placed in one's cabinet it might depreciate the value of the entire collection, for Mr. Samuel Stevens can assure you that the value of the specimens depends very much upon their being indubitably and unmistakeably British. A specimen caught in Kent which would fetch 2*l*. would not be worth 2*s*. if caught in Normandy. I satirised this practice several years since in the 'Entomologists' Weekly Intelligence' (vol. v. and 1858, articles 'Jeddo' and 'Insularity'), but it is yet far from extinct"

Perfectly concurring in Mr. Stainton's observations in the last paragraph, I would however add that there are purposes for which a local or geological collection distinct from the general one may be of great use, and such a collection would be much impaired by the introduction of stray foreign specimens. In a local museum, a separate room devoted exclusively to the productions of the locality is very instructive with reference to the history of that locality, and I have seen several such spoiled by the admission of exotic specimens, giving the visitor false impressions, which it takes time to remove. But it is never from such an exclusive collection that the fauna or flora of the district can be satisfactorily worked out, or that any branch of Zoology or Botany can be successfully taught.

Mr. Stainton adds, "It has been suggested to me that those who have critically studied the distinctions between closely allied species have rarely the time to work out in addition their geographical range, and that those who might work up the latter subject might fail in their good intentions for want of a proper knowledge of species." Upon this I would observe that, in the due appreciation of a species of its limits and connections, its geographical range and the various forms it assumes in different parts of its area are an essential element; and it appears to me that the neglect of this and other general characters is one reason why many able naturalists, who have devoted their lives to the critical distinction of races of the lowest grades unduly raised to the rank of species, have really contributed so little to any science but that of sorting and naming collections. On the other hand, the study of geographical range without a proper knowledge of species is little more than pure speculation. Division of labour carried too far tends to narrow the mind, and rather to delay than advance the healthy progress of science.

Mr. Stainton informs me that "there has just appeared a monograph of the Ephemeridæ, by the Rev. A. E Eaton (Trans. Entom. Soc. 1871), treating of those insects throughout the globe; and when any species are noticed which occur in this country, their entire geographical range is noticed. It is altogether a valuable paper, on account of the thoroughness with which it seems to be done."

Since I last noticed our biological publications two valuable and beautifully illustrated but costly Ornithological works, Sclater and Salvin's "Exotic Ornithology," and Sharpe's "Monograph of the Alcedinidæ," have been completed, and various Memoirs by Flower, Mivart, Parker, and others, have considerably advanced our knowledge of the comparative anatomy of various groups of Mammalia. In our own country also, as well as on the Continent, the biology of various distant lands has continued to be worked out in Memoirs or independent publications, which I had contemplated noticing in succession; but time obliges me now to stop, and defer to a future occasion the compilation of the notes I had collected on North American, Australian, and other Monographs, Faunas, and Floras.

## *SCIENTIFIC SERIALS*

THE *Geological Magazine* for June (No. 84) commences with some notes on Crinoids by Mr. John Rofe, relating rather to the zoological than to the geological aspects of that class of animals. Mr. Rofe describes some experiments made on recent Crinoids by treating them with solution of potash or muriatic acid, from which he arrives at the conclusion that their hard parts are invested by a membrane giving them a certain degree of flexibility, a general position which few naturalists will be inclined to dispute. But the details of structure described by Mr. Rofe will be found of much interest. In his concluding remarks he endeavours to show an approximation between the Crinoids and the Tunicata, which, to say the least of it, is very doubtful.—Mr. S. Allport publishes a note on the microscopic structure and composition of a rock from the "Wolf Rock" off the Land's End, which he identifies with phonolite, and justly protests against the system which gives different names to rocks identical in mineral composition because they happen to be of different geological ages.—Mr. D. Mackintosh describes the drifts of the west and south borders of the Lake district, with especial reference to their great granitic dispersions which he believes have taken place; and Messrs. C. and A. Bell discuss the divisions of the English Crags as indicated by their invertebrate fauna. They propose as the result of their investigations, to divide the Crag into Upper, Middle, and Lower; the Upper including the Norwich, and the upper part of the so-called Red Crag; the Middle, the remainder of the Red Crag; and the Lower, the Coralline Crag. The last paper consists of a comparison of the metamorphic rocks of Scotland and Galway, by Mr. G. H. Kinahan. The first and last of these papers are illustrated with plates.

The second part of Tome xliii. of the *Bulletin de la Société Impériales des Naturalistes de Moscou*, completing the first half volume for 1870, is the last portion of this publication that has yet reached this country. It contains the continuation of M. Ferd. von Herder's notice of the monopetalous plants collected by G. Radde and others (*Plantæ Raddeanæ Monopetalæ*) in Eastern Siberia, the Amurland, Kamtschatka, and Russian America, and includes references to numerous species of Compositæ.—M. N. Erschoff communicates a note upon the Lepidoptera of Western Siberia, containing a list of species from the town of Omsk. — A Russian paper on the Oligochætal Annelid, *Pelorycles inquilina*, by M. H. Zingera, will probably attract few English readers.—M. E. Regel publishes a portion of a second supplement to the enumeration of the plants collected by Sewerzow in 1857 in Central Asia. It includes the Ranunculaceæ, Berberideæ, Nymphæaceæ, Papaveraceæ, Fumariaceæ, and Cruciferæ. Several new species are described.—Another botanical paper is an abridged French translation of part of the Introduction to a Flora of Moscow, by M. N. Kauffmann, the translation being made by Mr. G. O. Clerc. The Flora, which is a Catalogue of the vascular plants of the Government of Moscow, will appear in future numbers.—We find in this number two entomological papers, both on Coleoptera, and one of them of great importance, namely, a Monograph of the Graphipteridæ by the Baron Chaudoir. The other paper is a continuation, by M. Victor Motschoulsky, of his apparently interminable enumeration of the new species of Coleoptera collected by him in his journeys. It includes descriptions of species of Melasomata, and is illustrated with two plates.—M. G. Schweizer describes an easy method of approximately finding the meridian line; and M. A. Trautschold gives a short notice of some cretaceous fossils from Ssaratof and Ssimbirsk.

*Palæontographica.—Beiträge zur Naturgeschichte der Vorwelt.* Herausgegeben von Dr. W. Dunker und Dr. K. A. Zittel. Band xx. Lief 1., 1871. In this part of the well-known and most valuable "Palæontographica," Prof. Geinitz commences a monograph of the fossils of the Lower Quader and Lower Pläner beds in the Saxon Elbe valley, which he regards as forming the lowest part of a great Quader-formation, including the Senonian, Turonian, and Cenomanian stages of the French geologists. His Lower Quader is equivalent to the Upper Greensand of English geologists. It is well known that sponges are among the most abundant and striking fossils of our Upper Greensand, and the corresponding beds in the valley of the Elbe seem to be equally rich in remains of this lowest class of animals. With the exception of a summary of the geology of the district, the whole of the present part of Prof. Geinitz's work is occupied by descriptions of sponges, the species of which are beautifully figured in the accompanying plates. Laying the reproaches of Oscar Schmidt to heart, Prof. Geinitz endeavours to arrange his fossil forms in accordance with the system of that author, although, as he justly remarks, it is impossible in the study of fossil sponges to have recourse to those minute characters derived from the spiculæ, which form the basis of recent attempts to classify the recent forms. He notices in all twenty-eight species, of which six appear to be new.

## SOCIETIES AND ACADEMIES

### London

Royal Society, June 15, "On a Law in Chemical Dynamics." By John Hall Gladstone, F.R.S., and Alfred Tribe.

It is well known that one metal has the power of decomposing the salts of certain other metals, and that the chemical change will proceed until the more powerful metal has entirely taken the place of the other. The authors have investigated what takes place during the process.

The experiments were generally performed as follows:—72 cubic centimetres of an aqueous solution of the salt of known strength, and at 12° Centigrade, were placed in a tall glass; a perfectly clean plate of metal of 3,230 square millimetres was weighed and placed vertically in this solution without reaching either to the top or bottom; the action was allowed to proceed quietly for ten minutes, when the plate was removed, and the deposited metal was washed off. The loss of weight gave the amount of metal dissolved, and represented the chemical action.

The most complete series of results was with copper and nitrate of silver.

In the earlier terms of this series, *twice the percentage of silver-salt gives three times the chemical action.* The close agreement of the observed numbers with those calculated on this supposition continues as far as the 9th term. The law then breaks down, and after about 7 per cent. the increased action is almost in direct ratio with the increased strength.

The position of the plate in the solution was found to make no difference to this 2–3 law.

Similar series of experiments were made with zinc and chloride of copper, zinc and sulphate of copper, zinc and nitrate of lead, iron and sulphate of copper, and other combinations; and in every instance where the solution was weak and the action simple, the law of three times the chemical change for twice the strength was found to hold good.

It was proved that the breaking down of the law at about 3·5 per cent. of salt in solution was irrespective of the quantity of the liquid, or of the time for which the plate was exposed. With 72 cub. centims. of a 1·41 per cent. solution of nitrate of silver, the rate of action remained sensibly the same for as long as twenty-five minutes, notwithstanding the constant deposition of silver. This apparently paradoxical result is due to fresh relays of the original solution being brought up to the plate by the currents produced, and that period of time elapsing before any of the products of decomposition are brought back again in their circuit.

When it was perceived that within easily ascertainable limits the chemical action is the same for similar consecutive periods of time, experiments were made in far weaker solutions. It was only necessary to lengthen the time of exposure. It was thus found that the law of three times the chemical action for twice the strength of solution holds good through at least eleven terms of the powers of 2; in fact, from a solution that could dissolve one gramme of copper during the hour, to a solution that dissolved only 0·000001 gramme, a million times less.

The manner in which the silver is deposited on a copper plate was examined, and the currents produced were studied. At first a light blue current is perceived flowing upwards from the surface of the plate, presently a deep blue current pours downwards, and these two currents in opposite directions continue to form simultaneously. A similar phenomenon was observed in every case where a metallic salt attacked a plate of another metal. The downward current was found to be a solution of almost pure nitrate of copper, containing about three times as much $NO_3$ as the original silver solution, while the upward current was a diluted solution of the mixed nitrates. Moreover, the heavy current took its rise in the entangled mass of crystals right against the plate, while the light current flowed from the tops of the crystalline branches. It was evident that when the fresh silver was deposited on these branches, and the fresh copper taken up from the plate, there was not merely a transference of the nitric element from one combination to another, but an actual molecular movement of it towards the copper plate, producing an accumulation of nitrate of copper there, and a corresponding loss of salt in the liquid that is drawn within the influence of the branching crystals. Hence the opposite currents.

The amount of action in a circuit of two metals and a saline solution must have as one of its regulating conditions the conducting-power of that solution. It appeared by experiment that a strong solution of nitrate of silver offers less resistance than a weak one; and it was also found, on adding nitrate of potassium to the nitrate of silver, that its power of attacking the copper plate was increased; that the augmentation of the foreign salt increased the action still further; and that the 2-3 law holds good between two solutions in which both the silver and potassium salt are doubled, though it does not hold good if the quantity of foreign salt be kept constant. Similar results were obtained with mixed nitrates of silver and copper.

While these later experiments offer an explanation of the fact that a solution of double the strength produces more than double the chemical action, they do not explain why it should produce exactly three times the effect, or why the ratio should be the same in all substitutions of this nature hitherto applied. The simplicity and wide range of the 2-3 law seems to indicate that it is a very primary one in chemical dynamics.

"On Cyclides and Sphero-Quartics." By John Casey, LL.D.

Royal Institution of Great Britain, July 3.—Sir Henry Holland, Bart., M.D., F.R.S., president, in the chair. William Amhurst Tyssen Amhurst and Lawrence Trent Cave were elected members.

Royal Geographical Society, June 26.—Major-General Sir Henry C. Rawlinson, K.C.B., president, in the chair. The following new fellows were elected: Thomas Brassey, M.P.; T. B. Baker, C.B.; D. Chinery (Consul-General for Liberia); Commander C. D. Inglis, R.N.; William Charles Jackson; G. W. Kennion; Alfred Morrison, William G. Margetts, Colonel R. Maclagen, R.E.; Captain G. S. Nares, R.N.; and James Rickards. A letter was read from Sir Roderick Murchison, giving Dr. Kirk's views of Dr. Livingstone's position, as communicated in a recent letter from Zanzibar, dated the 30th April last. It appeared that no one at Zanzibar had been to Manime, the place where Livingstone was last heard of; but Dr. Kirk had ascertained that it was about a month's journey (200 or 300 miles) west of Lake Tanganyika, and was a thriving ivory-mart. Dr. Kirk expressed his hopes that, if Livingstone should have settled the problem of the outflow of Tanganyika, he would be satisfied, and leave all the rest of the work to future travellers, seeing that he has been out upwards of five years, and must sorely want rest. Abundant supplies were awaiting the great traveller's orders at Ujiji, on the shores of the lake.—Letters were read from Dr. J. D. Hooker to Sir Roderick Murchison, giving a description of his recent ascent of the Atlas Mountains, at two points south-west of the city of Marocco. On the first attempt, Dr. Hooker's party ascended to 12,000 feet; and on the second to the summit of a peak, further westward, 11,500 feet high. Storms of snow and hail were encountered near the crests; but the snow seemed to lie more compactly, and to a lower level (7,000 feet) further east. Constant humid and cold winds from the north are the cause of the low temperature, in consequence of which northern species of plants are found on the Atlas, to the exclusion of southern types.—A paper was read by Captain A. F. P. Harcourt on the districts of Kooloo, Lahoul, and Spiti, in Northern India; and a second one, by Major Sladen, on an exploration between the Irrawady and south-western China. Sir Donald MacLeod (late governor of the Punjaub), Sir Arthur Phayre, General Fytche (Commissioner of British Burmah), Colonel H. Yule, Mr. T. T. Cooper, Sir John Bowring, and others took part in the discussion, which followed the reading of the two papers.—The President announced that the Council had renewed, for the year 1872, the offer of geological prize medals to the chief public schools; and that the special subject for the year, both in the physical and the political divisions, would be South America. A proposition from the president for a vote of thanks to the Chancellor and Senate of the London University, for the use of their great hall, met with unanimous approval. The president stated that, although the ordinary meetings of the session had terminated, it was likely that a special sitting would be held to receive the Emperor of Brazil, an honorary member of the Society, should his Majesty accept, on his arrival, the invitation the Council had forwarded.

Anthropological Institute, June 19.—Sir John Lubbock, Bart., president, in the chair. Mr. G. Latimer was elected a local secretary for Puerto Rico and Logan; Dr. D. H. Russell was elected a local secretary for Bonny, west coast of Africa.—Prof. Busk exhibited two human jaws of remarkable thickness found in the superficial deposit of a cave near Sarawak, Borneo.—Mr. Josiah Harris exhibited from Macabi Island, off the coast of Peru, wood carvings, pottery, and cotton rags. The rags extended many hundred yards at an average thickness of five feet, and below a deposit of several feet of guano. The wood and pottery were discovered at a depth in the guano of from fifteen to forty-five feet.—Mr. G. M. Atkinson communicated some interesting facts connected with the discovery of a kitchen-midden in Cork harbour.—Mr. H. W. Flower exhibited a large jade implement from New Zealand.—A paper by Mr. A McDonald was then read, "On the Mode of Preserving the Dead among the Natives of Queensland."—Dr. Sinclair Holden contributed a paper "On Forms of Ancient Interments in Antrim;" and Mr. Hodden M. Westropp read a paper "On Analogies and Coincidences among Unconnected Nations."

DUBLIN

Royal Irish Academy, April 24.—The Rev. J. H. Jellett, president, in the chair. Mr. R. C. Tichbourne read a report on the molecular dissociation by heat of compounds in solution. The Rev. Dr. W. Reeves read a paper on the Irish tract by Onegus the Culdee, on the mothers of the saints of Ireland.

PARIS

Académie Française.—This is the most ancient of the French Academies, its special object being the publication of a Dictionary of the French language, which is thus officially protected against innovations. No word is considered classical without being duly registered in the Dictionary of the French Academy. Several editions have appeared successively, each of them containing many alterations. The next edition will soon be published, and is just now in active preparation. On June 29 the French Academy elected its Perpetual Secretary. All the votes were taken by M. Patin, a member of the Institute for the last twenty-eight years, and Professor of Greek Literature at the Sorbonne. The principal work of M. Patin is a study of the Greek tragedians, which is highly esteemed in France and abroad. The late Perpetual Secretary was the celebrated M. Villemain, a great friend of M. Guizot, and a former Minister of State in Louis Philippe's time. The election of M. Patin, although undisputed, was an event in the academical world, and many members left their residences, and even foreign lands, to vote for him. Amongst these learned travellers we must notice Father Gratry, of London, and the Marquis de Noailles, French Ambassador in London. MM. Guizot, Octave Feuillet, Nisard, &c., were present.

Académie des Inscriptions et Belles Lettres.—This Academy has also been engaged in filling the vacancies death had created in its ranks. M. Villemain was an ordinary member of this Academy. A scrutiny took place on the 30th ult., for the election of his successor. M. Charles Thurot was nominated by twenty-three votes against very few given to four other candidates. The Academy had also to vote for a successor to M. Alexandre, an inspector of the Academy, who was known merely by the publication of a Greek dictionary, which is the most useful in grammar schools. The succession to this office was more vigorously contested. M. de Rozière was elected only after a scrutiny, since a candidate must receive the actual majority of votes. A correspondent was also appointed. The successful candidate was M. Amari, an Italian learned antiquary of universal celebrity. All these nominations will be submitted to M. Thiers for approval, but it is a mere formality, and the assent of the Executive has never been refused for more than thirty years. M. Thiers himself is a member of the Institute, belonging to the Académie Française.

Academie des Sciences Morales.—The last sitting was occupied by a discussion raised by M. Egger on the degree of perception and intelligence in children. The question is to ascertain if infants are inferior or superior to ordinary animals in their mental condition. The reasoning of the learned member was grounded more on theoretical grounds than on actual observation of facts. None of the arguments offered were supposed to be conclusive, and the problem is left open for future investigations.

## BOOKS RECEIVED

ENGLISH.—Travels in Central America: Mrs. M. F. Squier (Trübner and Co.).—A Practical Treatise on the Manufacture of Soap: Dr. C. Morfit (Trübner and Co.).—Overland through Asia: T. W. Knox (Trübner and Co.)—Notes on the Food of Plants: C. C. Grundy (Simpkin and Marshall).—Transactions of the Woolhope Naturalists' Field Club for 1870.

FOREIGN.—(Through Williams and Norgate)—Bericht über die wissenschaftliche Leistungen im Gebiete der Entomologie wahrend der Jahre 1867-68: Brauer u. Gerstaecker.—Lehrbuch der allgemeinen Zoologie: G. Jaeger.—Die Molecular-gesetze dergestellt: Dr. C. Wittwer.

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ERRATA.—Vol. iv., p. 168, 2nd column, line 28 from bottom, for "the Rev. T. C. Maggs" read "Mr. T. C. Maggs": p. 174, 2nd column, line 31 from bottom, for "Nicturation" read "Micturition."

THURSDAY, JULY 13, 1871

## THE NEXT TOTAL SOLAR ECLIPSE

IT is infinitely to the credit of English men of science that they are at the present moment busily engaged in making arrangements for observations of the Total Solar Eclipse in December next, and it is extremely fortunate for the advance of Science that this rare phenomenon—rare, that is, so far as the chance of observing it with moderate facility goes—occurs again just as the knowledge gleaned by the recent expeditions is being garnered to serve as a starting point for future inquiry.

When we state that the eclipse will be visible as a total one in India, Ceylon, and Australia, it may at first be imagined that in this case the facilities are not so very great. This would be quite true if it were necessary to garrison all these stations with observers from England; but, as it happens, the Governments both of India and Victoria have under their orders government astronomers —Mr. Pogson at Madras, and Mr. Ellery at Melbourne; and all that is necessary is to forward to those stations instructions, so that the observers there may glean all the experience gained in the last eclipse, and instruments such as are required to advance our present knowledge. And here we may remark that our knowledge in solar matters has recently advanced so rapidly, that astronomers have, as it were, to use new weapons in each attack, as artillery gives place to small arms, and small arms to the bayonet, in less scientific warfare.

That India and Australia will thus be provided with everything that may be necessary will be evident when we state that the Astronomer Royal is superintending the adaptation of instruments already in his possession for use in the former country, while the President of the Royal Society has already communicated with the authorities in Australia, offering to aid in every way in the proposed observations—an offer which we doubt not will be accepted, and in both cases we may hope for results of the highest importance, if the local observers set to work with a will. As to the entire sympathy of both governments there can be no question, India was magnificently helpful to Janssen in 1868, and Australia has her spurs to win; and there are good men in plenty, in both places, in whom the Governments may place their fullest confidence.

There remains, then, Ceylon. Both the Royal and Royal Astronomical Societies have determined, if the Government will help, to send out a small party of observers from England to garrison this mid-station, which modern helps to travel have placed at our doors, and who knows that at one station or other Americans and Frenchmen may not be found to join in the good work? The new railway has made an American Expedition extremely easy.

And now let us enter a little more into particulars.

The central line of the eclipse will first meet the earth's surface in the Arabian Sea, and entering on the western coast of India, will pass right across one of the most important parts of Hindustan, in a S.E. by E. direction. In this part of the peninsula the sun will be about 20° above the horizon when totally obscured. The duration of totality will be two minutes and a quarter, and the breadth of the shadow about seventy miles. On leaving the eastern coast of the Madras Presidency, the central line will cross Palk's Straits, passing about ten miles S.W. of the island Jaffnapatam, and over the northern part of Ceylon, where the small towns of Moeletivoe and Kokelay will lie near the central line; and also the well-known naval station of Trincomalee, which will be about fifteen miles S.W. of the line. Continuing its course over the Bay of Bengal, the shadow will cross the S.E. point of Sumatra, and will touch the south-western coast of Java, where Batavia, the capital, will lie nearly sixty miles N.E. of the central line; and two other smaller towns, Chidamar and Nagara, will also be very near the middle of the shadow path. In the Admiralty Gulf, on the N.W. coast of Australia, the eclipsed sun will be only ten degrees past the meridian, and not far from the zenith; in consequence of which the totality will last $4^m\ 18^s$, or only four seconds less than the time of greatest duration. Lastly, passing through the most barren and uninhabited portion of Australia, crossing the Gulf of Carpentaria and the York Peninsula, the shadow will ultimately leave the earth's surface in the Pacific Ocean.

At present not too much is known about the chances of weather at any place; but what is known seems to point to a fair chance of success in both India and Ceylon, as the eclipse occurs during the N.E. monsoon; but, in any case, the experiences of the last Expedition show that for such a momentary phenomenon these chances need scarcely be taken too seriously into consideration, seeing that where the finest weather was predicted a terrible pall of cloud covered the sky.

Next as to the work which the present state of our knowledge shows to be most desirable. This has been pointed out by Mr. Lockyer, in a communication to the Royal Society, and here we may in the main quote from his paper. Mr. Lockyer states:—

"In my opinion the fundamental points of attack are:

"α. Spectroscopic observations made with such an instrument as the one I took out to Sicily, *equatorially mounted*, and with reference spectra.

"β. Photographic observations made with such an instrument as the one I took out to Sicily, namely, a camera with large aperture and small focal length *equatorially mounted*. . . . .

"Perhaps I may clear the ground by stating what, in my opinion is comparatively UNIMPORTANT, so far as the crucial points are concerned, though to be tolerated if the crucial points are strongly taken up.

"α. Photographing prominences.

"β. Sketching anything but the *changes* in the corona.

"γ. Polariscopic observations.

"δ Observing Baily's Beads.

There should be one instrument, and Mr. Pogson could probably provide this in India, to determine the position of prominences before and after totality. *During totality they should not be observed at all except incidentally.*

"At each place (*i.e.*, India, Ceylon, Australia) the spectroscopes should be employed for half an hour (to be on the safe side) before totality, in scrutinising the crescent at its narrowest place and the chromosphere outside the following limb of the moon.

"At each place, as before defined, there should be a spectroscope with a finder, and equatorial motion (or some equivalent arrangement) directed to the sun's centre, to record any changes which take place in the spectrum from, say, half an hour before to half an hour after totality, and during totality, *bien entendu*. The relative darkness or brightness of the lines should be recorded every ten seconds.

"This spectroscope should have moderate dispersion, large object-glasses for collimator and telescope, and with focal length such that two or three degrees round the sun should be taken in (*i.e.*, 1° or $1\frac{1}{2}$° from the sun's centre), and a large field. . . .

"To come to the details of the expedition to Ceylon; I am of opinion that it need not exceed the following numbers, as my Sicilian experience has taught me that we may depend upon much valuable help from the officers at the place of observation:—

> "1 Telescope-Spectroscopic observer; 2 assistants.
>
> "1 Photographer; 2 assistants. This duty perhaps may be entrusted to skilled Sappers.
>
> "1 Spectroscopic observer; 1 assistant, or 8 in all.

"Among general observations, I would point out as being of extreme importance:

> "*a*. Rays before, during, and after totality—their length, direction, and colour.
>
> "*β*. Colours of the various layers of chromosphere, and of clouds and landscape. The *order* of these colours is of great importance.
>
> "*γ*. Dark rays or *rifts*; whether they change, and whether they extend to the dark moon, or stop short above the denser layers of the chromosphere.
>
> "*δ*. The colours of the corona between bright or dark rays.
>
> "*ε*. All changes in corona.
>
> "*ζ*. Comparative brightness of rays and chromosphere and outer corona. . . . . .

"In the above letter the nomenclature employed is the one I suggested in a recent lecture at the Royal Institution, namely:

> "*Corona*, embracing the whole compound phenomenon outside the prominences (including rays and streamers), part of which is undoubtedly non-solar.
>
> "*Chromosphere*, embracing the whole of the solar portion of corona, and all bright line regions outside the photosphere."

It is scarcely necessary to point out th[illegible] above deals with possibilities, rather than wi[illegible]bilities. We are convinced that a much larger party would do good work in Ceylon, but our scientific leaders are right in asking what our Government cannot refuse; and, moreover, we may hope that the magnificent stations in India on the Neilgherries, at considerable elevations, will be strongly garrisoned, as they can well be by the eminent observers now in India.

We trust that these efforts to procure fresh observations will meet with the largest measure of success, for certainly the question of the Sun's Corona is the scientific question of the day. Once settle what is the real nature of the sun's surroundings, and the path of work is open for the more distant stars. So long as our knowledge of the sun is clouded by contending hypotheses, we cannot hope for real progress.

For our part we do not doubt that the Government will act as admirably as they did last year in the same branch of research when the requirements of Science are properly laid before them; and if the elements are equally kind, we may hope for a large increase of our knowledge.

## TYNDALL'S "HOURS OF EXERCISE IN THE ALPS"

*Hours of Exercise in the Alps.* By John Tyndall, LL.D., F.R.S. (London: Longmans.)

THIS volume is a collection of short articles which have already seen the light in various publications, and are here thrown together, as the author says, "partly to preserve to myself the memory of strong and joyous hours, and partly for the pleasure of those who find exhilaration in descriptions associated with mountain life." Accordingly we find in it accounts of exciting scrambles, such as the Lawinenthor and the Old Weissthor, the first ascent of the Weisshorn, and the various assaults upon the Matterhorn, crowned at last with success. Of sadder interest are the story of the death of Benner, the professor's faithful guide, upon the Haut de Cry, contributed by one of the survivors; notices of the accidents on the Col de Géant and on the Matterhorn; and, hardly less in interest though with happier ending, the rescue of a porter from the jaws of a crevasse on the great Aletsch Glacier, and the author's own hairbreadth escape on the Piz Morteratsch. All these are described with his usual graphic power and intense appreciation of natural scenery; sometimes in the philosophic vein, when a glass of whisky gives "a flash of energy," and even a ham sandwich can only be regarded as a conditioned form of potential muscular force; or sometimes in the more jubilant mood, when we are shown the grave professor "delighting to roll himself in a bubbling pool in some mountain stream, and afterwards dance himself dry in the sunshine."

Together with these sunny memories of alps and cascades, snow-fields and glaciers, there are some chapters of a more distinct scientific import, to which, as most germane to the pages of NATURE, we shall confine our notice. The first of these—the twentieth in the volume—is on Alpine Sculpture. The professor, we need hardly say, is a strong "Erosionist," attributing the valleys to the sculpturing influences of water, frost, and ice, as opposed to those who regard them as the result of fissures in the earth's crust produced by strains during its upheaval. His summary of the evidence for "sculpture *v.* fracture" strikes us as particularly good, and, as it happens, we can bear testimony from personal experience to the accuracy of the facts cited. He shows that by a simple geometric calculation, the width of the fissures produced by the upheaval of a hundred miles of the earth's crust to a maximum height of four miles would bear a very small ratio to the width of the existing valleys; therefore that the most which can be claimed for fissures is that they have guided the action of meteoric forces, have, as it were, drawn the rough sketch on the stone which has directed the picks of Nature's quarrymen, and guided the chisels of her sculptors. He points out that in the most fissure-like of gorges, such as those of the Via Mala or Pfäffers, characteristic water-marks are visible from top to bottom. His description of the latter may be taken as a summary of the evidence in these and many other cases which he has quoted. "Here the traveller passes along the side of the chasm, midway between top and bottom. Whichever way he looks, backwards or forwards, upwards or downwards, towards the sky or towards the river, he meets everywhere the irresistible and impressive evidence that this wonderful fissure has been sawn through the mountain by the waters of the Tamina." The only points in Prof. Tyndall's description to which we a little demur are when he speaks of the traveller "passing along the chasm midway between top and bottom," the fact being that the well-known gallery is only a few yards above the Tamina; and where he quotes the gorge as an illustration of water-action upon *limestone* rock. It is true that the strata here are not crystalline, and they may be occasionally calcareous, but we should hardly venture to apply the name of limestone to the hard black shales or slates out of which the gorge itself is cut. Professor Tyndall also omits to call attention to the close connection between the direction of the principal joints and the form of the gorge. This is especially noteworthy at Pfäffers, where the chasm is not vertical, but inclined to the horizon at an angle of some 70°, the water having followed, as is its wont, the direction of least resistance, viz., one of the sets of joint planes. The gorges of the Pantenbrücke, the Aar above Im-Hof, with many others, might be quoted as instances of the same. We think, indeed, that in arguing against those who ascribe alpine sculpture mainly to fracture, the professor does not quite do justice to the influence which fissures, faults, and joints (which last may, in many cases, be connected with the others) exercise in directing the meteoric agents. These have not, indeed, fashioned the mountains, but they have obliged the sculpturing forces to work in certain directions, have been like the rails or the points which cause a locomotive to follow a particular course instead of wasting its power in wandering over the fields.

Further on in the chapter, Prof. Tyndall refers to his own favourite theory of glacier sculpture, with regard to which he expresses himself more guardedly than in the paper originally published in the *Philosophical Magazine* (vol. xxiv. p. 169). Still we cannot say that we are convinced by his arguments even in their modified form. No one, of course, would deny that a glacier can deepen its bed; the question is simply one of degree. With regard to this our space will allow us to do little more than express dissent, and indicate one or two points where, while not disputing Prof. Tyndall's facts, we cannot accept his inferences.

The silt which is brought down by a glacier stream cannot, we think, be taken as a measure of the abrasion exercised by the glacier; surely the greater part is derived from the stones crushed between the ice and rock; it is the grist from the glacier mill, rather than the detritus of the nether stone. We fail also to see how, unless under exceptional circumstances, a glacier can "do more than abrade." Granted that "rocks are not homogeneous, they are intersected by joints and places of weakness which divide them into virtually detached masses," we doubt if it follows that "a glacier is undoubtedly competent to root such masses bodily away." A heavy body sliding over such masses and in close contact with them, would, we think, be more likely to keep them in their place, and certainly rocks from which glaciers have retreated do not exhibit evidence of this kind of erosive action. We confess, therefore, to still regarding the effects of glaciers as comparatively superficial, and classing the ice ploughs of past ages as among the efforts of scientific imagination.

A considerable portion of the latter part of the volume is devoted to a *résumé* of the "viscous" and "regelation" theories of glacier motion; a controversy which can hardly yet be regarded as concluded, seeing that the experiments of Mr. Mathews and Mr. Froude, to which Prof. Tyndall briefly alludes, appear likely to have a very important bearing upon the question of whether or not ice under any circumstances is a flexible or plastic substance to an appreciable extent.

Among the very miscellaneous scraps with which the volume terminates, is an account of the voyage to Algeria to observe the Eclipse. This, so far as its main purpose went, was a dismal failure, but remarks are introduced on the colour of the sea and sky, a subject already treated by the author in his "Glaciers of the Alps." During the voyage home a number of bottles of sea-water were secured from various stations, which were afterwards examined in London by passing through them a beam of electric light—the purity or impurity of the water is then shown by the less or greater amount of light which it scatters. Briefly, the result was that the dark blue water was very pure, the cobalt-blue rather less so, while the green tints denoted the presence of much suspended matter, and the yellowish green was very thick. A remarkable instance of this variety of colour which, if our memory serve us, he has not quoted, is in the Lakes of Thun and Brienz; the waters of the latter, which receives the silty streams of the Aar and the Lutschine, are distinctly green, while those of the former, into which no important glacier torrent directly enters, are of a beautiful blue.

In fine, though there is little new about the book, many of Prof. Tyndall's admirers will be glad to possess in a convenient form so many thoroughly characteristic papers, displaying at once his thoughtful mind and intense love of nature, as well as his great command over nervous and picturesque English.

T. G. BONNEY

## OUR BOOK SHELF

*The N[illegible]tural History of Plants.* By H. Baillon. Translated by Marcus M. Hartog. Vol. I. (London: L. R[illegible] and Co. 1871.)

HAVING noticed, on its publication, the first volume of Prof. Baillon's "Histoire des Plantes" (see NATURE, vol. i, p. 52) we need scarcely do more than call attention to the English edition which now lies before us. The translation, we may say at the outset, appears to us to be well done; the meaning of the original is, as far as we have observed, carefully preserved; and a better knowledge of his subject is shown by the translator than is always the case in English renderings of foreign scientific works. The co-ordination of the natural orders followed in the work is, as was mentioned in our notice of the original, novel; whether it will stand is a question on which we ought not, perhaps, to express an opinion until the plan is more fully developed. We could have wished that the author had given in this first volume some general sketch of his new system, with a defence of its peculiarities. So competent an authority as Prof. Baillon cannot have departed from the ordinary arrangement without cogent reasons, which we should have liked to have known. It is always a great advantage to English systematists to know the views of their fellow-workers on the Continent. We miss also the great assistance that is afforded to the systematist by a tabulated clavis of the genera belonging to each natural order. The amount of information contained in the volume as to the various relationships of the natural orders described in it, the morphology of their genera, the distribution of the different types, and the economic products obtained from the species, is immense. It possesses, however, the defect so common in foreign scientific works, of the absence of any table of contents or index to the subjects treated of. Had the publishers of the English edition supplemented the index of genera and subgenera with one referring to the various topics discussed, they would have rendered the English edition a practically more useful contribution to botanical literature

CALYCANTHUS FLORIDUS: Floriferous shoot.

than the French original. The illustrations are profuse, and of that excellence which we look for in vain in works originally published in this country. We append one of the well-known "Allspice Tree," the *Calycanthus floridus*. The small order Calycanthaceæ, including only the American *Calycanthus* and the Japanese *Chimonanthus*, is one the true position of which has been much disputed by systematists. Baillon makes it a "series" of Monimiaceæ, with which he also unites the Australian Atherospermeæ, bringing this order forward from its usual position among the Incompletæ to close alliance with Magnoliaceæ and Anonaceæ. A. W. B.

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### A New View of Darwinism

I HAVE only just seen the two letters in answer to one from me on Darwinism which you were good enough to insert in NATURE, and to which I ask the favour of being allowed to reply. I have to thank Mr. Darwin for his references and for the tone of his letter, which is in such marked contrast to the angry dogmatism of Mr. Wallace.

Mr. Wallace commences by ridiculing the phrase the Persistence of the Stronger. The phrase was not mine, it has been used by a better man than I, namely, by Prof. Jowett, and it has the advantage of not involving an identical expression, which the Survival of the Fittest does. "That those forms of life survive which are best adapted or best fitted to survive," is not a very profound discovery; it might have suggested itself even to a child, and if Mr. Wallace means nothing more than this when he speaks of the theory of Natural Selection, he cannot claim to have added much to the world's philosophical opinions.

He then complains that I have only touched one of the many facts relied upon by Darwinians; I refer him to my letter, in which I distinctly say that it contained only one of my objections, and that I have many more which will follow if the Editor have patience with the discussion. The reply to Mr. Wallace will confine me, however, in this letter to the ground covered by the former one. Having disposed of the formal and personal matters, I now approach the matters of fact about which we are at issue.

Here, I am sorry to say, I am met in a very different spirit by Mr. Wallace to that in which Mr. Darwin meets objections. Dogmatism, bold and unwavering, was the privilege of the philosophy of the Schools, but in the 19th century it is puerile. Mr. Wallace states boldly, without any authorities, merely as an imperial *ipse dixit*, that the most vigorous plants and animals are the most fertile. I had, at least, the decency to quote the book of Mr. Doubleday, containing a magazine of facts and examples in support of my view, and which tells exactly the other way.

This view has not been correctly stated by Mr. Wallace. The position I maintain is this, that, as a general law, those individuals which are underfed and lead precarious lives, are more fertile than those whose advantages make them vigorous and healthy. The ringing of the bark and the pruning of the roots of barren fruit trees and the starving of domestic animals to make them fruitful were examples to this end.

Mr. Wallace quotes only one example in his own support, and I will accept it as a crucial test of my position, which he will acknowledge to be fair; the case of the Red Indian and the Backwoodsman. The Red Indian lives entirely on flesh, the Backwoodsman almost entirely on vegetable food. Like meat livers in every part of the world, in Mexico, on the River Plate, in Siberia, in Turkestan, and in some parts of Russia, the Red Indian is not a fertile creature. The Backwoodsman, like vegetable feeders everywhere who are not luxurious, in India, China, Poland, and the Russian provinces bordering on it, Ireland, &c., is comparatively fertile, but only comparatively. It is a mistake to suppose that the Backwoodsman is specially fertile, and in a few years he becomes, as the inhabitants of Kentucky and Tennessee have been long known to be, diminishing in numbers, the population of the States being kept up by immigration.

Mr. Chadwick, in his "Sanitary State of the Labouring Classes," observes that where mortality is the greatest there is much the greatest fecundity; thus, in Manchester, where the deaths are one to twenty-eight, the births are one to twenty-six, while in Rutlandshire, where deaths are but one to fifty-two, births are one to thirty-three, showing that a state of debility of the population induces fertility. This only supports the common dicta of doctors that consumptive patients are generally very fertile. The pastoral tribes of Eastern Russia which have recently taken to agriculture, such as the Tchuvashes, &c., have begun to increase most rapidly. The Hottentots at the Cape, who were formerly a numerous race living very hard lives, are almost extinct now that they are carefully tended and well fed. The Yeniseians, the Yukahiri, and other Siberian tribes, have disappeared like smoke before the advance of Russian culture; they have suffered little if at all from the Russian arms.

Let me quote a curious example in answer to Mr. Wallace from the very race to which he has referred. Captain Musters, in describing his recent journey through Patagonia at the Anthropological Institute, told us that it was the custom for the Patagonian women to be bled at certain times referred to, as they believed *it made them fertile*. Among the Patagonians, therefore, we meet with empirical witnesses, unsophisticated by our philosophy, to the truth of the position I maintain. But those who live in large cities need not travel to Patagonia. The classes among us who teem with children are not the well-to-do and the comfortable, but the poor and half-fed Irish that crowd the lowest parts of our towns. I am not contrasting now the fat with the lean, but the comfortable classes with those who lead precarious lives—the vigorous in health with the sickly, the half-fed, and the weak. It will be asked, why rely so much upon man? The answer is that I quite agree with Mr. Darwin that man is subject to the same natural laws as the animals, and further I believe that since we have studied man more closely and under a greater variety of conditions, facts derived from our experience of man are of greater value than those deduced from our examination of the other animals.

But let us turn to these latter for a space; and here I tread with much greater diffidence, for I am aware of the vast experience and fund of illustration possessed by Mr. Darwin, and I have to say that I am unconvinced by the arguments he has adduced. With the transparent frankness of all his writings, Mr. Darwin, in one of the references to which he has commended me, has collected a very large number of examples that tell very strongly against him, and which I again commend to Mr. Wallace. I refer to the 18th chapter of Mr. Darwin's book on the "Variation of Plants and Animals under Domestication," and especially to that portion beginning on page 149. In speaking of animals, he says:—"The most remarkable cases, however, are afforded by animals kept in their native country, which, though perfectly tamed, quite healthy, and allowed some freedom, are absolutely incapable of breeding Rengger, who in Paraguay particularly attended to this subject, specifies six quadrupeds in this condition, and he mentions two or three others which most rarely breed. Mr. Bates, in his admirable work on the Amazons, strongly insists on similar cases, and he remarks that the fact of thoroughly tamed wild animals and birds not breeding when kept by the Indians, cannot be wholly accounted for by their negligence or indifference, for the turkey is valued by them, and the fowl has been adopted by the remotest tribes. In almost every part of the world, for instance, in the interior of Africa, and in several of the Polynesian islands, the natives are extremely fond of taming the indigenous quadrupeds and birds, but they rarely or never succeed in getting them to breed," and so on, through sixty pages of closely-packed examples. And what is Mr. Darwin's commentary on these facts? I again quote page 158:—"We feel at first naturally inclined to attribute the result to loss of health, or at least to loss of vigour, but this view can hardly be admitted when we reflect how healthy, long-lived, and vigorous many animals are under captivity, such as parrots and hawks when used for hawking, chetahs when used for hunting, and elephants. The reproductive organs themselves are not diseased, and the diseases from which animals in menageries usually perish are not those which in any way affect their fertility. No domestic animal is more subject to disease than the sheep, yet it is remarkably fertile." Mr. Darwin, with equal clearness and conclusiveness, decides that this sterility cannot be due to a failure of sexual instincts, change of climate or of food, or want of food or exercise; and he concludes that certain changes of habits and of life affect in an *inexplicable manner* the powers of reproduction. But what is true of man it is reasonable to suppose is true of all these instances—namely, that it is a more luxurious habit, a more vigorous health, a less precarious existence, induced by the care and attention of domesticators, that have caused the sterility; that these animals are too well off, and not that they are ill off in any way; and this theory explains the whole most conclusively. On the other hand, and in opposition to this vast and uniform collection of examples, Mr. Darwin adduces a few instances which tell the other way, but they are very few in number, and seem to me explicable on other grounds. Ferrets, it is notorious, are always kept in a state of extreme depletion and as thin as possible. Domestic poultry are fed almost entirely on poor vegetable food, while their wild and semi-wild relatives feed much more on worms, insects, and on animal diet generally. In regard to sheep, it is notorious that very weak ewes generally bear twins, that Somersets and Dorsets are more fertile than Southdowns and Leicesters. We have, I may add, no facts to guide us in regard to wild dogs, and few in regard to wild cats; but we do know that in tame ones the half-fed lantern-ribbed curs are more prolific than their sleek relations. In regard to domestic fowls, and especially pigeons, we must remember that their condition is materially altered by the disuse or only very partial and irregular use of their powers of flight, this must reduce their circulation and vigour very considerably, and make them *pro tanto* so much weaker. But these instances, upon which Mr. Darwin relies to answer Doubleday and others, are very partial indeed. In his own pages, as I have already said, they form a very small element compared with the overwhelming cases he quotes on the other side. So much so, indeed, that these cases may be taken as exceptions which prove the rule that domestication and improved conditions of life induce sterility in animals.

It savours of scholastic philosophy to speak of Nature as exercising any influence on the regeneration of races, and yet there may be sound philosophy in the old notion that when an individual or a class is in danger of being extinguished from want, Nature puts forward a special effort to preserve it. The sickly mother, the half-starved plant, is more likely to breed than the healthy and the vigorous. If we remove the peasant's family to the drawing room, it will cease to be composed of ten and twelve children. If we remove our daisies and cowslips to the greenhouse, their flowers grow double, and they ripen no seeds. The vine that has felt the frost is the one to pay the rent. Wherever we turn, in fact, we meet with examples of the universal law; and this law seems to be at issue with an important portion of Mr. Darwin's theory, namely, that in the struggle for existence, the vigorous, the hearty, and the well-to-do, elbow the weak and decrepid until they elbow them out of existence, and supplant them. If I have said anything above which can be construed into an impertinence, I unconditionally withdraw it. The only excuse for soreness, is an impatience at what seems to the writer to be indefensible dogmatism. The days will not be ripe for scientific dogmatism until the Infallibility of Positive Philosophers has been generally accepted, and it does not do to forestal that millennium.

H. Howorth

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Mr. Wallace has effectually set aside Mr. Howorth's new views on Darwinism, and it now only remains to point out that the latter gentleman, in his instances, puts the cart before the horse. Hens that are fat and don't lay are fat because they don't lay. When the sexual powers, either in plants or animals, are defective from accident or design, the overgrowth always takes place, and this among animals is chiefly by the increase of adipose tissue.

Birmingham Lawson Tait

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## Recent Neologisms

I have been long accustomed to register the first appearance of new words and phrases. Of course the vast majority of these take no root, perishing where they fall. Here is a sample of the latest issue: *Survival*, introduced, I think, by Darwin; *indiscipline* and *impolicy*, which were brought in by the Franco-Prussian War, and also the vulgarism *to telegram*. The greatest atrocities in this line are committed by "physicists," if the shade of Faraday will pardon me the use of that word; and far away the worst coinage I ever encountered is due to Mr. Alfred R. Wallace. As it is "meet and right and our bounden duty" to stigmatise such intruders, and if possible prevent their adoption, I take the liberty of making my feeble protest against Mr. Wallace's "prolificness," which he introduces to our notice in his letter on Mr. Howorth (Nature, July 6, 1871, p. 181). In this case the hideousness of the coinage is some guarantee against its reception.

Malvern Wells, July 8 C. M. Ingleby

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## Affinities of the Sponges

I have just read with much interest the paper in Nature by Mr. W. Saville Kent, criticising my friend Carter's article in the "Annals of Natural History" for this month, in which I fully concur. How Mr. Carter can have fallen into such an error, for such I must call it, I cannot imagine, as comparing a group of animals in Botryllus to those sponge cells, even in so highly a developed form as Grantia. For, taking this as the highest known form of sponge animal, it is at most only a monociliated sac, as shown both by Prof. Clark and by Mr. Carter. Now, it is well known to all investigators, and Mr. Carter has shown it himself, that the animals of Botryllus have distinct oral and fæcal apertures, whereas the sponge cell, so far as has yet been seen, has only an oral aperture. Again, the Ascidian Botryllus is shown to be far higher in the scale when we come to compare its internal organisation, and not merely to confine ourselves to the sac-like tunic. The discharge of the fæcal matter into a common cloacal canal is to me not a sufficient reason for comparing these groups of animals to the sponge animals in *Grantia*.

But what I wish to draw attention to more particularly is this, that in the hurry and bustle of our investigators of the present day, all old associations are mostly, if not entirely, forgotten. I can scarcely think that they are ignored, but are forgotten. Thus, Prof. Grant was, I believe, the first to determine the character and the full importance of the seed-like body in *Halichondria* by placing watch-glasses in the vessel in which living specimens of the above sponge was placed; the bodies were thus discharged from the fæcal canal of the parent sponge, and attached themselves to the watch-glasses, and he then carefully watched their development. Mr. Carter, being a pupil of Dr. Grant, no doubt followed his teacher's plan of investigation, which has led to the brilliant results of this gentleman's investigations of the fresh-water species in the tanks at Bombay. The clear and lucid manner of investigation detailed by Prof. Grant in the *Edinburgh New Philosophical Journal* (1826-27) might be held as a pattern for investigators, but he appears to be almost entirely lost sight of.

Again, as regards the animals of *Grantia compressa*, Prof. Reay Greene certainly preceded both Prof. Clark and Mr. Carter in his investigations, and has figured these monociliated animals in his handbook, published in 1859, p. 31, fig. 6. The figures are on the same scale as those given by Mr. Carter, and indeed some of the groups figured are so much like those given by Mr. Carter in the "Annals," pl. 1, sqr. 13, *a*, *g*, *h*, that it would be difficult to separate them, and the same may be said of Fig. 41 of Prof. Clark's in "Ann. Nat. Hist" pl. 6, 1868. The only difference being the want of the funnel-shaped mouth, which seems to have escaped the observation of Prof. Greene, probably owing to want of definition in the instrument used in the investigation. Now there is an amount of credit due to the first demonstrator of these animals, which, so far as I have seen, does not appear to have been accorded to him; and I therefore take the liberty of directing attention to this fact. I do not know Prof. Greene, and therefore do not take up this matter on personal grounds, but only in fairness due from one scientific man to another, and I hope my friend Carter will take this in the spirit it is intended. EDWARD PARFITT

Exeter, July 8

## Cramming for Examinations

I ENCLOSE one or two *bonâ fide* extracts from "Middle Class" examination papers which have during the past few weeks come under my notice officially.

I do not wish thereby to reflect so much on the candidates as upon the mode of teaching in Middle Class schools, which produces such results.

As might be expected, where evidence of "cramming" from a text-book and want of practical knowledge are equally manifest, some of the answers in the papers from which these are selected are pretty good—but what can be the real value of knowledge of this sort?

The questions are sufficiently indicated by the answers.

CANDIDATE A.

Chlorine may be taken from decayed vegetable matter and animal matter, also manure. . . . It is used for killing insects, it is compounded with lime, and is very good when compounded with lime for the manuring of fields. Lime is chiefly formed from Chlorine.

CANDIDATE B.

Chlorine is prepared by mixing $Ca\,Cl_2$ with $H_2O$

$$Ca\,Cl_2 + H_2O = CaO + H_2 + Cl_2$$

Chlorine is a colourless invisible gas. Has no odour nor taste. . . . Hydrochloric acid is prepared as follows—

$$Ca\,Cl_2 + H_2O = CaO + 2\,H\,Cl$$

CANDIDATE C.

Carbon is an elementary substance, it is one of the constituents of the atmosphere, it is found in lime and pits among the coal. When the lime is soaked with water the carbon escapes out and the lime moulds away.

AN EXAMINER

## Great Heat in Iceland during the present Summer

MR. R. M. SMITH has received a note from Dr. Hjaltelin, Corresponding Member of the Scottish Meteorological Society at Reykjavik, dated June 30, of which the following is an extract:—

"We have now the most excellent season you can imagine in these latitudes, the average temperature for this month (June) being as high as 59°, which is 12° higher than the mean temperature of the past four Junes. I was yesterday near the Hengil Mountain, just at that place where we pitched our tent last time you were here, and the heat was quite unsupportable in the valleys. The wind has been continually blowing from the south-west. Some Englishmen setting out for the Geyser will have something to tell of the extraordinary heat we have at present."

ALEXANDER BUCHAN

## The Late Thunderstorm

AN ash tree in the garden attached to the farmhouse of Wester Cringate, near Fintry, struck by lightning on the 20th of June, presents a singular appearance.

About 20ft. from the ground a large branch has been torn from the trunk. The bark has been neatly peeled off for a few feet above and below the place from which the branch shot out. The wood has been first struck a little above the branch, and shows a clean cut, such as might have been made by a sharp-edged tool, as if a chisel three inches broad had been driven into the wood for about four inches. The branch itself has been torn, not cut, and a stripe of the trunk about two feet long below the branch has also been torn out.

For the next four or five feet the tree has suffered no damage of any kind, but after that space the trunk bears six parallel downward scars, varying in length from two to five feet. The scars do not all begin or end at the same height, although each might be cut in some point by a horizontal plane passing through the tree. They spread over about half the circumference of the trunk, and can all, or nearly all, be seen from one standpoint. The most striking circumstance, however, is the almost perfect parallelism of the scars, which are not vertical, but a little twisted round the trunk like the rifling of an Armstrong gun, the rifling in this case being on the outside of the barrel. Six chisels of about half an inch in breadth seem to have ploughed into the wood, tearing off at the same time rather broader stripes of the bark. Towards their lower ends the three right-hand scars cease to be quite parallel, and tend to converge; but all three die out before the convergence takes place, and the tree for the next two feet or so is unscathed. Five feet from the ground (at about the point at which the three scars would converge, if produced) a single rut cutting deeply into the wood commences, which continues down to the soil.

The garden wall (which is a "dry-stone dyke," *i.e.* of loose uncemented stones) passes some three feet behind the tree, on the side directly opposite to that on which the markings above described occur. Outside of this garden the lightning has ploughed two pretty deep parallel ruts through the grassy soil some four feet apart, and stretching from the foot of the wall to the edge of a ditch, a distance of three feet. These ruts are the last observable traces of the passage of the lightning, and were probably made by the currents which engraved the three left-hand scars on the tree. Of course it is impossible to decide whether the currents passed through the open wall, or down the outside of it.

Three sheep on the neighbouring farm of Spittalhill were killed in the same thunderstorm. Their carcases were found lying in a line and were very much swollen, but bore no external marks of injury. A small patch of wool had been stripped from the flank of one of them, but probably this had no connection with the cause of death. R. L. JACK

Geological Survey, Fintry by Glasgow, July 5

## Saturn's Rings

As you have favoured my work on "Saturn's Rings and the Sun" with criticism, I feel sure that as that criticism is adverse to my views, you will in fairness allow me to reply to it.

I will do so in detail. Your reviewer commences very much under the impression that Prof. Clerk Maxwell having investigated the "Stability of Saturn's Rings," no one else is to venture into any discussion touching on their nature or origin. In fact he issues a *caveat*—Prof. Clerk Maxwell has concluded the subject! Next he asserts that I have not seen the Professor's work, because I ascribe to the perusal of Mr. Proctor's "Saturn and its System," the enlistment of my "interest in favour of the Satellite Theory." This is surely beyond his province, as I am free to choose my own point of starting. Mr. Proctor's work interested me, and so did Mr. Clerk Maxwell's, but the former elicited my work, the latter did not.

He next accuses me of placing too great faith in figures, and shows surprise at my giving the hourly rate of the solar motion to a mile, and the solar parallax to four places of decimals. The solar motion is that given by the Herschels, and the solar parallax by several observers. He is hard to please. But my reviewer has unfortunately missed the point of my arguments. The actual velocity of this solar motion is perfectly immaterial; indeed, had he followed the reasoning, he would have seen how pointless are his objections.

As regards my arguments in favour of the meteoric theory of the sun, the reviewer is equally inaccurate. As to my being blindly enraptured with that theory, as he is pleased to state, I only reply that very clever men have held it, as he is perhaps aware; and certainly none of the modern theories, cumbrous vagaries of the brain, can compare with it. I have never said that the meteoric theory is the real explanation, but I doubt if we shall ever arrive at a more truthful representation of the solar phenomena.

Lastly, he culminates by saying I am "either innocently or wilfully ignorant of the palpably cyclonic appearance which spots frequently present." All I can say in answer to this is that, having observed sun-spots myself for many years, probably as often as the reviewer, I have *never* observed one single appearance of a cyclonic nature. As I possess Mr. Carrington's valuable work, I have again referred to it, and find it in agreement with the assertion in my book and my own observation. I must apologise for my lengthy letter. A. M. DAVIES

2, Gloucester Terrace, Sandgate, July 4

## On an Error in Regnault's Calculation of the Heat Converted into Work in the Steam Engine

IN Watts's "Dictionary of Chemistry" (vol. iii. p. 125), in the article on Heat by Prof. G. C. Foster, it appears to me that an important error has crept into the discussion of the above calculation.

The nature of the calculation is as follows:—A unit weight of saturated steam at the temperature of 152°C. contains 653 units of heat. Suppose we allow the steam to expand and to do work until the temperature falls to 503°C. the steam then contains 621 units or 32 units less than before, hence starting with water at 0°C., we give it 653 units of heat, and of this 32 only are converted into work, giving us the fraction $\frac{32}{653}$ as the amount of heat converted into work; but the real work produced by an engine is more than twice this. This difference in theory and practice is accounted for by the fact that saturated steam, in expanding and doing work, is partly condensed, hence the body with which we have to deal at the lower temperature is not all steam, but partly condensed water, therefore, does not contain so much heat as was allowed it.

This explanation is so intelligible as to be at first sight sufficient to account for the whole difference; there is, however, another cause, quite as important, and which is this; every time steam passes from the boiler to the cylinder it does work before it is cut off, and allowed to expand; this work is not done at the expense of the steam that passes into the cylinder, but of the whole mass of steam in the cylinder and boiler, which expands and is thereby cooled. The mass of water and steam in the boiler is, however, so large compared to that which passes into the cylinder, that a thermometer could scarcely detect the cooling effect upon it, and before the next stroke this loss of temperature is made up by the fire. Though thus inappreciable, it is nevertheless very important, and in most engines would amount to one-third the work done; in fact all the work done by the steam before it is cut off and allowed to expand is entirely neglected in this calculation, and a source of error introduced.

To correct it there should be added to the heat in the steam at the initial temperature, as many units of heat as the work done before the steam is cut off, would, if converted into heat, raise the amount of water which passes at every stroke in the form of steam into the cylinder. A. W. BICKERTON

Hartley Institution, Southampton, June 26

## *THE CAUSES OF THE COLOURS OF THE SEA* *

PROF. TYNDALL, in his article in the *Fortnightly Review* for the 1st of March, attributes the greenness of the sea to the matter which it holds in solution. Perhaps the following may corroborate his theory. About the Andaman Islands, where the sea is of the deepest blue, there are most startling and sharply-defined changes of colour, from bright blue to green, where a bed of coral exists. This coral is white out of the water, what its colour when growing may be I know not, but the change I mention appears to corroborate the remarks in the article in question, which are appended below, about the green hues observed upon the plate, the screw blades, and the white bellies of the porpoises. One looks down from a hill into a bay of the brightest blue; you see it broken up here and there like a child's puzzle map by irregular patches of as bright green, often crossing several acres as sharply defined as it is possible to imagine, and indicating the existence of coral beds or reefs just below.

Bellary, Madras Presidency W. M'MASTER

* Communicated by Prof. Tyndall.

[We give the passages referred to from Prof. Tyndall's lecture.—ED.]

"Let us clear our way by a few experiments towards an explanation of the dark hue of the deep ocean.* Colour, you know, resides in white light, appearing generally when any constituent of the white light is withdrawn. Here is a liquid which colours a beam sent through it purple, and this colour is immediately accounted for by the action of the solution on a spectrum. It cuts out the yellow and green, and allows red and blue to pass through. The blending of these two colours produces the purple. Does the liquid allow absolutely free passage to the red and blue? No. It enfeebles the whole spectrum, but attacks with special energy the yellow and green colours. By increasing the thickness of the stratum traversed by the beam, we cut off the whole of the spectrum. Through the deeper layer, which I now place in the path of the beam, no colour can pass. Here, again, is a blue liquid. Why is it blue? Its action on the spectrum answers the question. It first extinguishes the red; then as the thickness augments it attacks the orange, yellow, and green in succession; the blue alone finally remains, but everything might be extinguished by a sufficient depth of the liquid.

"And now we are prepared for a concentrated but tolerably complete statement of the action of sea water upon light, to which it owes its blackness. Here is our spectrum. This embraces three classes of rays—the thermal, the visual, and the chemical. These divisions overlap each other; the thermal rays are in part visual, the visual rays in part chemical, and *vice versâ*. The vast body of thermal rays is here beyond the red and invisible. They are attacked with exceeding energy by water. They are absorbed close to the surface of the sea, and are the great agents in evaporation. At the same time the whole spectrum suffers enfeeblement; water attacks all its rays, but with different degrees of energy. Of the visual rays the red are attacked first, and first extinguished. While the red is extinguished, the remaining colours are enfeebled. As the solar beam plunges deeper into the sea, orange follows red, yellow follows orange, green follows yellow, and the various shades of blue, where the water is deep enough, follow green. Absolute extinction of the solar beam would be the consequence if the water were deep and uniform, and contained no suspended matter. Such water would be as black as ink. A reflected glimmer of ordinary light would reach us from its surface, as it would from the surface of actual ink; but no light, hence no colour, would reach us from the body of the water. In very clear and very deep sea water this condition is approximately fulfilled, and hence the extraordinary darkness of such water. The indigo, to which I have already referred, is, I believe, to be ascribed in part to the suspended matter, which is never absent, even in the purest natural water, and in part to the slight reflection of the light from the limiting surfaces of strata of different densities. A modicum of light is thus thrown back to the eye, before the depth necessary to absolute extinction has been attained. An effect precisely similar occurs under the moraines of the Swiss glaciers. The ice here is exceptionally compact, and owing to the absence of the internal scattering common in bubbled ice, the light plunges into the mass, is extinguished, and the perfectly clear ice presents an appearance of pitchy blackness.

"The green colour of the sea when it contains matter in a state of mechanical suspension has now to be accounted for; and here, again, let us fall back upon the sure basis of experiment. This white plate was once a complete dinner-plate, very thick and strong. It is, you see, surrounded securely by cord, and to it a lead weight is fastened. Forty or fifty yards of strong hempen line were attached to the plate. With it in his hand, my assistant, Thorogood, occupied a boat fastened as usual to the davits of

* A note written to me the 22nd of October, by my friend Canon Kingsley, contains the following reference to this point:—"I have never seen the Lake of Geneva, but I thought of the brilliant, dazzling dark blue of the mid Atlantic under the sunlight, and its black blue under-cloud, both so solid that one might leap off the sponson on to it without fear; this was to me the most wonderful thing which I saw on my voyage to and from the West Indies."—J. T.

the *Urgent*, while I occupied a second boat nearer to the stern of the ship. He cast the plate as a mariner heaves the lead, and by the time it had reached me, it had sunk a considerable depth in the water. In all cases the hue of this plate was green, not, of course, a pure green, but a mixture of green and blue; and when the sea was of the darkest indigo, the green was the most vivid and pronounced. I could notice the gradual deepening of the colour as the plate sank, but at its greatest depth in indigo water the colour was still a blue green.

"Other observations confirmed this one. The *Urgent* is a screw steamer, and right over the blades of the screw there was an orifice called the screw-well, through which you could look from the poop down upon the screw. The surface glimmer which so pesters the eye was here in a great measure removed. Midway down a plank crossed the screw-well from side to side, and on this I used to place myself to observe the action of the screw underneath. The eye was rendered sensitive by the moderation of the light; and still further to remove all disturbing causes, Lieutenant Walton had the great kindness to have a sail and tarpaulin thrown over the mouth of the well. Underneath this I perched myself, and watched the screw. In an indigo sea the play of colours was indescribably beautiful, and the contrast between the water which had the screw-blades for a background, and that which had the bottom of the ocean as a background, was extraordinary. The one was of the most brilliant green, the other of the most lustrous ultramarine. The surface of the water above the screw-blade was always ruffled. Liquid lenses were thus formed, by which the coloured light was withdrawn from some places and concentrated upon others. The screw-blades in this case replaced the plate in the former case, and there were other instances of a similar kind. The hue from an indigo sea was always green at a certain depth below the surface. The white bellies of the porpoises showed the same hue, varying in intensity as the creatures swung to and fro between the surface and the deeper water. In a rough sea the light which had penetrated the summit of a wave sometimes reached the eye. A beautiful green cap was thus placed upon the wave when the ship was in indigo water.

"But how is this colour to be connected with the suspended particles? Take the dinner-plate which showed so brilliant a green when thrown into indigo water. Suppose it to diminish in size until it reached an almost microscopic magnitude. It would still behave substantially as the larger plate, sending to the eye its modicum of green light. If the plate, instead of being a large coherent mass, were ground to a powder sufficiently fine, and in this condition diffused through the clear sea water, it would send green to the eye. In fact, the suspended particles which the home examination revealed in green sea water act in all essential particulars like the plate, or like the screw-blades, or like the foam, or like the bellies of the porpoises. When too gross, or in too great quantity, the suspended particles thicken the sea itself visibly. But when sufficiently small, but not too small, and when sufficiently diffused, they do not sensibly interfere with the limpid greenness of the sea itself. They then require the stronger and more delicate test of the concentrated luminous beam to reveal their presence."

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## THE TEMPERATURE OF THE SUN

PROF. NEWCOMB, in reviewing P. A. Secchi's work on the Sun, shows that if the temperature reached 10,000,000° Cent., as asserted by the author of "Le Soleil," the earth would speedily be reduced to vapour. In answer to this objection Père Secchi urges, "that a body may have a very high temperature and yet radiate but very little;" contending that "a thermometer dipped inside the solar envelope in contact with the photosphere," would indicate the temperature mentioned. He adds, "This high temperature, besides, is really a virtual temperature, as it is the amount of radiation received from all the transparent strata of the solar envelope, and this body at the outer shell must certainly be at a lower temperature." What information is intended to be conveyed by the statement that 10,000,000° Cent. "is really a virtual temperature," on the ground that it is the "amount of radiation received from all the transparent strata" outside of the photosphere, I will not attempt to explain; but I propose to show that a thermometer dipped inside the solar envelope in contact with the photosphere, cannot possibly indicate the enormous temperature of 10,000,000° Cent. assumed by Père Secchi. The assertion that "a body may have a very high temperature and yet radiate but very little," were it correct with reference to the photosphere, does not affect the question. It is of no consequence whether the sun's photosphere belongs to the class of active or sluggish incandescent radiators imagined by the distinguished savan; the temperature of the radiant surface, not its capacity to radiate more or less copiously, is the problem to be solved. Accordingly the following statement is intended to show that the temperature of the sun's photosphere at the point where the author of "Le Soleil" supposes his thermometer to be applied, cannot much exceed 4,000,000° Fahr. Observations conducted in lat. 40° 42′, with an actinometer (a drawing of which has been published in *Engineering*) have enabled me to ascertain, with desirable accuracy, the intensity of solar radiation for each degree of the sun's zenith distance from 17° to 75°. The atmospheric depth at the first mentioned zenith distance being only 0·046 greater than the vertical atmospheric depth, I have demonstrated, by prolonging the curve constructed agreeable to the observations referred to, that the intensity of solar radiation on the ecliptic is 67·20° Fahr. at the time when the earth passes the aphelion. The accompanying table, the result of two years of observations, shows the atmospheric depth and the intensity of solar radiation for each degree from the vertical to 75° zenith distance. The ratio of diminution of intensity of the radiant heat during the passage of the rays through the atmosphere being accurately defined by this table, it has been easy to calculate that the amount of retardation of the radiant heat on the ecliptic is 0·207 or 17·64° Fahr. Adding this loss of energy to the amount of observed radiant heat, it will be found that the intensity of solar radiation at the boundary of our atmosphere when the earth passes the aphelion corresponds with a thermometric interval of 17·64 + 67·20 = 84·84° on the Fahrenheit scale. Now, the aphelion distance of the earth is 218·1 times greater than the radius of the sun's photosphere; hence, basing our calculations on the established truth that the intensities are inversely as the areas over which the rays are dispersed, we prove that the temperature of the photosphere is $218{\cdot}1^2 \times 84{\cdot}84° = 4{,}035{,}584°$ Fahr. And if we then add the amount of loss of intensity attending the passage of the rays through the solar envelope, we establish, with absolute certainty, the temperature to which a thermometer will be subjected if "dipped inside the solar envelope in contact with the photosphere."

With reference to the retardation of the rays in passing through the solar envelope, we possess practical data of such a nature that the solution of the problem is by no means mere hypothesis. We know that the density of atmospheric air would be reduced to $\frac{1}{8000}$ of the ordinary density if subjected to a temperature of 4,000,000° Fahr.; hence, if we assume that the solar envelope consists chiefly of hydrogen, it may be shown, due allowance being made for the superior attraction of the sun's mass, that the density of the terrestrial atmosphere at equal depth from the boundary is fully 2,000 times greater than that of the solar envelope. Accordingly, as the sun's rays lose only 17·6° in passing vertically through our cold atmosphere, it may be demonstrated that the loss of energy during the passage of the rays through a transparent solar envelope 80,000 miles in depth from the photosphere, cannot exceed 0·01 or 40,000° Fahr. Let us be careful not to confound this diminution of energy with the reduction of temperature consequent on the dispersion of the rays as they recede from the photosphere during their course through the solar envelope. The reduction of temperature attending dispersion, obviously does not involve any diminution of mechanical energy. It would be waste of time to enter on any further demonstration in refutation of the extravagant assumption that a thermometer in contact with the photosphere would indicate some 12,000,000° Fahr. higher temperature than that which we have established on the basis of the known distance and radius of the sun's photosphere, and the ascertained radiant intensity at the boundary of the earth's atmosphere. Nor need we point out the inconsistency of the doctrine that the sun's photosphere possesses less radiant power than incandescent terrestrial substances, such, for instance, as iron and carburetted hydrogen. But the advocates of high solar temperature may urge, that the law, agreeable to which the temperature of 4,000,000° Fahr. has been determined, is mere *theory*, which, although true for distances of a few feet, may be wholly erroneous when the radiator is millions of miles away.

It has been one of the principal objects of my researches connected with solar heat, during the last three years, to endeavour to determine this question. Accordingly, the difference of intensity of solar radiation at midsummer and midwinter has been particularly observed. Fortunately, the eccentricity of the earth's orbit is sufficient to produce a marked difference of intensity at different seasons; but, on the other hand, the varying depths of the atmosphere resulting from the varying inclination of the earth's axis, apart from the varying distance between the sun and the earth, present serious obstacles. My observations as before mentioned have been conducted in lat. 40° 42′, hence 17° 12′ from the ecliptic at the summer solstice, and 64° 12′ at the winter solstice. Accordingly, the depth of atmosphere has varied during the investigations in the ratio of 1·04 to 2·25; thus rendering comparisons between the actual intensities very difficult. A series of observations made at different hours and seasons has ultimately enabled me to construct the curve before referred to, defining the maximum intensity of the sun's radiant heat for all latitudes at the time when the earth passes the aphelion; likewise defining the retardation of solar intensity for all zenith distances not exceeding 75°. Evidently an accurate knowledge of the solar intensity corresponding with given zenith distances removes the obstacles attending the varrying inclination of the axis of the earth. The variation of intensity consequent on the eccentricity of the earth's orbit has also been accurately determined for each day in the year. The detail not being immediately connected with the subject under consideration, it will suffice to state that actinometer observations conducted under very favourable circumstances, January 7, 1871, proved the sun's radiant heat to be 57·25° Fahr., the zenith distance being 63° 15′. Referring to the table, it will be seen that for equal zenith distance—63° 15′—the temperature produced by solar radiation is only 51·77° when the earth passes the aphelion. An *increase* of solar intensity of 57·25 − 51·77 = 5·48°, when the earth is in perihelion, has therefore been established. This important fact enables us to test on a grand scale the correctness of our assumption that the intensity of solar radiation diminishes in the inverse ratio of the area over which the rays are dispersed.

The aphelion distance of the earth being 218·1 times greater than the radius of the sun's photosphere, while the perihelion distance is 210·9 times that radius, the temperatures produced by solar radiation at the boundary of the earth's atmosphere at midsummer and at midwinter, will be inversely as $218·1^2 : 210·9^2$. Consequently, as the ascertained maximum temperature at the former period is 84·84° Fahr., the temperature produced by solar radiation at the latter period will be $\frac{218·1^2 \times 84·84°}{210·9^2} =$ 90·72° Fahr. Let us ascertain if this theoretical temperature correspond with actual fact. Our table shows that the diminution of solar intensity attending the passage of the rays through the atmosphere, when the zenith distance is 63° 15′, amounts to 15·43° in addition to the diminution of 17·64° on the ecliptic, together 33·07°. Adding this to the temperature 57·25°, observed January 7, 1871, we establish the fact that the temperature at the boundary of the atmosphere is 90·32° Fahr. Agreeable to the foregoing theoretical determination, the temperature ought to be 90·72°, difference = 0·4° Fahr. This discrepancy is accounted for by the fact that the sky, although unusually clear, was not quite free from cirrus haze on the day of observation, as proved by the indication of the solar calorimeter, an instrument by which the presence of any obstruction in the atmosphere is ascertained with absolute certainty. In addition to the proof thus furnished in support of the theory on which our calculations are based, that the temperature at the surface of the sun's photosphere does not much exceed 4,000,000° Fahr., other tests have been adopted with nearly identical results, an account of which, together with necessary delineations, has been published in *Engineering*. These tests prove that, unless the photosphere of the sun possesses relatively less radiating power than incandescent cast iron, or metallic substances coated with lampblack, and maintained at ordinary boiling heat, the temperature indicated by a thermometer "dipped inside the solar envelope in contact with the photosphere" will not exceed 4,100,000 deg. Fahrenheit.

*Table showing the depth of atmosphere, and intensity of solar radiation, for each degree of zenith distance, when the earth passes the aphelion.*

| Zenith Distance | Depth of Atmosphere. | Maximum Intensity | Zenith Distance | Depth of Atmosphere | Maximum Intensity |
|---|---|---|---|---|---|
| Deg. | | Fah. | Deg. | | Fah. |
| 0 | 1·000 | 67·20 | 38 | 1·265 | 62·11 |
| 1 | 1·000 | 67·20 | 39 | 1·283 | 61·81 |
| 2 | 1·000 | 67·19 | 40 | 1·302 | 61·50 |
| 3 | 1·001 | 67·18 | 41 | 1·322 | 61·19 |
| 4 | 1·002 | 67·16 | 42 | 1·342 | 60·88 |
| 5 | 1·003 | 67·12 | 43 | 1·363 | 60·57 |
| 6 | 1·005 | 67·08 | 44 | 1·384 | 60·25 |
| 7 | 1·007 | 67·02 | 45 | 1·406 | 59·93 |
| 8 | 1·010 | 66·96 | 46 | 1·431 | 59·60 |
| 9 | 1·013 | 66·90 | 47 | 1·457 | 59·25 |
| 10 | 1·016 | 66·84 | 48 | 1·485 | 58·88 |
| 11 | 1·019 | 66·77 | 49 | 1·514 | 58·51 |
| 12 | 1·023 | 66·70 | 50 | 1·545 | 58·12 |
| 13 | 1·027 | 66·62 | 51 | 1·577 | 57·72 |
| 14 | 1·031 | 66·54 | 52 | 1·612 | 57·31 |
| 15 | 1·036 | 66·44 | 53 | 1·648 | 56·89 |
| 16 | 1·041 | 66·33 | 54 | 1·686 | 56·46 |
| 17 | 1·046 | 66·21 | 55 | 1·726 | 56·02 |
| 18 | 1·051 | 66·08 | 56 | 1·769 | 55·56 |
| 19 | 1·057 | 65·95 | 57 | 1·815 | 55·09 |
| 20 | 1·063 | 65·82 | 58 | 1·864 | 54·60 |
| 21 | 1·070 | 65·68 | 59 | 1·916 | 54·10 |
| 22 | 1·077 | 65·53 | 60 | 1·970 | 53·58 |
| 23 | 1·085 | 65·38 | 61 | 2·037 | 53·05 |
| 24 | 1·093 | 65·22 | 62 | 2·098 | 52·50 |
| 25 | 1·102 | 65·04 | 63 | 2·164 | 51·90 |
| 26 | 1·111 | 64·86 | 64 | 2·235 | 51·40 |
| 27 | 1·121 | 64·67 | 65 | 2·312 | 50·81 |
| 28 | 1·132 | 64·48 | 66 | 2·398 | 59·20 |
| 29 | 1·141 | 64·28 | 67 | 2·490 | 49·57 |
| 30 | 1·152 | 64·07 | 68 | 2·591 | 48·91 |
| 31 | 1·164 | 63·85 | 69 | 2·701 | 48·25 |
| 32 | 1·176 | 63·63 | 70 | 2·821 | 47·55 |
| 33 | 1·189 | 63·40 | 71 | 2·952 | 46·84 |
| 34 | 1·203 | 63·16 | 72 | 3·097 | 46·12 |
| 35 | 1·217 | 62·92 | 73 | 3·255 | 45·37 |
| 36 | 1·232 | 62·67 | 74 | 3·428 | 44·60 |
| 37 | 1·248 | 62·40 | 75 | 3·624 | 43·78 |

J. Ericsson

## GREYTOWN AND ADJACENT COUNTRY

GREYTOWN is important as the only port possessed by Nicaragua on its Atlantic coast, and is situated in 11° N. lat. and 84° W. long. The place itself is insignificant enough, as a glance at the accompanying view of the interior of the harbour will show; at the same time it is of strategical importance in many ways, and its history is not uninteresting. The climate is humid, and along the low coast-lands a tropical heat prevails. The heat is never oppressive while the trade winds blow, but during calms it is sultry and overpowering. The prevailing type of disease appears to be a low form of intermittent fever, which is not to be wondered at, considering that Greytown is built upon a swamp. June, July, and August are considered the unhealthy months, and January, February, and March the healthiest, the thermometer seldom exceeds 82° Fahr., or falls below 71° Fahr. in the shade.

SEASONS *

| RAINY | DRY |
|---|---|
| June | January |
| July | February |
| ⅔ August | March |
| ½ October | April |
| November | May |
| December | ⅓ August |
| | ½ October |
| The rain descends in a perfect deluge, accompanied by thunder and lightning. | Sometimes not a drop of rain falls, but generally it is showery, even in the so-called dry season at Greytown. |

In the interior, where the forest vegetation has been cleared away in the neighbourhood of the islands and lakes, the seasons are more marked, and the dry season is really dry, not a drop falling. At times Greytown is visited by terrible gales or hurricanes styled "Northers," at such times the trade wind is gradually killed, and a

GREYTOWN HARBOUR

calm precedes the coming storm, the barometer falls rapidly, and the clouds bank up in the horizon. After these warnings the norther commences without further prelude, and in an incredibly short time the sea is churned up into great and violent waves, whilst the surf on the bar is terrific. A norther will sometimes last for three whole days.

The whole civilised population of the Nicaraguan and neighbouring republics is collected on the Pacific side of Central America; the Caribbean coasts being almost entirely uninhabited, with the exception of a few independent tribes of Indians along the banks of the large rivers like the Indian and Rama. The principal tribes are the Valiente, Rama Cookwra, Woolwa Tonga, and Poya tribes, all interesting from an ethnological point of view, especially as they are fast disappearing. There is generally a small camp of some of these tribes on the sandy spit (Punta d'Arenas) at the entrance to Greytown harbour, who catch and sell turtle, &c. Accounts of these Mosquito tribes will be found in the Journal of the Royal Geographical Society, 1862, p. 242, &c., by Mr. Bell, and in the last volume of Memoirs of the Anthropological Society, by Mr. Collinson. This region, *i.e.* the valley and lowlands of the San Juan and the lakes of Nicaragua and Managua, is more particularly interesting to naturalists and geologists, as forming the border land between two of the great primary distributional provinces for the terrestrial vertebrata in the present world recognised by Prof. Huxley, viz., the boundary line betwixt *Austro-Columbia* and *Arctogæa*. For it was in this direction apparently, that, during the Miocene epoch, these two great land divisions were separated by that great equinoctial ocean whose currents rolled from eastward beyond and over the present sites of the Sahara deserts and the plains of Hindostan.

As the line of the American Cordilleras was upheaved, the continents more nearly approached each other, an archipelago of detached volcanic summits probably first indicating the future isthmus; whilst the bounds of the ocean were narrowed, and previous to the actual junction but a narrow channel or strait was left. It is supposed that the last indication of this strait is yet observable in the line of the San Juan and the waters drained by it. This theory has received substantial support from the ob-

* See Capt. Pim's "Gate of the Pacific," p. 71.

servations of Mr. Osbert Salvin, the well-known ornithologist, who, from long studying the peculiarities of the Central American bird-fauna, has come to the conclusion that an oceanic separation is plainly indicated as having formerly existed between Costa Rica and the country north of the Nicaraguan lakes. This upheaval has by no means ceased, and the lakes of Managua and Nicaragua, up to which the Spanish galleons proceeded, *viâ* the San Juan, are now 156 and 128 feet respectively above the mean level of the two oceans. So that now with difficulty stern-wheel light-draught steamers, drawing but eighteen inches of water, make their way between the rapids, their cargo having to be shifted across these impediments. A rise of six feet in the waters of the lakes enables bongos to pass the rapids in the wet season.

Every year apparently adds to the difficulties of the navigation, which Mr. Collinson attributes to the continual rise of the Pacific coast. Indeed, it is not improbable, if a careful series of observations were established, that after a lapse of years the rate of rise might be ascertained, which, if compared with seismological observations in the same district, would prove of the utmost value and interest.

It has been before noticed that Greytown is the only settlement of any size on the Caribbean coast, owing to its position at the mouth of the San Juan river, which is the only one which offers facilities for transit across the isthmus; and consequently a portion of the Californian traffic has for some years passed in this channel, an enterprising American company having monopolised the "transit-route." Owing, however, to the rapid silting-up of the embouchure of the San Juan at Greytown, this town would infallibly have lost all its importance, had it not been that the rapid development of marine telegraphy has given rise to a great demand for india-rubber, a valuable kind of which is collected from trees which are numerous in the dense forests of the Central American isthmus, especially on the Atlantic coast.

Greytown is the principal port for the export of india-rubber on the coast. It is collected by parties of Indians, Caribs, or half-caste Creoles, seldom by Europeans, to whom the dealers, who are also storekeepers, advance the necessary outfit of food, clothing, and apparatus for collecting rubber, on condition of receiving the whole of the rubber collected at a certain rate. The rubber hunters are termed *Uleros* (*Ule* being the Creole term for rubber). A party of Uleros, after a final debauch at Greytown, having expended all their remaining cash, generally make a start in a canoe for one of the rivers or streams which abound on the coast, and having fixed on a convenient spot for a camp, commence operations. The experienced rubber hunter marks out all the trees in the neighbourhood. The rubber tree is the *Castilloa elastica*, which grows to a great size, being on an average about four feet in diameter, and from twenty to thirty feet to the first spring of the branches. From all the trees in the almost impenetrable jungle hang numerous trailing parasites, lianes, &c., from these, and especially the tough vines, are made rude ladders, which are suspended close to the trunks of the trees selected, which are now slashed by machetes in diagonal cuts from right to left, so as to meet in the middle in central channels, which lead into iron gutters driven in below, and these again into the wooden pails. The pails are soon full of the white milk, and are emptied into larger tin pans. The milk is next pressed through a sieve, and subsequently coagulated by a judicious application of the juice of a Bejuca (an *Apocyna* ?) vine. The coagulated mass is then pressed by hand, and finally rolled out on a board with a wooden roller. The rubber has now assumed the form of a large pancake, nearly two feet in diameter and about a quarter of an inch thick, on account of which they are termed *tortillas* by the Uleros; these cakes are hung over the side poles and framework which supports the *rancho*, which is erected in the woods, and allowed to dry for about a fortnight, when they are ready to be packed for delivery to the dealer.

In the meantime others of the party go in pursuit of game, such as tapirs or *dantes*, or mountain cows, as they are termed, of which there are several species; or they harpoon the manatee,* which they dexterously follow in their canoes, as it cannot remain under water long. The point of the harpoon used by the Indians is moveable, and, attached to a line and floating reel, it becomes detached from the shaft when the siren is struck. The wild boar or javali (domestic pig run wild?) and the *waree*, or peccary, which are shot in June and July, and the deer, which are shot in December, afford good pork and venison. The waters of all the numerous rivers and lakes are characterised by an astounding number of distinct ichthyological faunæ. The Indians are good fishermen, and will shoot fish in the water by bow and arrow, or cut them down with a machete; the best fish are perhaps the *guapote*, *mojarra*, and *savallo*. By way of feathered game the curassows and guans (*Crax alector*, *C. fasciolata* and several Penelopes) of different species are of good size and flavour, whilst iguanas and land turtle eggs serve to vary the bill of fare of the Ulero gourmet.

The picnic life of the Ulero is not all *couleur de rose*. At night the jaguars and pumas (*Felis onca*, *F. melas* and *F. concolor*) will prowl in the neighbourhood of the *rancho*. These beasts are sometimes brought to bay with dogs by the Carib mahogany cutters in the fork of a low tree, and then speared; the spear in this instance is always provided with a stout cross bar, to prevent the transfixed animal from reaching his assailant.

Besides this the alligators abound in the water, which renders bathing slightly precarious; but as a general rule these brutes are cowardly enough when not hungry. On one occasion one of the party (with whom the author was in these woods) having shot a dante, which sank to the bottom of the River Rama, an Indian dived after it to attach a rope to the carcass: while the alligators, attracted by the smell of blood, surrounded the canoe in a circle of some score yards in diameter, but none of them ventured an attack on the bold diver. Both Caribs and Indians have a profound contempt for the alligator in these rivers. On shore, again, the snakes are numerous, such as the *tboba*, *vipora de sangre*, a long black snake, *Coryphodon constrictor*, the lovely coral, and barber pole snakes, and, worst of all, the small tamagusa or "tommy goff." The Caribs assert the valuable propereies of a vine—a species of Aristolochia—which they declare will allay the effects of a snake bite.

The greatest drawbacks, however, to the enjoyment of Ulero life in Mosquitia and Costa Rica are the swarms of garrapatas or ticks (*Ixodes*), which persecute remorselessly the hunter or woodsman. The *chigoe* or jigger is also another annoyance. By-the-bye, it is said, I do not know on what grounds, that this last-mentioned pest is only to be found where domestic swine are kept. I only know that I have suffered from one in the woods many miles from any domesticated swine. Do they appear therefore where there are wild hog or peccary? There is also a digusting bot fly and swarms of mosquitoes near the water.

The Formicidæ are likewise numerous and formidable; a gigantic black ant which especially pervaded the eboe (*Dipterix oleifera*) trees is justly dreaded, and we always avoided slinging our hammocks from these trees if pos-

* The genus Manatus appears to be the most ubiquitous of the sub order Sirenia, and various species are to be found not only on the rivers inland lakes, and coasts of Tropical America, but along the entire opposite coast of Africa, where the habitat of the *Manatus senegalensis* extends round the Cape, and as far north on the Mozambique coast as the river Zambesi; besides which its presence is recorded in the Lake Shirwa by Dr. Kirk. A species, *M. Vogelii*, also occurs in the upper Niger, and, according to Barth, in Lake Tsad, whilst Heuglin notices one species in the Tana Sea in Abysinia. So it is not improbable that the Manatus may occasionally meet its East Indian congener the *Halicore Dugong*.

sible. Stout Indians will howl and writhe with agony from the effect of their bites. A minute red fire ant also infests the acacia trees, and is barely more endurable. The howling of the black monkeys also is not conducive to sleep when they choose some neighbouring branches for their "serenade." The above slight sketch may serve to give some insight into the pleasures of a country life in the vicinity of Greytown, pleasures, however, of which the Nicaraguan citizens seldom avail themselves.

There have already appeared in NATURE some accounts of peculiar nocturnal vibrations observable in iron vessels off Greytown, which I will not allude to further.

The drawing which accompanies this notice was taken from the pier of the Transit Company's wharf; the town itself is barely visible from this point, and lies beyond the few buildings shown. The remains of one of the flat-bottomed streamers which ascend the river is shown lying by the shore. Canon Kingsley appears to have been disappointed at only twice catching a glimpse of the black fin of a shark during his recent visit to the West Indies; let me recommend the bar of Greytown Harbour and its vicinity as an exceptionally favourable locality for studying these monsters in their native element.

S. P. OLIVER

## *THE DATE OF THE INTERMENT IN THE AURIGNAC CAVE*

IT is a remarkable fact in the history of Archæology that the palæolithic age of the human interments in the cave of Aurignac has been universally accepted without any criticism of the evidence. It has passed into the condition of an article of scientific faith, partly through the eminence of M. Lartet, the describer of the cave, and partly through the high authority of Sir Charles Lyell, who followed his views in the "Antiquity of Man." The ready faith with which it has been received stands in marked contrast to the scepticism which refused to allow the value of the discovery of flint implements in the caves of England and Belgium for more than a quarter of a century, and up to within some three years of M. Lartet's investigations in Aurignac. The importance of examining the data on which M. Lartet's theory is based can hardly be over-estimated in the present state of the science of man. If the human interments really be of the same relative date as the extinct Mammalia found in the cave, and M. Lartet's interpretation of the circumstances be true, then, to quote Sir Charles Lyell, "we have at last succeeded in tracing back the sacred rites of burial, and, more interesting still, a belief in the future state," to the palæolithic age, and we have a powerful argument against the progressive development of religious ideas. This point did not escape Mr. Wallace in his speech at the Exeter meeting of the British Association. If, on the other hand, the interments be not proved to be palæolithic, the sooner an element of error is eliminated from a most difficult problem, the nearer shall we be to its solution. I shall first of all take the facts as they are now universally interpreted; and then I shall check them by the independent evidence of the late Rev. S. W. King, who finally explored the cave.

M. Lartet's account falls naturally into two parts: first that which the original discoverer of the case told him, and secondly that in which he describes the results of his own discoveries. I shall begin with the first. In the year 1852 a labourer named Bonnemaison, employed in mending the roads, put his hand into a rabbit-hole and drew out a human bone, and, having his curiosity excited, he dug down, until, as his story goes, he came to a great slab of rock. Having removed this, he discovered on the other side of it a cavity 7 or 8 feet in height, 10 in width, and 7 in depth, almost full of human bones, which Dr. Amiel, the Mayor of Aurignac, believed to represent at least 17 individuals of all ages. All these human remains were collected, and finally committed to the parish cemetery, where they rest at the present time undisturbed by the sacrilegious hands of archæologists, the discoverer and the sexton being alike ignorant of their last resting-place. Fortunately, however, Bonnemaison, in digging his way into the grotto, had met with the remains of extinct animals and works of art, and these were preserved until, in 1860, M. Lartet heard of the discovery, and resolved to examine the cave for himself. It must be remarked that before his advent the interior had been ransacked, and the original stratification to a great extent disturbed, a circumstance which obviously does away with any argument based on the association of remains in the cave.

M. Lartet's exploration resulted in the discovery that a stratum containing the bones of cave-bear, lion, rhinoceros, and hyæna, along with undisputable works of art of the palæolithic type—like those of the Dordogne—passed from a plateau on the outside into the cave. On the outside he met with ashes and burnt and split bones, which implied that it had been used by the palæolithic hunters as a feasting place; within he detected no traces of charcoal, and no traces of hyænas, which were abundant outside. Inside he met with a few human bones, which were in the same mineral state as those of the extinct Mammalia. That, however, identity of mineral state is any clue to age is disproved by the varying condition of bones of the same geological age in every bone cave with which I am acquainted. As an example I might quote the re-remains of cave-lion in the Taunton Museum. Such is the summary of the facts which M. Lartet discovered. He has, of his personal knowledge, only proved that Aurignac was occupied by a hunter tribe during the palæolithic age.

Is he further justified in assuming that it was used as a sepulchre at that remote period? Bonnemaison's recollections may be estimated at the proper value by the significant fact that, in the short space of eight years intervening between the discovery and the exploration, he had forgotten where the skeletons had been buried. And even if his account be true in the minutest detail, it does not afford a shred of evidence in favour of the cave having been a place of sepulture in palæolothic times, but merely that it had been so used at some time or other. If we turn to the diagram constructed by M. Lartet to illustrate his views (An. des Sc. Nat. Zool. iv. ser. t. xv., pl. 10), and made for the most part from Bonnemaison's recollection, or to the amended diagram given by Sir C. Lyell (Antiquity, fig. 25), we shall see that the skeletons are depicted above the strata containing the palæolithic implements and the quaternary mammals, and therefore, according to the laws of geological evidence, they must have been buried after the subjacent deposit was accumulated. The previous disturbance of the cave earth altogether does away with the value of the conclusion that the few human bones found by M. Lartet are of the same age as the extinct mammalia in the same deposit. The absence of charcoal inside was quite as likely to be due to the obvious fact that a fire kindled inside would fill the grotto with smoke, while outside the palæolithic savages could feast in comparative comfort, as to the view that the ashes are those of funereal feasts in honour of the dead within, held after the slab had been placed at the entrance. The absence of the remains of hyænas from the interior is also negative evidence disproved by subsequent examination.

The researches of the Rev. S. W. King in 1865, hitherto unpublished, complete the case against the current view of the palæolithic character of the interments, insomuch as they show that M. Lartet did not complete the examination which he began; and that he consequently wrote without being in posesssion of all the facts. The entrance was blocked up, according to Bonnemaison, by a slab of stone which, if the measurements of the entrance be correct, must have been at least 9 feet long and 7 feet high, placed, according to M. Lartet, to keep the hyænas from the corpses of the dead. It need hardly be remarked that the access of these bone-eating animals to the cave would be altogether incompatible with the preservation of the human skeletons, had they been buried at the time. The enormous slab was never seen by M. Lartet, and it is very hard to understand how it could have been removed by one workman cutting a trench after a few hours' work. And it certainly did not keep out the hyænas. In the collection made by the Rev. S. W. King from the interior, there are two hyæna's teeth, and nearly all the antlers and bones bear the traces of the gnawing of those animals. The cave, moreover, has *two* entrances instead of one, as M. Lartet supposed, when his paper in the Annales was published. There are also in the collection above quoted —now presented by Mrs. King to the Christy Museum— two metacarpals of sheep or goat—animals which, as yet, have not been proved to have been living in Europe during the quaternary period, and which, probably, were introduced by neolithic races of men, as well as a fragment of pottery of precisely the same kind as that in the superficial deposit in Kent's Hole.

In a word, the evidence in favour of the interment in Aurignac being of a later date than the occupation seems to me to be overwhelming, and it does not afford the slightest ground for any hypothesis as to the belief of palæolithic men in the supernatural. On that point, up to the present time, modern discovery is silent, and negative testimony is valueless. W. BOYD DAWKINS

## *DAYLIGHT AURORAS*

WE have published several letters lately on this subject, in some of which doubts are suggested as to the reality of the phenomenon. The following extracts from a paper which we have received from Mr. Glaisher, will put the matter to rest :—

*The Aurora of Feburary 12, appearing in Daylight.*

"The accounts of auroræ appearing by daylight are very few indeed, yet the following reports made by two of the observers in the magnetic department of the Royal Observatory, Greenwich, who called my attention to the appearance of the sky and to the fixity of the arch, as well as to the apparent avoidance by the clouds of the clear space, together with the disturbed state of the magnetic elements at the time, seem to decide that the appearances were really due to an aurora appearing by daylight.

"Mr. Wright says that at about noon the clouds in the north began to break, and soon after an almost perfect arch of clear sky, with its apex in the magnetic meridian, was visible. This space of clear sky kept its shape more or less perfect for more than an hour—a remarkable fact, as the clouds in the remaining portion of the sky were being driven rapidly across by a strong N.E. wind. The clouds immediately above the top of the arch seemed to be charged with electricity, the edges assuming the ragged appearance common to thunder-clouds. At times these clouds were slightly tinged with a reddish colour. About $0^h 45^m$ P.M., a very remarkable cloud of a reddish-brown colour passed slowly across the clear space from E. to W., being apparently much nearer to the observer than the ordinary clouds. Apart from the ordinary motion of the clouds from N.E. to S.W., caused by the wind, there seemed to be an apparent vibratory motion from E. to W.

"Mr. Marriott says :—'About noon the clouds in the north began to break, and shortly after, there was a perfectly clear space of blue sky in the form of an arch, the apex of the arch being in the magnetic meridian. At the circumference of the arch were very fine cumulus clouds, the edges of which were tinged with a reddish colour; and along the whole of the north horizon there stretched a bank of cumulus clouds to the altitude of 10° or 15°. At about $0^h 20^m$, just below the apex of the arch, I observed something like steam shooting up and moving from east to west ; this, I imagine, is what streamers would be like in the daytime. At $0^h 45^m$ a small cloud of a brick-red colour traversed the clear space ; a few other clouds which passed over at the same time were not tinged. The arch was very well defined for about an hour or an hour and a half; and although the wind was blowing a gale from the north-east, and the clouds passing rapidly over the other portions of the sky, this space was not encroached upon by clouds. The altitude of the arch was about 50°, and the point at which the supposed streamers first appeared was about 7° below the apex. I also observed auroral light at night.

"On the 11th day and till $6^h 35^m$ P.M. the movements of the several magnets were those of the ordinary diurnal changes, and at this time the western declination was 19° 55′. At $6^h 40^m$ a sudden disturbance began ; the declination decreased 19′ by $7^h 19^m$, then increased to 19° 56′ by $8^h$; at $8^h 12^m$ it was 19° 47′, increased to 20° 4′ by $8^h 29^m$, was 19° 45′ at $9^h 11^m$, was 20° 6′ by $9^h 58^m$ P.M., then there were several small movements of 3′ or 4′ both to the east and to the west ; at $11^h 30^m$ the declination was 19° 58′, and by midnight had increased to 20° 12′.

"The magnet still continued to move through small arcs, but gradually decreasing to $4^h 10^m$ A.M. on the 12th. to 19° 47′ ; then there were frequent changes of position, but such that the declination generally increased, and was 20° 3′ at $8^h 45^m$ A.M. ; by $10^h 40^m$ it decreased to 19° 55′.

"There were frequent movements of the magnets between this time and till after noon. On the 12th day, at $0^h 30^m$ P.M., the declination was 20° 12′, at $0^h 45^m$ it was 20° 3′ ; this movement of the magnet towards the east is remarkable as having taken place immediately before the passage of the reddish-coloured cloud from east to west across the clear space of sky, and attained its maximum at about the time of the passage of the cloud. The movement of this cloud was not that of all other clouds, viz. from N.E. to S.W., and it would seem to be of auroral origin. . . . .

"Authentic instances of auroral displays by daylight are very few.

"The first instance I can find is recorded at p. 189, vol. ii. of the 'Transactions of the Royal Irish Academy,' from which the following extract is made :—

"'An account of an Aurora Borealis seen in full Sunshine. By the Rev. Henry Ussher, D.D., F.R.S., and M.R.I.A.

"'On Saturday night, May 24, 1788, there was a very bright aurora borealis, the coruscating rays of which united, as usual, in the pole of the dipping-needle. The next morning, about 11, finding the stars flutter much, I examined the state of the sky, and saw whitish rays ascending from every part of the horizon, all tending to the pole of the dipping-needle, where at their union they formed a small thin and white canopy, similar to the luminous one exhibited by an aurora in the night. These rays coruscated or shivered from the horizon to their point of union.'

"The only other account is extracted from the 5th vol. of the 'Transactions of the Royal Society of Edinburgh,' and is as follows :—

"'An account of an Aurora Borealis observed in daylight at Aberfoyle, in Perthshire, on the 10th of February, 1799. By Patrick Graham, D.D., minister of Aberfoyle.

"'On the 10th of February, 1799, about half an hour past 3 o'clock P.M., the sun being then a full hour above the horizon, and shining with an obscure lustre through a leaden-coloured atmosphere, I observed,' says Dr. Graham, 'the rare phenomenon of an aurora borealis by daylight. The weather for several days before had been intensely cold, and during the two preceding days much snow had fallen. On this day a thaw had come on, and the temperature of the air was mild. The general aspect of the sky was serene. Some dark clouds hung on the horizon between S.W. and W. I was intensely observing a large halo about the sun, of about 20° in semi-diameter. It exhibited the prismatic colours, though obscurely, except in one quarter, where it coincided with the skirt of a dark cloud on the horizon, almost directly west. In that portion of the halo the colours of the iris were very distinctly exhibited.

"'Whilst I was attending to this appearance, the whole visible hemisphere of the heavens became covered with a light palish vapour, as I at first imagined it to be. It was disposed in longitudinal streaks, extending from the west, by the zenith, and all along the sky towards the east. On examining this appearance more narrowly, I found it to be a true aurora borealis, with all the characters which distinguish that meteor when seen by night, excepting that it was now entirely pale and colourless. The stream of electric matter issued very peceptibly from the cloud in the west, on the skirts of which the halo exhibited the prismatic colours; thence diffusing themselves, the rays converged towards the zenith, and diverged again towards every quarter of the horizon; and the coruscations were equally instantaneous, and as distinctly perceptible as they are by night.

"'This appearance continued for more than twenty minutes, when it gradually vanished, giving place to thin scattered vapours, which, towards sunset, began to overspread the sky. Through the ensuing night, I could not discern the smallest trace of these meteors in the sky.'"

## *NOTES*

OUR readers will learn from another column that an appeal is about to be made to Government to aid another Eclipse Expedition, this time a very small one. Seeing that another so favourable opportunity will not occur for some time, it is to be hoped that the Government will respond to the call, and deserve as hearty thanks from all lovers of scientific progress as it earned for its efforts last year.

THE American Association for the Advancement of Science will be opened at Indianopolis, Indiana, on August 17. The president for this meeting is Prof. Asa Gray.

IT is with great regret that we have to record the death of Mr. Alexander Keith Johnston, LL.D., to whose eminent services in the promotion of meteorological and physico-geographical science we had occasion to refer but a few weeks since on the occasion of the medal awarded him by the Royal Geographical Society. Dr. Johnston was president-elect of the geographical section of the British Association at its approaching meeting at Edinburgh. He died on Sunday last, at Ben Rhydding, in Yorkshire.

THE Natural History Society of Montreal, with the aid of the Government of Canada, is sending an expedition to dredge in the deeper parts of the Gulf of St. Lawrence. The Hon. Mr. Mitchell, Minister of Marine and Fisheries, has taken much interest in the matter, and has placed the government schooner *La Canadienne* at the disposal of the party. The gentlemen selected are Mr. Whiteaves, F.G.S., secretary of the Society, and Mr. G. F. Kennedy, B.A. Principal Dawson, the president of the Society, sends the latter gentleman on behalf of the museum of M'Gill University. It is hoped that the deepest parts of the gulf will be searched, and that much interesting information will be obtained, bearing both on zoological and geological questions, and also on the prosecution of the fisheries.

MR. W. S. ALDIS, of Trinity College, Cambridge, Senior Wrangler in 1861, has been appointed Professor of Mathematics at the College of Physical Science at Newcastle-on-Tyne.

WE are enabled to state that the scheme proposed for the institution of the Sharpey Scholarship at University College, London, has been adopted by the Council. Its principal features are that the scholarship may be held for three or a greater number of years, and that the holder of it shall act as an assistant to the Professor of Practical Physiology, having opportunities afforded to him of pursuing original investigations, and having the right to use the laboratory and its apparatus for that purpose.

THE Brown Institution which has just been founded by the University of London, will comprise, in addition to a hospital for the treatment of animals, a laboratory for the study of pathology on the model of the Pathological Institutes of Germany, which have been already described in these columns. In this laboratory those who desire to learn the methods of exact research, or, after having learnt them, to carry out pathological or therapeutical investigations of their own, will have the opportunity of doing so under the guidance of the new Brown Professor, Dr. Burdon Sanderson. As we before announced, Dr. E. Klein, of Vienna, is expected to have the direction of the microscopical work of the laboratory, for which his numerous researches show him to be so pre-eminently fitted.

WE have to record the death of Mr. George Tate, of Alnwick, Hon. Secretary of the Berwickshire Naturalists' Club, which took place on June 7, at the age of 66. His treatises on the archæology of his native borough and county entitle him to take rank among the best of local historians; and his articles on Archæology and Geology, published in the "Transactions of the Berwickshire Naturalists' Club," show powers of observation and clear habits of thought of no ordinary kind.

A REPORT on the progress and condition of the Royal Gardens at Kew during the year 1870 has just been issued by the director, Dr. J. D. Hooker. The number of visitors was not quite so large in 1870 as in 1869. The improvements in the laying out of the grounds of the Botanic Gardens, which have been in progress for the last five years, are now nearly brought to a close. The pleasure grounds have suffered severely from the long and severe drought of last summer, acting on the excessively poor natural soil; very large numbers of trees have perished, especially the older elms, ashes, beeches, and sycamores. These are being replaced, and preparations have been made for the formation of the new Pinetum, which will be immediately commenced. Notwithstanding the rage for planting Conifers which has prevailed in England for many years, and which has almost supplanted the growth of hardy deciduous trees, no complete public, arranged, and named collection of hardy conifers exists in this country. The interchange of living plants and seeds with foreign and colonial botanic gardens has been vigorously prosecuted, especial attention having been paid to the promotion of the growth of the cinchona, and the introduction of the ipecacuanha into our Indian possessions. The museums, herbarium, and library have been enriched by numerous purchases and donations.

AT a recent meeting of the Scientific Committee of the Horticultural Society, Mr. Andrew Murray read a paper on the blight of plants, in which he combated the ordinary theory that the lower forms of vegetable organisms, which constitute ordinary blight, are developed from germs existing in the plant or floating in the air. The extraordinary rapidity of their propagation, frequently after a few hours' east wind, when no trace of them has been visible for many months, the prodigious numbers in which they appear, and the great variety of species developed sometimes on the same plant, and other considerations, have led him to the conclusion that these lowly organised fungi are evolved out of previously-existing organic materials, without the intervention of a germ, by the process erroneously called spontaneous generation.

A VERY interesting collection of paintings is now on view at the Langham Hotel, Portland Place, being delineations of Arctic scenery, by Mr. William Bradford, of New York. In company with Dr. J. D. Hayes, Mr. Bradford spent four months of the summer of 1869 in an expedition to the coasts of Labrador, Greenland, Melville Bay, &c., for the express purpose of studying the pictorial effects of Arctic scenery. A very large number of photographs were taken, as well as many sketches, from which the finished paintings were afterwards completed. The collection is, therefore, unique of its kind. Among the most striking of the paintings is one representing sunset among the icebergs.

THE Geologists' Association organised excursions of its members to Ilford on the 17th of June, and to Riddlesdown on the 1st of July. In the former the chief objects of attraction were the famous mammaliferous brick-pits of Ilford, to which Mr. Henry Woodward acted as cicerone. Mr. Woodward and Mr. Searles V. Wood consider the Ilford beds to be older than those at Grays. The distribution of the fossils is remarkably different; *Elephas primigenius*, for instance, being the common species at Ilford, and *E. antiquus* at Grays. The party were afterwards kindly invited by Sir Antonio Brady to inspect his magnificent collection of mammalian remains. The excursion to Riddlesdown gave a good opportunity for examining the sections of the Upper Chalk, and the sequence of the formations of the Cretaceous system. This was the last excursion of the season.

THE "Working Men's Club and Institute Union" has just issued a paper recommending the establishment of classes at each institution for the study of one or more of such branches of Natural History as Botany, Geology, and Entomology, according to the circumstances of the several localities. It is proposed that these classes shall on Saturday afternoons sally forth into the fields and woods for the collection of specimens illustrating the particular subjects of their studies. With the view of encouraging such pursuits, a member of the Council of the Union offers two prizes of three and two guineas respectively to the best collection made during the present season by members of workmen's clubs. It is hoped that this suggestion may lead to the formation of museums of natural history at the clubs—the contents being collected and arranged by the members. The adoption of such pursuits in leisure hours will not only be productive of much mutual enjoyment to the working people of this country, but afford a powerful argument for the more general adoption of the Saturday half-holiday by employers.

THE Leicester Literary and Philosophical Society has recently revived its old custom of instituting geological excursions to some of the many objects of interest in the county. One of these took place last month under the guidance of the veteran geologist, Mr. J. Plant, and was an eminently successful one.

THE Liverpool Naturalists' Field Club has issued its Report of Proceedings for the Session 1870-71. The address of the president, the Rev. H. H. Higgins, refers chiefly to the interesting palæontological discoveries made during the last two years in the neighbourhood of Liverpool, and is illustrated by a plate of fossils from the Ravenhead Collection in the Free Public Museum. An epitome is given of the results of each of the summer excursions and of the papers read at the evening meetings, including one on the microscopic structure of the plants of the Coal Measures, by Prof. Williamson. A unique feature of this Society is that at each Field Meeting five prizes are competed for, for the best flowers gathered or collected during the excursion. We are glad to see the Report published at so low a price as one shilling, or to members, sixpence, and commend this laudable practice to the notice of other similar societies.

THE following schools have been invited by the Royal Geographical Society to take part in the competition for prize medals for the ensuing year:—English Schools: St. Peter's College, Radley, Abingdon; King Edward's School, Birmingham; Brighton College; Cathedral Grammar School, Chester; Cheltenham College; Clifton College; Dulwich College; Eton College; Haileybury College; Harrow; Hurstpierpoint; Liverpool College; Liverpool Institute. London: Charter House; Christ's Hospital; City of London School; King's College School; St. Paul's; University College School; Westminster School; Royal Naval School, New Cross. Manchester School; Marlborough College; University School, Nottingham; Repton; Rossall; Rugby; King's School, Sherborne; Shoreham; Shrewsbury; Stonyhurst College, Blackburn; Uppingham School; Wellington College; Winchester School. Scotch Schools: Aberdeen Grammar School; Edinburgh Academy; Edinburgh High School; Glasgow High School. Irish Schools: Royal Academical Institute, Belfast; Dungannon Royal School; Ennis College; Portora Royal School, Enniskillen; Foyle College, Londonderry; Rathfarnham, St. Columba's College.

THE part of the "Proceedings of the Geologists' Association" just published contains an interesting article by Mr. H. Woodward "On Volcanoes," and reports of the excursions made during 1870.

THE last number of Petermann's "Mittheilungen" contains an admirable physical map of the region covered by Hayward's journey from Leh to Kaschgar in 1868-69.

UNDER the title "The Geographical Distribution of Sea-grasses," Dr. P. Ascherson gives an account in Petermann's "Mittheilungen," of the distribution of the phænogamous plants native to sea-water. Of these he enumerates twenty-two, belonging to eight genera, and two natural orders. The area of each species is generally very limited, its distribution being dependent on the present condition of the sea in which it is found. Those which grow in temperate regions are frequently represented by closely allied species in tropical seas. Although the Isthmus of Suez is of comparatively modern geological date, the nine species of the Red Sea are entirely distinct from the four species of the Mediterranean, and, with one exception, belong to different genera. A map accompanies the paper.

WE have on our table the *Astronomical Register* for June, and have much pleasure in calling the attention of astronomers to this magazine, which is rapidly improving in usefulness.

THE December number of the *Canadian Entomologist* concludes the second volume. It is intended to be increased on the commencement of the third volume, without any corresponding increase of subscription, to twenty pages each number, and will remain under the editorship of the Rev. C. J. S. Bethune.

THE volume of lectures delivered at the Industrial and Technological Museum, Melbourne, during the spring session of 1870, shows great activity in scientific matters in Victoria. Among the subjects discussed are the Circulation of the Blood, the Conservation of Energy, the Application of Phytology to the Industrial Purposes of Life, Chemistry applied to Manufactures and Agriculture, the Preservation of Food, the common Uses of Astronomy, and On Methods of Diffusing Technological Knowledge.

WE have before us the number for May of the "Journal of the Franklin Institute," containing several valuable articles. We may notice in particular the continuation of a series on "Iron Manufactures in Great Britain," by Mr. R. H. Thurston, and "A Method of Fixing, Photographing, and Exhibiting the Magnetic Spectra," by Dr. A. M. Mayer.

WE have received the first volume of an important continental flora, "Flora der preussischen Rheinlande," by Dr. P. H. Wirtgen, including as far as the end of Thalamifloræ. Descriptions of each species are given, with physiological and morphological annotations, and a copious list of localities of the less abundant species. Independently of its scientific value, the book will be very useful to the numerous summer visitors to that district.

A NEW port has been opened in Southern Chile in the Department of Constitucion. It is called Curanipe, and it appears that already the population is 1,186, and the tonnage in and out 7,867 in 1870.

ON May 11 two distinct shocks of earthquake were felt at Peshawur, in India.

ON May 22 an earthquake was felt at Landour, Meerut, Agra, and Nynee Tal. At the latter place it was severe.

AN earthquake was felt at Hayti on May 30.

ON June 16 a severe storm assailed Constantinople. During its height three waterspouts swept across different parts of the Bosphorus in great volume and with unusual fury. By one of them a caique was destroyed. The lightning struck the lightning-conductor on the great Gulata Tower in Pera, and also the wire at the Observatory connecting it with the arsenal at Tophaneh. On the other side of the Bosphorus, at Scutari, a house was struck.

A REPORT has been published in the Hong Kong press of March 25 by Captain Frost, of the Noord Brabant. He says he sighted Tinakoro, or Volcano Island, one of the Santa Cruz group, in lat. 10, 23 S., 155 long. E., and lay becalmed there five days. The island is a cone of perfect symmetry, resting on a base of three miles in circumference, and, except about the base, destitute of vegetation. The volcano, estimated to be about 2,500 feet high, was in constant activity, presenting the appearance of a great flame vent. Captain Frost denies the description of Captain Wilson, of the *Duff*, that there are several low islands there, at least on its south and west quarters, about seventeen miles off.

A REPORT has been sent in by the Governor of the Province of Leon in Ecuador as to the condition of the volcanic region of Cotopaxi in his province. He states that the principal mountains which stand forth in the great circle formed by the two branches of the Andes are Cotopaxi, Quillindana, Puchalagua, and the Calpon. Of these Cotopaxi alone is known as a volcano, which after many years of inaction became active in June 1851. These eruptions continued and became gradually weaker until 1867, when they ceased. In 1868 subterranean noises were again heard, and a slender column of smoke appeared. In May 1868 there were some earthquakes, which ruined Palate and Pelileo. In July 1869 noises were again heard and an awful flood took place, but without earthquakes and subterranean noises. Abundant fountains of water burst forth, hundreds of immense rocks were rent and thrown down, and the rivers were flooded. The Governor, who was at the time in the Cordillera, considers that the landslides were not owing to the action of water, but rather to a pressure upward from below, as if from accumulated gases seeking an exit. The most curious effect reported by him is a variation in the climate. Many plants, such as the sura, flowered, which had not done so before. After this premature ripening the surales all closed up again, and have not revived. After this event it was noticed the sugar cane could be cut in twenty-four months instead of thirty. At present Cotopaxi is inactive, but its condition is looked upon with dread.

FROM the *Australasian* of April 22, we learn that Mr. Russell, the Government astronomer at Sydney, has visited Deniliquin and picked up there something which astonished him, in the shape of the greater portion of a meteoric stone which fell some years ago at Barratta, thirty-five miles below Deniliquin. The stone (Mr. Russell secured about one half of it, weighing about 150lb.) was originally about 300lb. in weight, but has been broken, and parts of it given away as curiosities. Mr Russell made provision for despatching the stone to the Sydney Museum.

## SCIENTIFIC INTELLIGENCE FROM AMERICA *

AT a recent meeting of the New York Lyceum of Natural History, Professor D. S. Martin described the remarkable deposit of magnetic iron at Cornwall, Pennsylvania, and exhibited the group of minerals found in connection with the iron. The ore is a soft, often pulverulent magnetite, associated with copper, and often pyrites. It is found in three hills which owe their relief to the erosion of their surroundings, and are composed mainly of iron ore embraced between walls of trap, the whole mass lying at the junction of the Triassic red sandstone and older metamorphic series. The yield of the Cornwall mines is 160,000 tons per annum. Prof. Martin exhibited beautiful specimens of allophane, brochantite, and other minerals collected at Cornwall. —Prof. Newberry, at the same meeting, exhibited a series of lignites from the Far West, with ultimate analyses of each. He said these modern coals were the only mineral fuels found west of Omaha. The Los Brances (Sonora) coal is Triassic anthracite. Most of the New Mexico and Arizona coals are Cretaceous, the beds sometimes thirty feet in thickness. The Placer Mountain coal is a Cretaceous anthracite. The coal of Colorado is both Cretaceous and Tertiary; the coal of Mount Diabolo, California, is Cretaceous; and that of Vancouver Island, Coose Bay coal, is Tertiary. Alaska furnishes some of the best Western coal—a Tertiary lignite. A Cretaceous anthracite found in Queen Charlotte's Island is nearly as good as that of Pennsylvania. All these anthracites are caused by volcanic action baking lignites. The calorific power of the Western coals is generally greatly impaired by the large percentage (ten to twenty per cent. each) of oxygen and water they contain. The average Western lignite has about half the heating power of our best coals. The gas and coke made of some of them, however, are excellent furnace fuels, though they are generally worthless.—Prof. Davidson, of the United States Coast Survey, has lately devised an apparatus for recording the temperature at different depths by means of an electro thermal pile. He proposes to register the depth by breaking the circuit of an electric current passing through two insulated wires in the sounding line at about every one hundred fathoms by means of the wheel-work of the Massey or similar apparatus. In the changes of temperature an electro-thermal pile eighteen inches long, insulated, surrounded by a non-conductor except at one end, is used in combination with a Thompson's reflecting galvanometer, not liable to derangement on shipboard. At every one hundred fathoms, when the chronograph registers the depth, the observer notices the readings of the galvanometer, which readings are reduced to Fahrenheit degrees. —One of the most original and important contributions to the zoology of the day is that constituting the third number of the Bulletin of the Museum of Comparative Zoology at Cambridge, treating upon the mammals and winter birds of East Florida. The author, Mr. J. A. Allen, an assistant of Prof. Agassiz, is well known for the thoroughness of his research into the vertebrata of America, and the critical attention paid by him to the proper limitation of species, both in their relationships to each other, and in their geographical distribution. In the present work he gives a summary of the views to which he has been led within a few years past by his studies of the immense collection in the Cambridge Museum, and makes numerous important generalisations. Among these he corroborates the conclusion previously announced by others, of the diminution in size of the American birds in proportion as their birthplace is more southern, and also that there is a similar difference existing between the animals of the higher and lower altitudes. He also finds that with the more southern locality of summer abode there are corresponding differences in colour and proportion, as well as in habits, notes, and song, the vivacity of the bird decreasing as its size increases. The principal difference in colour with the more southern localities consists in the darker tints and the reduced extent of any white markings, with other features that our space will not permit us to give at the present time. The entire work is one eminently worthy of careful study, and is destined to exercise a very important influence upon the methods of zoological research. — Late advices from Prof. Hayden's expedition announced that he was to leave Ogden, Utah, on June 9 for Virginia City and Fort Ellis, in Montana, a distance of about 430 miles, with the special object of proceeding from the last-mentioned place to the exploration of the Yellow Stone Lake and its immediate vicinity. It

* Contributed by the Scientific Editor of *Harper's Weekly*.

is an interesting fact that the head waters of tributaries of the Columbia, the Colorado, the Missouri, and the Yellow Stone rivers rise within a short distance of each other in this mysterious region; which, in addition, is characterised by the extraordinary development of hot springs, spouting geysers, mud volcanoes, extensive beds of sulphur, gypsum, the silicates, &c. The party, as at present organised, embraces thirty-two persons, including specialists in all branches of science, and accompanied by several artists, who take advantage of Dr. Hayden's protection to visit the interesting region referred to. The party carries materials for a boat, which is to be launched on the Yellow Stone Lake, and used in a thorough hydrographical and topographical survey of it. As the expedition will probably remain in that vicinity during the summer, we may hope for a complete solution of all the remaining questions in regard to its physical features and natural history. A competent photographer with the expedition expects to make instantaneous views of the spouting geysers, so as to enable those who cannot visit the locality to have a correct idea of their character. A company of cavalry will escort the expedition into the Yellow Stone Lake region, although no trouble from the Indians is anticipated. In the course of the journey from Ogden to Fort Ellis it is proposed to make an accurate map of a belt fifty miles wide, so as to furnish a basis for reference in subsequent explorations.—In the monthly report of the Department of Agriculture for March and April of the present year, we find a valuable paper upon the cultivation of the Cinchona in Jamaica, by Dr. C. C. Parry, the botanist of the Department, who accompanied the San Domingo Investigating Committee, and in returning spent some time in Jamaica. As the general result of his inquiries in regard to the cultivation of this plant, and the possibility of introducing it into any portion of the United States, he states, first, that the peculiar conditions of soil and climate suitable for the growth of the best varieties of cinchona plants cannot be found within the present limits of the United States, where no suitable elevations possessing an equable, moist, cool climate, free from frost, can be met with; second, that the island of San Domingo, located within the tropics, and traversed by extensive mountain ranges attaining elevations of over 6000 feet above the sea, presents a larger scope of country especially adapted to the growth of cinchonas than any other insular region in the western hemisphere; third, that the existence of successful cinchona plantations in Jamaica within two days' sail from San Domingo, would afford the material for stocking new plantations in the latter island at the least possible expense of time and labour.—In a recent communication to the Academy of Natural Sciences of Philadelphia, by Prof. Leidy, attention was invited to certain teeth of fossil mammals, forwarded to him for examination by Prof. Whitney. One of these was a fragment belonging to the *Mastodon americanus*, obtained from a depth of eighty feet beneath the basaltic lava of Table Mountain, Tuolumne County, California, where it was found associated with the remains of human art. There was also a molar of a large fossil horse, found sixteen feet below the surface on Gordon Gulch. Two other teeth, somewhat similar in character, were determined as belonging to the species of *Protohippus*. In other specimens Dr. Leidy found evidences of the existence of a gigantic animal of the camel tribe, allied to the llama.

## *CORRESPONDENCE OF NORTHERN AND SOUTHERN AURORÆ*

I TAKE the liberty of sending you a paper containing corresponding observations of Aurora Borealis and Australis, with the request to insert them in your valuable journal.

*Corresponding Observations of Aurora Polaris, made in the Northern and Southern Hemispheres.*

In the years 1859-65 I kept up a correspondence with the active director of the Flagstaff Observatory at Melbourne, (Australia), Mr. George Neumayer, in order to make observations concerning the contemporaneous appearance of aurora polaris in the northern and southern hemispheres.*

* See Results of the magnetical, nautical, and meteorological observations made at the Flagstaff Observatory, Melbourne, and at various stations in the colony of Victoria, Melbourne, 1860. His, "Wochenschrift für Astronomie und Meteorologie," 1859, 1860, 1861, 1863, 1865.

Some years since, when Dr. Neumayer returned to his native country, this correspondence was interrupted. But the numerous appearances of aurora borealis which occurred last year, induced me to recommence this correspondence with the present director of the same establishment, Mr. C. Moerlin. Sending him a list of all the appearances of aurora borealis and magnetical disturbances in the year 1870 known to me, I begged him to favour me with the corresponding observations viewed by him. I subjoin the answer of Mr. Moerlin.

I received your letter of December 2, 1870, and in reply shall be most happy to comply with your request, of informing you periodically of the occurrence of the aurora australis, and of magnetic disturbances observed here.

To this end I have made out a list, which is enclosed, of auroræ observed since January 1, 1870, containing the dates and times (Melbourne mean time) of their occurrence, from which it appears that at most of the dates you mention in your letter, as having observed the aurora borealis, the aurora australis has been observed here. The greatest magnetic disturbances occurred on April 5 and October 25; on the latter day the disturbances continued during two days; the minimum of easterly declination occurred about 5 A.M. on the 26th, and the maximum about 6 A.M. on the same day, the range being 51′ of arc, with corresponding disturbances in the other two elements. Unfortunately the sky was completely overcast during the night, with a slight break only at midnight, when the display was very beautiful, but visible only for a few minutes; but during the evening of the 25th an intense, but ever varying, luminosity only of the whole southern sky was the sole indication of aurora.

I would remark that at all the dates on which auroræ were observed, magnetic disturbances invariably took place of a greater or less extent; but disturbances occurred also at other times, of the very same nature as took place generally during aurora displays, on which, however, no auroræ were observed. These dates I give you enclosed also, separately, as these may be of interest to you in connection with the possible occurrences of the aurora borealis on one or the other of those dates.

I shall continue from this date to send you periodical notice of the occurrence of the aurora australis and magnetic disturbances at Melbourne, and shall be happy to furnish any information respecting physical phenomena, which you may desire, and I may be able to give. C. MOERLIN

Melbourne Observatory, Feb. 7

Date and time of occurrence of the Aurora Australis observed at Melbourne during the period from January 1, 1870, to February 21, 1871, during which, at the same time, great disturbances in the magnetic elements generally took place.

LAT. 37° 49′ 53·5″ S. LONG. $9^h\ 39^m\ 54{\cdot}8^s$ E.

1870, January 8.—During the evening the aurora was seen at Adelaide, South Australia, as reported by Mr. Food, Superintendent of Electric Telegraph.

February 1.—A fine display between 8 and 10 P.M.; shortly after nine some magnificent streamers.

April 5.—Became visible shortly after $7^h$ P.M., and lasted all through the evening and night. The display at times was most brilliant, particularly at $10^h\ 30^m$ P.M., and again at $12^h\ 30^m$. Slight disturbances in the magnetic elements occurred during the afternoon, which increased shortly before $7^h$ P.M. At $10^h\ 45^m$ P.M. a rapid decrease of easterly declination and increase of horizontal force took place, which lasted until a few minutes before $11^h$ P.M., when both elements as rapidly returned to their former state. Comparatively slight disturbances until $12^h\ 30^m$, when a similar movement to the above mentioned took place, but to a smaller extent. The minimum of easterly declination took place a few minutes before $11^h$ P.M., and the maximum at 10 minutes before $6^h$ A.M. on the 6th, and the range of the disturbance amounted to about 54′ of arc, while the range in the horizontal force was 0·06273 of the absolute (English) unit, = 0·02892 Continental unit.

May 20.—Faint display, most distinct at $10^h\ 30^m$ P.M.

August 22.—At $6^h\ 40^m$ P.M. some fine streamers visible, but not for long.

September 21.—Visible from about $6^h$ to $8^h$ P.M., but not very brilliant; 24, visible from shortly before $9^h$ P.M.; the finest display took place $11^h$ P.M.; 25, traces visible during evening in S.S.E.; 30, traces visible during evening, S.E.

October 21.—Visible during the evening, at $10^h\ 30^m$ P.M., some fine streamers 30° and 40° high; 25, visible at times during the evening, though completely overcast, as a luminous sheet, extending from S.W. to S.E.; 26, shortly after midnight a beautiful display, though cloudy.

November 9.—Visible shortly after midnight until early morning, again during the whole evening; fine red streamers visible through bright moonlight; 15, auroral light visible during the evening, but no streamers; 17, visible at $9^h\ 30^m$ P.M., for a short time; 18, visible all through the evening; 19, visible all through the evening; at $9^h\ 20^m$ P.M. very fine streamers; 20, visible from $11^h$ P.M.; at 10 minutes past midnight, a fine display, with streamers extending from S.E. to S.W. At $4^h$ A.M. on the 21st the whole extent of the southern sky, from the horizon upwards, was illuminated by a reddish light, terminating in something resembling a corona, but no streamers at all were visible; a thunderstorm occurred towards daylight, and the whole appearance vanished instantaneously at $4^h\ 40^m$ A.M, when a terrific thunderclap occurred; 23, visible between $11^h$ P.M. and midnight; 24, traces visible all through the evening; 25, traces visible all through the evening; 29, visible from $8^h\ 30^m$ to $10^h$ P.M., but not brilliant.

December 10—Faint streamers visible all through the evening; 16, visible all through the evening, at $10^h$ P.M. very fine streamers, and at intervals, up to $2^h$ A.M. on the 17th, a very fine display; 17, visible during the evening, some fine streamers at $9^h$ P.M.

1871, January 3.—Visible during the evening; 13, visible after $11^h$ P.M, no streamers, but strong reddish light in S.S.W.; 15, at midnight, faintly visible; 20, visible during evening, but only faint; 21, visible during evening, but only faint.

February 12, visible for a short time at $9^h$ P.M.

List of dates when great disturbances in the magnetic elements took place, of the same nature as during auroral displays, but when no auroras were visible, or at least observed:

1870: January 3, 4; February 10, 11; March 20, 21; April 22, 23, 28; May 16; June 13, 14, 16, 17; July 5, 28; August 3, 7, 19, 20, 21, 23; September 4, 5, 6, 7, 8, 16, 18, 26, 27; October 1, 15, 24; November 10, 22, 27; December 5, 6, 7, 9, 11, 15, 22, 23, 25, 27. 1871: January 5, 6, 10, 27, 28, 30; February 4, 5, 9, 13, 14, 15.

We add to the above-mentioned auroræ australes and magnetical disturbances observed in Melbourne the following ones observed in our own hemisphere:

1870, January.—To the aurora on January 8, at Melbourne, corresponds the aurora borealis on 8th at Oxford, Liverpool, Cockermouth, and North Shields. To the magnetic disturbances on January 3 and 4 correspond the disturbances observed on the same days at Rome; on January 3 auroræ boreales were observed in Piedmont and in France; also in England at Guernsey, Worthing, Royston, Norwich, Boston, Eccles, and Culloden. Aurora borealis visible on the 4th in England at Wisbech.

February.—To the aurora australis visible on February 1, from 8 to $10^h$, at Melbourne, correspond the aurora borealis seen at many places of the Europe on the same day, at Münster, Munich, Ruhrort, Nevtomysl, Peckeloh, Lennep, at Upsala ($5^h\ 50^m$ to $13^h$), also at Cœslin, Petersburg, Königsberg, Paris, London, Calais, Cracow, Stockholm, and in England at Eastbourne, Royston, Little Wratting, Norwich, Wisbech, Boston, North Shields, and Culloden. To the magnetic disturbances on 11th at Melbourne correspond the aurora borealis observed on the same day at Upsala, and in England at Taunton, Wilton, Streatley, Cardington, York, Hawsker, North Shields.

March.—To the magnetic disturbances at Melbourne on the 21st correspond the magnetic disturbances at Rome on the 22nd and the aurora borealis in England at Little Wratting, Stonyhurst, and York.

April.—To the aurora australis on April 5, at Melbourne, correspond the aurora australis observed at many places of Europe on the same day, at Münster, Peckeloh, Lennep, Bonn, Linzig, Dülken, Brunswick, Niederorschel, Stettin, Kurnik, Munich, Feldkirch, Wolgast, Berlin, France and Italy, Paris, Austria, Athens, at Upsala, Petersburg, Riga, Pulbus, and Stockholm. To the magnetic disturbance on April 23 correspond the magnetic disturbances at Rome, and the aurora borealis at Papenburg on the same day.

May.—To the aurora australis on the 20th at Melbourne corresponds the very fine aurora borealis at Münster, which also was seen on the same day at Mannheim, Paris, and London, and the great magnetic disturbances visible in Rome and Munich.

June.—To the magnetic disturbances in Melbourne on the 13, 14, 16, 17 correspond the magnetic disturbances at Rome on the same days. (Bulletino Meteorologico dell' Osservatorio del Collegio Romano, No. 7, vol x.)

July.—To the magnetic disturbances on July 8 and 28 at Melbourne correspond the disturbances at Rome on the same days.

August.—To the aurora australis on the 22nd at Melbourne corresponds the aurora borealis on the 21st at Volpeglino near Tortona in Italy. To the magnetic disturbances in Melbourne on the 3, 7, 19, 20, 21, 23 correspond the contemporary disturbances of the magnetic instruments at Rome. With the magnetic disturbances on the 7th the aurora borealis at Upsala coincides. With the disturbance on the 19th the aurora borealis at Münster and at Carthaus near Dülmen. With the disturbances on the 20th the aurora borealis at Münster, Groeningen, Peckeloh, Oesel, Leipzig, and Upsala. To the magnetic disturbance on the 23rd corresponds the aurora borealis at Glasgow.

September.—To the aurora on the 25th in Melbourne corresponds the aurora borealis at Carthaus, Danzig, Peckeloh, Weisenheim, also at Arnsburg, Oesel in Schleswig, Lichtenberg, Hamburg, Upsala. To the aurora australis on the 26th at Melbourne corresponds the aurora borealis at Lichtenberg, Weisenheim, Upsala, Glasgow. To the aurora on the 30th at Melbourne corresponds the aurora borealis on the same day at Upsala and Lichtenberg. To the aurora australis on the 21st at Melbourne correspond the contemporary aurora borealis at Upsala, Schleswig, Arnsburg, Lichtenberg, Hamburg, Norburg, Alsen, and the magnetic disturbance at Rome. To the aurora on the 24th at Melbourne corresponds the contemporary aurora borealis at Carthaus near Dülmen, Niederorschel, Groeningen, Danzig, Wolgast, Peckeloh, Weisenheim, Norburg, Alsen, Eger, Prague, Oderberg by the Inn, Kremsmünster, Moncalieri, Vienna, Stockholm, Hawkhurst, London. On the same day great disturbances of the magnetic instruments were observed at Rome and at Kremsmünster.

October.—To the aurora australis on the 21st at Melbourne corresponds the aurora borealis on the same day in England, and on the former day in Westphalia and England. To the aurora australis on the 25th at Melbourne corresponds the brilliant aurora borealis which was seen at many places in Germany, England, Russia, Sweden, Italy,* Greece, and Turkey on the same day. To the aurora on the 26th at Melbourne corresponds the aurora borealis on the same day in Hamburg, Lichtenberg, Keitum, Athens, and in England. To the magnetic disturbances on the 1st at Melbourne corresponds the aurora borealis at Peckeloh, Upsala, and in England, and the magnetic disturbances on the same day. To the magnetic disturbances on the 15th at Melbourne correspond the contemporary mag-

* Bulletino Meteorologico dell' Osservatorio del Collegio Carlo Alberto in Moncalieri.

netic disturbances in Rome, and the aurora borealis at Upsala. To the magnetic disturbance on the 24th at Melbourne correspond the great magnetic disturbances at Rome, and the very fine auroræ boreales on the same day in Germany, Russia, England, Turkey, Greece, and Sicily.

November.—The aurora australis of November 9 at Melbourne, lasting from midnight till the morning twilight, corresponds to an hour to the aurora borealis which was seen at clear full moon on the evening of the 8th in Schleswig, and to the magnetic disturbances at Rome on the 8th and 9th. To the auroræ australes on the 15th, 17th, and 18th at Melbourne correspond the auroræ boreales on the 14th, 17th, and 18th in England. To the great aurora australis on the 19th at Melbourne corresponds the contemporary aurora borealis at Münster, Niederorschel, Peckeloh, Schleswig, also at Upsala and in England. To the aurora on the 23rd in Melbourne corresponds the aurora borealis in England of the 22nd and 23rd. To the aurora australis of the 24th corresponds the aurora borealis at Upsala of the 24th and in England. The magnetic disturbances at Rome on the 19th, 20th, 23rd 24th, 25th, and 29th coincide with the aurora australis, on the same days, and the magnetic disturbances at Rome on the 10th, 22nd, and 27th, with the disturbances at Melbourne on the same days.* Besides the aurora borealis on the 22nd in England, and on the 27th in Brünn coincide with the contemporary magnetic disturbances at Melbourne.

December.—To the aurora australis on the 6th and 17th at Melbourne corresponds the aurora borealis at Peckeloh, Keitum, and in England. To the aurora on the 17th at Melbourne corresponds the contemporary aurora borealis at Münster, Schleswig, Breslau, Keitum, and in England. The magnetic disturbance on the 22nd at Melbourne coincides with the aurora borealis on the 22nd in Schleswig.

January 1871.—To the aurora australis on the 3rd and 13th at Melbourne correspond the magnetic disturbances at Rome on the same day, and to the aurora australis of the 13th corresponds the aurora borealis on the same day at Münster, Breslau, Cologne, Schleswig. To the aurora on the 15th at Melbourne corresponds the aurora borealis at Breslau and Schleswig on the 15th. To the aurora on the 20th at Melbourne corresponds the aurora borealis on the 19th at Thurso.

February.—To the aurora australis on the 12th at Melbourne corresponds the aurora borealis on the 12th at Münster and Niederorschel, Peckeloh, Wolgart, Moncalieri, Coeslin, Breslau, the pharos of the Weser, on the west coast of England, Eger, Datschitz, Florence, Rome, Volpeglino, and the aurora borealis on the 13th at 3 A.M. at Rome. The magnetic disturbances on the 4th at Melbourne correspond to the magnetic disturbances at Rome on the same day. To the magnetic disturbances on the 5th at Melbourne corresponds the aurora borealis at Breslau. To the magnetic disturbances on the 9th at Melbourne corresponds the aurora borealis at Cleve and Thurso.

EDWARD HEIS

Münster, Westphalia, June 30

* Bulletino Meteorologico del Collegia Romano, dell' Osservatorio di Palermo edel Collegio Carlo Alberto a Moncalieri.

## *SOCIETIES AND ACADEMIES*

### LONDON

Entomological Society, July 3.—A. R. Wallace, president, in the chair. Prof. Westwood exhibited the minute-book of proceedings of an Entomological Society existing in London in 1780, but which appeared to have been dissolved after about a year. The members seemed to have consisted of Messrs. Drury, Honey, Swift, Francillon, Jones, and Bentley; the meetings being held weekly.—Mr. S. Stevens exhibited a collection of Coleoptera recently made in Ireland, the most interesting species being *Chlænius holosericeus* from near Killaloe. Mr. Champion exhibited an example of *Emus hirtus* recently captured by him in the New Forest; also rare British Hemiptera. Mr. Blackmore exhibited a collection of insects of all orders from Tangiers; locusts were extremely destructive there, and on the shore the pedestrian is often up to his ankles in the dead and dying accumulations of these insects.—Mr. Dunning read a letter from the Rev. Mr. Wayne, of Much Wenlock, calling attention to the damage done to his strawberries in consequence of a Myriopod effecting an entrance into the interior of the ripe fruit; also complaining that his young carrots were destroyed by a dipterous larva, probably that of *Psila rosæ*, which bored into the root.—Mr. Druce exhibited a collection of rare Diurnal Lepidoptera, including species of *Papilio*, *Euryades*, *Heliconia*, *Eresia*, *Catagramma*, *Agrias*. *Paphia*, &c.—Mr. Stainton exhibited an example of *Botys fuscalis* captured by the Rev. R. P. Murray in the Isle of Man, to the head of which a portion of the puparium still adhered; the insect was flying briskly when taken, notwithstanding that it must have been nearly blind. Mr. Albert Müller exhibited a leaf from a vine growing at Basle showing the damage done by *Phytoptus vitis*.—Mr. Riley, State Entomologist for Missouri, exhibited a collection of American insects with their transformations.—Prof. Westwood read a paper on new species of exotic *Papilionidæ*. Mr. S. S. Saunders read a monograph of the Strepsiptera, describing twenty-one species; he considered the group as undoubtedly pertaining to the Coleoptera, in the vicinity of *Rhipiphorus*. Mr. C. O. Waterhouse read a memoir on some species of *Cantharis*. The Baron de Selys Longchamps communicated a statistical sketch of the *Odonata*; the number of species of dragon flies now known he estimated at 1,344.

Society of Biblical Archæology, July 4.—Samuel Birch, LL.D., F.S.A., in the chair. The Rev. F. K. Cheyne, M.A., was duly elected a member of the society. The Rev. B. T. Lowne, M.R.C.S., read a paper "On the Flora of Palestine." He considered that it comprised eight distinct elements, four of the dominant existing floras of Southern Europe, Russian Asia, North Africa, and that of Arabia and North Western India. Each of these floras was stated to occupy a distinct region of the country. Interspersed with these are found numerous examples of plants belonging to palæarctic Europe, constituting its fifth element. The Arctic flora of Hermon and Lebanon constitutes the sixth. Mr. Lowne thought further that the cedars of the Lebanon, and the papyrus of the Jordan lakes were the remnants of two ancient and almost extinct floras belonging to two distinct geological periods.—James Collins read a paper "On the Gums, Perfumes, and Resins mentioned in the Bible," particularly pointing out the fact that few of them were indigenous to Palestine, and that many have been wrongly named by the Greek and later botanists. In the course of his observations Mr. Collins detailed the characteristic differences between the true and false Balm of Gilead, ladanum, sandal wood, &c., and the greater or less efficacy of their medicinal properties. Mr. Lowne and Mr. Collins brought for exhibition a large number of mounted specimens, and a complete collection of gums, perfumes, &c., to illustrate their respective papers.

### PARIS

Académie des Sciences, June 28.—M. Claude Bernard in the chair. M. Robin presented a new edition of his great work on the Microscope.—M. Elie de Beaumont presented a most valuable book by M. Rivat, who died recently, and who was one of the chief engineers in the mining service, containing a new method of extracting silver from sulphuric ores, with the assistance of super-heated steam. The quantity of steam required was originally very great, and is now reduced to $\frac{1}{50}$th of what it was when the first experiments were tried. This process of quantitative analysis is largely used in the Laboratory of the Ecole des Mines, at Paris.—Father Secchi sent a memoir on a supposed relation between protuberances, sun-spots, and "faculæ," as discovered by him.—M. Struve and others sent a letter on behalf of the German astronomers, who will meet at Vienna, and asking for the presence of French astronomers. Some instruments destroyed by the Communists were intended for that meeting.—M. Delaunay has circulated amongst the members a small notice relating to an intended meteorological atlas of France, and presented the volume of meteorological observations made at the National Observatory, which he calls the "Observatory of Paris." M. Charles Sainte-Claire Deville rose immediately in order to present the French Academy with the observations made at the observatory of Montsouris. The two observatories are at a distance of something less than a mile, and a deadly feud appears to exist between them.—M. Ch. Sainte-Claire Deville then read a paper relating to the part taken by him in the projecting of the meteorological atlas of France in 1847.—M. de Falen and Fisher described bathymetrical observations and researches executed on the coasts of France, in 1847, in depths varying up to 250 fathoms. The submarine fauna has no peculiarity worth mentioning. M. Gustave Tissandier, one of the postal aeronauts, presented a *résumé* of the results obtained by the sixty-four postal aeronautical expeditions during the siege of Paris. He merely gives however the number of letters and pigeons sent, but not the number of pigeons returned to Paris, and of letters duly posted in the post-offices of the French postal service delegated in the provinces.

July 3.—M. Claude Bernard in the chair.—M. Delaunay read a letter from M. Marie Davy, in answer to M. Ch. Sainte-Claire Deville's communication on the Physical Atlas of France. The learned astronomer, supporting M. Marie Davy, admits that the idea of constructing a physical atlas belongs to M. Ch. Sainte Claire Deville, who originated it in 1847; but he contends that in 1868 he tried to start it, since nothing had been done during twenty-one years. M. Delaunay contends moreover that it is a duty for the National Observatory to undertake such a publication. It is to be hoped that M. Delaunay's exertions will not interfere with M. Sainte-Claire Deville's own publications, and at all events, that we shall have at least an atlas worthy of the French reputation in meteorological matters. But the safer way for both contending parties should be to agree in a common work. Such a resolution would diminish the expenses to the Republic, and enlarge the chances of common success. M. Sainte-Claire Deville's brother, the chemist, was not returned a member for Paris, although he received more than 50,000 votes.—M. Delaunay presented for M. Latterade a most extraordinary memoir on "The Theory of two Suns." M. Latterade contends that the warm period which is demonstrated by the presence of tropical fossils in Sweden and Norway was produced by the proximity of a very powerful star which had given to the earth an immense quantity of heat, and which from that time has receded into the abysses of celestial space. M. Latterade contends that the *supplementary sun* has not disturbed the elements of the planets, because its attractive power was smaller than its warming power. He states, moreover, that the warming power does not vary according to the mass, like the attractive power. This communication was referred gravely to a committee composed of three members.—M. Champion sent a new memoir on nitro-glycerine, which he has studied with so much care during the investment of Paris. It is not only a very dangerous study, but also a very painful labour, as violent headaches are experienced by persons engaged in such operations. The whole of the memoir is worthy of being read attentively by working chemists. We will not try to analyse it, but merely mention two facts. Electricity is without action on glycerine as proved by Ruhmkorff, and explosion does not take place at 360° Fah. as supposed, but at 540° only.—M. Quatrefages presented an interesting memoir from M. Dareste, who is pursuing with constant success his studies on artificial monstrosities, produced by different operations on eggs during incubation. The learned physiologist examines the alterations produced in the blood, and finds the number of corpuscles is very small indeed under special circumstances.—Father Denza sent from Italy an account of the aurora borealis observed in Italy on the evenings of April 9, 10, 18, and 23. Father Denza mentions other auroræ boreales on the 7th, 12th, and 18th of June. This last display was very brilliant, and was accompanied with very great magnetical disturbances. It coincided, moreover, with great storms observed in England and other countries.—Baron Larey announced that Dr. Castano is just leaving France for a climatological and medical inspection of Denmark, Sweden, Norway, and perhaps Iceland, as well as the Faroe Islands.—In its secret sitting the Academy is discussing the titles of several candidates to fill the room of M. Lamé, who was mostly engaged in abstruse researches on the application of high mathematics to molecular physics during his whole lifetime. M. Puiteux was chosen as candidate in the first line. He will be certainly returned on the 10th. M. Lamé cannot have any fitter or more qualified successor.—M. Delaunay has published the result of observations for the month of June. The greatest excess of black bulb thermometer *in vacuo* exposed to the sun over the ordinary thermometer in the shade was 35½° Fah. on June 1, and the smallest on the 5th, when it was only 4°.

VIENNA

Imperial Academy of Sciences, May 11.—Dr. Neilreich communicated a critical revision of the species, forms, and hybrid forms of the genus *Hieracium* hitherto observed in Austria and Hungary. The author remarked upon the peculiar difficulty of deciding what constitutes a species among the Hawkweeds, and pointed that by one course, the number of species is inordinately increased, whilst the other diminishes it to an unnatural minimum. In his treatment of the Hawkweeds of Austria and Hungary he has adopted a middle course, namely, the establishment of what he calls "artificial species." — Prof. E. Linnemann transmitted a memoir on the simultaneous formation of propylic aldehyde, acetone, and allylic alcohol with acroleine, by the desiccating action of chloride of calcium upon glycerine.—Prof. F. Simony presented the conclusion of his memoir upon the glaciers of the Dachsteingebirge.—Prof. V. von Lang communicated a paper on the dioptrics of a system of centred spherical surfaces.—Prof. C. Jelinek communicated a note by Prof. Handl containing corections of errors in Kunzek's meteorological observations made at Lemberg.—Dr. von Monckhoven exhibited a blowpipe constructed by him for the production of the Drummond light, which permits the use of hydrogen, common gas, or alcohol as the combustible material. He also discussed some of the incandescent materials which may be employed, of which he seems to prefer white marble. Prof. Brühl transmitted three plates of the anatomy of the lice, intended for early publication, for the purpose of claiming priority in case of his results being hit upon by Dr. v. Graber, in his memoir on the same subject lately communicated to the Academy of Sciences.

*May* 16.—The following memoirs were communicated:—"Graphical determination of the stereographic and allied projections of the lines of the geographical sphere," by Prof. J. O. Streissler; "The pressure of water as a motor," by M. F. Schindler.—Director C. von Littrow presented a report upon the determination of the latitude and azimuth effected by Prof. E. Weiss at Dablitz.—M. F. Unferdinger communicated two mathematical papers, one upon four integrals, the other upon the theory of that spherical triangle in which one angle is equal to the sum of the other two.

## BOOKS RECEIVED

ENGLISH.—Mycological Illustrations: W. W. Saunders, W. G Smith, A. W. Bennett, part 1 (Van Voorst).—Darwinism Refuted: S. H. Laing (E. Stock).—A Treatise on Asiatic Cholera: C. Macnamara (Churchill).—A History of British Birds: W. Yarrell, edited by A. Newton, part 1 (Van Voorst).—The Census of England and Wales for 1871, Preliminary Report.

AMERICAN.—A Treatise on Diseases of the Nervous System: W. A. Hammond (New York, Appleton).

FOREIGN.—Das Leben der Erde: N. Humnel (Leipzig, Fleischer).—Die Grundsätze graphischen Rechnens, part 1: K. Von Ott (Prag, Calve).

## CONTENTS

THURSDAY, JULY 20, 1871

## *THE NEWCASTLE-UPON-TYNE COLLEGE OF PHYSICAL SCIENCE*

WE have apprised our readers from time to time of the progress of the arrangements for the proposed College of Physical Science in Newcastle. In our report of the meeting held on the 25th of March, at which the scheme was first publicly broached, we expressed an estimate of the administrative ability of the committee appointed to carry into effect the resolutions then adopted, which events have amply justified. A second public meeting was held in Newcastle on Saturday last to receive at the hands of the executive an account of their labours, and the Report now before us shows the energy which has been brought to bear upon a complicated and laborious task. Few who read the announcement of the first meeting—probably few even of those who were there present—supposed that the ship, of which the lines were then but talked of, could be ready at the expiration of little more than three months to launch and make her trial voyage.

We need not reprint the whole Report, as portions of it are merely the official announcement of arrangements already made public; but its contents may be briefly summarised. The Committee state that—

"At their first meeting it was found that the scheme had so far interested the general body of the inhabitants of the district, that very much more support would be forthcoming than was sufficient to provide for the six years' experiment; and as the University of Durham intimated that the promised aid (1,000*l*. per annum) would be made permanent if a capital sum could be obtained sufficient to secure the continuance of the support from the district, it was considered advisable to appeal to the public for 30,000*l*. This sum was mentioned not as being completely adequate to meet the expenses of a collegiate institution, but as providing sufficient funds for the commencement of such an undertaking."

Towards this fund 21,460*l*. has been obtained, together with three subscriptions each of 100*l*. a year, and a hope is expressed that within the next few months between 30,000*l*. and 40,000*l*. may be raised.

The election of Professors in Mathematics, Geology, Chemistry, and Experimental Physics, and the determination of the Committee to open the College in October are announced. It is recommended that the lecture fees should be such as may secure a large attendance of students and it is suggested that five guineas yearly for each course, and one guinea entrance, would be suitable to this end.

It had been agreed by the committee to propose the following Constitution. The governing bodies of the College to be:—firstly, the *Governors*, secondly, the *Council*.

1. The *Governors*; to be limited at first to forty-seven, of whom nine are to be *ex-officio* members of the body. Of the remaining thirty-eight, three are to be north-country Members of Parliament, and two Professors in the College. Nine are to be elected by subscribers to the fund, and the rest in different proportions by the Chapter of Durham, the Senate, the Convocation, the Municipal Councils of twelve northern towns, and the Scientific Societies of the district. Of the elected members one-third are to vacate their seats every two years, but are to be eligible for re-election.

2. The *Council*, which is proposed to "consist of fifteen members, including a Chairman, of whom five shall be members of the Chapter, Senate, or Convocation of the University of Durham; and of these one shall be the Warden, and of the other four at least two shall be members of the Senate; but all members of the Council shall be elected out of and by the Governors themselves. They shall be elected for five years, one-third of the number resigning their places every three years, but being re-eligible. They shall in all cases retain their appointment for two years."

This Council is to meet monthly, and to transact all the ordinary business of the College, arranging the periods of study and fees of the students, and to assume the general direction of affairs.

To a committee of this body, with which all the Professors shall be combined, is entrusted the more strictly academical administration and discipline, including the number and direction of the Professors' Lectures, subject to the general control of the Council.

In conclusion, it is proposed "that while steps are being taken to draw up the necessary documents and to proceed with the election of the Council in the terms of the Constitution, the government of the College shall remain in the hands of the Executive Committee, who shall act as the Council for the period of one year.

"At the invitation of the North of England Institute of Mining and Mechanical Engineers, the Literary and Philosophical Society, the Medical College, and the Natural History Society, occupying a group of buildings in a good situation already dedicated to scientific purposes, a suitable suite of rooms for the lectures, private rooms for the professors, laboratories, and offices, has been secured, which will be sufficient for the accommodation of the College for some years, when it is hoped the success of the Institution will be so secured that a sum of money sufficient to build separate and suitable accommodation will be easily procured."

It is almost needless to say that this report was well received and unanimously adopted, and that the meeting willingly accorded to the Executive Committee the proposed continuation of their powers for a year. In the discussion some remarks were made by gentlemen taking an active part in the labours of the Committee, which are worthy of note.

Mr. Lowthian Bell alluded to the present available accommodation for the College as insufficient, except for temporary purposes, and suggested the probable necessity for building at any rate new laboratories. He also bade the meeting to regard the four professorships already established as but a commencement, there being many other departments of Physical Science which must ere long be provided for, and made special allusion to the claims of Biology to representation.

It is true that another member of the committee with very pronounced political views, suggested that a chair of Political Economy should be the next subject adopted, but as he did not attempt to show the position of Political Economy in his scheme of physical science, it was scarcely thought necessary to argue the point.

In far wider spirit were the remarks of a subsequent speaker, who looked hopefully forward to the time when success in teaching physical science should enable them to enlarge the basis of the Institution, so as to include all the higher branches of a liberal education.

We have, on more than one occasion, advocated the addition of a Biological chair to the four already agreed upon, for it has seemed to us an anomaly that a School of Physical, or, to use the correlative term, Natural Science, should be without teaching in general Natural History, especially in a locality in which excellent facilities exist. But there is another view which has been prominently in our mind. If the College were intended to be a mere mining and engineering school, established to enable engineers and coal-viewers to educate their pupils with less labour and cost, its claim upon the general public would be small. It is due to the public that Science for its own sake,—Science with less direct reference to considerations of pounds, shillings, and pence, should be recognised; and in no way could this be so readily done, under existing circumstances, as by the establishment of a Chair in Biology.

We readily admit the pre-eminent importance of the subjects selected to commence with, and as willingly record our confidence that the Executive Committee will approach this as all other subjects with the single desire to do what is right.

Finally, we would make one comment on the attitude of Durham University. For many years past it has been regarded as almost hopeless to expect any active assistance in educational matters from that hitherto somewhat sleepy body. But with the new Dean seems to commence a new *régime*, and facts appear to bear out the testimony of many of the speakers at Saturday's meeting, that in all arrangements in connection with the new College, the University authorities have shown the widest liberality and unselfishness.

The vast importance of schools of this sort, and the prospect of a movement with a similar object in the West Riding of Yorkshire, renders needless any apology for reviewing with some detail this last addition to our scientific institutions.

## PERCY'S METALLURGY OF LEAD

*The Metallurgy of Lead, including Desilverisation and Cupellation.* By John Percy, M.D., F.R.S. (London: J. Murray, 1870.)

THE preparation of metallic lead from its several ores, amongst which galena stands foremost, presents to us processes and circumstances which, though generally simple, are amongst the most interesting and delicate in the whole range of productive metallurgy. It is therefore with even more expectation than attached to his former volumes on Copper, Zinc, and Iron that we opened Dr. Percy's present volume; and, in finding a copious and well-arranged compilation, we have not been disappointed, although we might have anticipated something more of original research.

It would, indeed, be improbable, with the great power of obtaining information directly from manufacturers necessarily belonging to the influence and position of a Professor at the Government School of Mines, that the result should be any other. Accordingly, the reader who desires to obtain a distinct and tolerably detailed though "bird's-eye" view of all the various forms of commercial metallurgy of lead (in humbler phrase of lead smelting) will here find a classified survey of it as practised in Great Britain, all over Europe, and in North America, with some notices of attempts made in South America. Of the very ancient lead-smelting processes of Asia, probably the earliest practised on a large scale in the world, and still believed to be in use in China and Japan, we do not find a word. Of recent methods in use in Japan there is a brief notice from Mr. Pumpelly at p. 384, and in China at p. 479.

The first one hundred pages are occupied with the physical and chemical properties of lead viewed from the metallurgical stand-point, one which we cannot but think is always essentially misty and unsatisfactory. The physics and chemistry of any metal ought to be the same to everybody, and it seems to us ought to be fully and accurately known before ever the student opens a metallurgical book. If that be admissible, then metallurgy proper has its limits advantageously defined and narrowed, and its treatises ought to be then divided into two distinct classes—the one like the small octavo volume of Rammelsberg (that most elegant and classic work, now several years published, but yet as true and valuable in almost every page as when it was wet from the press), which teaches the *principles* of metallurgy, that is to say, the principles of those reactions which occur in the established and fully-adopted processes of commercial metallurgy, without going into any details as to apparatus, furnaces, or criticism, as to whether this or that method or construction of plant be better or worse. The other, consisting *not* of any attempt to aggregate in one volume the details of manufacturing apparatus, of trying to tell all about the minutiæ, of all the diversities of all the commercial metallurgies in the world —which, we are compelled to say, is impossible within even the very diffuse limits taken by Dr. Percy—will best consist, we think, of *monographs*, such as those of M. Grüner, in the *Annales des Mines* of a year or two back, on this subject of lead. Each one of these monographs, with the necessary plates of illustration, should really, and in a genuinely practical way, exhaust one single national or special system of smelting of lead, or of some one other metal.

Such has been the plan almost universally adopted in Germany and France, and with results at once far more comprehensive, clear, and exact, than are practicable from the hand of any one man, however able, or in any volume though bulky, illustrated only by woodcuts however excellent, and those of Dr. Percy's present volume are remarkably clear and good.

For the practical and exhaustive description, in fact, of any single smelting process largely in commercial use, an atlas of folio copper plates, forming a volume in itself, is indispensable. The result of the contrary view of the metallurgist's descriptive task, is inevitably that want of balance, and yet incompleteness here and there, which characterise all these metallurgic volumes of Dr. Percy. Thus, for we feel bound to give an example to sustain our criticism, in his volume on Iron, Dr. Percy goes into the question of blowing machines, blast cylinders, and the like—a thing really as foreign to the metallurgy of iron as the theory and practice of building chimney stalks would be to that of lead;

and Dr. Pole, it appears, wrote for him the rather jejune algebraic investigation of the principles of such machines, which, when we come to examine it, we find is merely what we may find in any elementary book on pneumatics; and owing to the omission of all the *structural conditions producing loss of effect* in blowing machines, exists, in fact, as a mere parade of useless symbols, of no value to the constructor or the purchaser or the user of such apparatus.

Now we are wholly unable to see the necessity for thus cumbering with a needlessly hooked-on subject a book on Iron Metallurgy at all; but if otherwise, then it should have been gone into thoroughly, and in a way to be of real value to the constructor. To have done this, however, would have required some fifty pages or more, so that a far better mode, in our judgment, would have been to have simply confined the point to a reference to the great monographs which exist on this special subject, both theoretic and practical. Neither Dr. Percy nor Dr. Pole seems to have been aware of the fact that a quite exhaustive investigation of the theory of blowing machines (omitting none of the conditions of practice) and of high merit, was published as long ago as 1805, by Herr J. Baader, Counsellor of Mines of the Kingdom of Bavaria, and which was specially and by the authority of Napoleon I. translated into French and published in the *Annales des Mines* in 1809. There may be such a thing as apparent completeness, which yet is only the piling together of incongruity or of incompleteness.

But this want of the sense of balance and of relative importance is not confined to such collateral subjects of practice. Dr. Percy, in the volume here noticed, devotes nine pages to the physical properties of lead, in commencing, and of these we find four (under the head of Resistance to Pressure) are occupied with details of Coriolé's fruitless attempts in 1829 to construct weighing machines, whose indications were to be derived from the compression suffered by known volumes and forms of lead pieces—a subject as indirect and foreign to the physical properties as it is far away from the metallurgy of lead.

One statement made in this part of the volume is undoubtedly incorrect, where it is said, "by hammering lead becomes harder, but acquires its original softness by annealing." The actual fact is, that lead cannot be made harder by hammering, for its annealing temperature is so low (that of every metal being a function of its fusing point), and it suffers so large a deformation by reason of its softness when hammered, that enough heat is evolved by internal work to cause the metal to anneal itself,—in other words, never to become harder. This has been fully ascertained, and the fact has even been taken practical advantage of by those engaged in "drawing lead pipe" by the older methods, who are well aware that a hard pinch at first or rapid reduction in diameter in passing through the holes of the draw-plate, heating the lead, enables it to be drawn into finished pipe with a less total expenditure of power than if drawn slowly and with so gradual a reduction in diameter as that the lead should remain always nearly cold. Were the lead hardened here by a compression quite the same in effect as hammering, the very reverse must be the case. This volume comprises a very good account of the Pattinson process for separation of silver, and also of Parkes's zinc process. What can have induced Dr. Percy (who is, we believe, fond of scholarship) to employ such barbarous compounds as "lithargefication," "desilverisation," and "decopperisation," in place of "disargentation," "decuperation"? What would be thought of "desugarification" as a substitute for "desaccharisation"?—but these are matters of taste and no more.

The chapter on the ores of lead and that on the assay of lead ores are amongst the very best in the volume, which is beautifully printed with the clearest of type and paper, and with good indices. There are nine pages near the end devoted to poisoning by lead, which, though certainly not the metallurgy of lead, may prove of some use to those employing work-people in lead smelting or manufacturing operations; though we think here, perhaps, the wisest instructions might have been simply, "send the patient to the doctor." We have little confidence in amateur or improvised medicine on the part of "laymen," in such cases as lead-poisoning, at any rate. On the whole, though, as we have had to point out, this work of Dr. Percy's is not free from faults, it is, we think, in several ways the best of all those on Metallurgy which have appeared from under his pen, and in the collection and discussion of a vast array of facts is a noble volume, the very best that yet exists in English on its subject.

## NEWMAN'S BRITISH BUTTERFLIES

*An Illustrated Natural History of British Butterflies.* By Edward Newman, F.L.S., F.Z.S., &c. 8vo. (London: W. Tweedie, 1871.)

THE British Butterflies form a small but striking group of insects, and hence not only are they as a general rule the first objects on which the collecting spirit of the

RED ADMIRAL (*Pyrameis Atalanta*). Upper side.

Under side.

young entomologist is exerted, but they also offer one of the best means of commencing the study of entomology. Thus they are easily collected and preserved, their appearance is pleasing and often beautiful, their characters are generally very clear and distinct, so that the discrimination of the species is by no means difficult, and their

PAINTED LADY (*Pyrameis Cardui*).

Painted Lady. Var. 1.

Natural History is easily studied; whilst the small number of the species renders it an easy matter for the beginner to procure in a season or two by far the greater proportion of the known forms.

Although there are already many books treating specially of the British butterflies, some of them expensive, and others so cheap as to come within the reach of every one, we cannot blame Mr. Newman for adding one more to the number, especially as his work is distinguished by the great prominence given in it to the Natural History of the species. Upon this subject, as also upon the distribution of the species in Britain, Mr. Newman has long been publishing details from his own observations and those of other naturalists, in his periodicals the "Zoologist" and "Entomologist;" and the whole of the information thus accumulated is here summarised and supplemented with observations derived from other sources. Another useful feature in the present work is the insertion of notices, and frequently of figures of the more important varieties of each species, which will often relieve the young student from a state of puzzled suspense in the determination of his specimens. The classification adopted is founded, in its broad outlines, upon the preparatory states of the insects, but it leads pretty nearly to the same results as the system more generally followed.

The illustrations are very numerous, including figures of all the species and of both sexes when there is any difference either in the upper or lower surface. They are all woodcuts, and are generally well executed, as may be seen from the examples which we are enabled to give.

W. S. DALLAS

## OUR BOOK SHELF

*The Western Chronicle of Science.* Edited by J. H. Collins, F.G.S., Secretary to the Royal Cornwall Polytechnic Society. January to June, 1871. (Falmouth. Pp. 96.)

WE are glad to afford space for a short notice of this cheap scientific journal, which, although specially intended for the benefit of the mining population of Cornwall and West Devon, deserves a wide circulation in all our mining districts. Each monthly number contains one or two original articles, either on general subjects, as "The Practical Value of Scientific Knowledge," or giving descriptions of various forms of machinery, followed by notices of books, and a monthly chronicle of science. From one of the editorial articles on "The Practical Value of Scientific Knowledge," we learn that a good stoker may effect an annual saving of nearly 35*l.* per annum over a bad one, and that it is a common Cornish habit to hang heavy jackets, great coats, &c., on the lever of the safety valve of engines devoid of a pressure guage; while the farmers, with the view of giving their ground two good things at once, mix lime with their guano some days before spreading the manure. A very remarkable natural-history statement is made by Mr. Williams, of Hayle, in his paper on "Scientific Nursing." "I have (he says) in my possession a double chick, the produce of an egg laid by a barn-door fowl, one half being the natural species, the other half composed of the sparrow-hawk!" Until this remarkable chick appears *in propriâ personâ* at the office of NATURE, or, at all events sends us its photograph, we must, with much regret, decline to accept the fact.

*Medizinische Jahrbücher.* Herausgegeben von der K. K. Gesellschaft der Aerzte, redigirt von S. Stricker. Jahrgang, 1871, Heft I. und II. Mit 26 Holzschnitten und 2 lithographirten Tafeln. (Vienna: Braumüller; London: Williams and Norgate, 1871.)

THE two parts before us are the continuation, in a new form, of an old and valuable periodical, and, under Prof. Stricker's able editorship, its tendency, instead of being, as heretofore, chiefly clinical, will be so far modified as to embrace all the results obtained in the physiological laboratory. A glance at the table of contents is sufficient to prove the truth of this statement. Thus putting aside the first paper by Prof. Stricker, entitled "Pathology and Clinical Observation;" the rest, nine in number, are nearly all devoted to the results of microscopic research. Thus, Dr. Genersich contributes a paper on the Serous Canals of the Cornea; Dr. Heiberg one on the Regeneration of the Corneal Epithelium; Dr. Güterbock one on Inflammation of Tendons; Dr. G. F. Yeo one on the Structure of Inflamed Lymphatic Glands; Dr. Lang one on the First Stages of Inflammation in Bone; Dr. Albert and Dr. Stricker one on Surgical Fever, and the latter author another on the nature of the Poison of Pus, and so on. The journal leads off with a good start, and if it continues as it has commenced, will probably take up a leading position. We notice one or two of the papers that appear to be of general interest. H. P.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Cotteau's "Echinides de la Sarthe"

A NOTICE of Cotteau et Triger's *Echinides de la Sarthe* in a recent number of NATURE (June 15, p. 120) is likely to convey a false impression of the accuracy of M. Cotteau, and throws considerable doubt on the value of his work. It is not often that French scientific men are as conscientious as he is in the examination of authentic types. There is hardly a collection of fossil Echini which M. Cotteau has not examined; and his thorough acquaintance with all that has been written on his subject, as well as his intimate correspondence with the principal echinologists, is a sufficient guarantee that no important memoir (such as Wright's monograph) could have escaped him. Anyone who will take the trouble of turning to Cotteau's work (p. 111) will find, under *Pseudodiadema hemisphæricum*, a notice of Dr. Wright's figure of the same species (so much superior, with many others, to Cotteau's?) and a reference to his description. Nor is this an isolated case. Throughout the work M. Cotteau discusses and criticises more or less the results of this very monograph, said to have been overlooked by him. The mistake Cotteau is accused of making of assigning to Desor instead of Agassiz the specific name of *Pseudodiadema hemisphæricum* is entirely unfounded. Referring again to p. 111., we find, as a synonym, *Diadema hemisphæricum* Agass. M. Cotteau, like many continental and American writers, does not interpret the notation of species as is required by the laws of the British Association, but for that reason he should not be accused of committing mistakes which his own writings show him not to have committed. M. Cotteau, in common with others, looks upon nomenclature simply as a matter of registration; and when M. Desor transfers to *Pseudodiadema* the *Diadema hemisphæricum* Agass., M. Cotteau writes, therefore, *Pseudodiadema hemisphæricum* Desor, and not Agassiz; he may be wrong, according to the principles of the writer in NATURE, but he has not, either in this instance or in the other cases alluded to, committed a mistake through ignorance of the subject. A. AGASSIZ

### Mr. Howorth on Darwinism

MR. HOWORTH sneers at "Survival of the Fittest" as an "identical expression" which "might have suggested itself even to a child," an axiom, in short, of which the truth cannot be disputed. This is satisfactory; but it is strange that he did not apply this axiom to his own theory, and see how they agreed together. He would probably admit, as another discovery "that might have suggested itself to a child," that *as a rule* the entire offspring of each animal or plant, except the one or two necessary to replace the parents, die before they produce offspring (this has never been denied since I put it prominently forward thirteen years ago). He would further admit, I have little doubt, that a great majority of animals and plants produce during their lifetime from ten to a thousand offspring, so that fifty will be a low average, but the exact number is of no importance. Forty-nine. therefore, of every fifty individuals born, die before reaching maturity; the fiftieth survives because it is "best fitted to survive," because it has conquered in the struggle for existence. Will Mr. Howorth also admit as self-evident, that this one survivor in fifty is healthy, vigorous, and well nourished, not sickly, weak, or half-starved? If he maintains that it is the latter, I shall ask him to prove it; if the former, then what becomes of his theory as an argument against Natural Selection? For, admitting as a possibility that his theory of the greater fecundity of the weak, &c., is true, how are these weak or sickly parents to provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves? The one in fifty who survives to leave descendants will inevitably be the strong and healthy offspring of strong and healthy parents; the forty-nine who die will comprise the weaker and less healthy offspring of weak and sickly parents; so that, as Mr. Darwin and myself have long ago shown, the number of offspring produced is, *in most cases*, the least important of the factors in determining the continuance of a species.

I have thought it better to go thus into the heart of the question, rather than defend myself from the charge of dogmatism, for stating as a fact that the most vigorous plants and animals are the most fertile. I repeat the statement, however, referring to Mr. Darwin's observations, and especially to those in which he *demonstrates by experiment* that cross-breeding produces the most vigorous and luxuriant plants, which again produce by far the largest quantity of seed. The facts that wild animals and plants are, as a rule, healthy and vigorous, that the head of the herd is the strongest bull, and that weak and sickly carnivora are rarely found because they must inevitably starve to death, sufficiently refute Mr. Howorth's theory as against Natural Selection. If he can point to any district upon the earth where the animals and plants are in a state of chronic debility, disease, and starvation, I may admit that there his theory holds good; but such a district has not yet come under my observation, or, as far as I am aware of, been recorded by any traveller.

I still maintain (Prof. Jowett's authority notwithstanding) that the phrase "Persistence of the Stronger" does not truly represent "Natural Selection" or the struggle for existence;" and, though it may often be true, is not the whole truth. The arguments of Mr. Howorth from the history of savages will, I think, not have much weight, if we may take as an example his putting together as cause and effect the extinction of the Hottentots and their now obtaining enough to eat. ALFRED R. WALLACE

MR. ALFRED WALLACE directs attention to the gross error of supposing that "the struggle for existence means the persistence of the stronger," and correctly stigmatises this view of Mr. Howorth's "a pure misrepresentation."

It is, as Mr. Wallace remarks, very curious and even ludicrous, after all that has been said and written upon the matter, that anyone should fail to recognise the advantages to their possessor of "obscure colours," "cunning," "nauseousness," "bad odour," and other qualities superior to strength alone. The creature having these properties, at last brought to perfection through the operation of natural selection, acting through countless generations, will assuredly have the advantage in the battle of life over its less fortunate neighbours. It will survive in the struggle for existence. Having survived, is it not better that it should at once teach the world the law of its survival, and proclaim itself the fittest to survive, than that it should remain silent until those whom it has destroyed may rise from the dead and admit that their doom was deserved because they were not fit to live? LIONEL S. BEALE

MR. HOWORTH, it seems to me, has not chosen a very favourable time for so strongly maintaining the truth of Mr. Doubleday's theory, seeing that the recent census has shown that the population of England has increased not only with an increment absolutely greater than that shown by any previous census, but also—and this is still more important—with an increase proportionally greater than during the last decade. Yet never, surely, has luxury been so prevalent among us as during these last ten years. The evidence thus afforded will perhaps be deemed more conclusive than the argument of Mr. J. S. Mill, who invites those who may be inclined to accept Mr. Doubleday's opinions "to look through a volume of the Peerage, and observe the enormous families almost universal in that class; or call to mind the large families of the English clergy, and generally of the middle classes of England" ("Principles of Political Economy," bk. 1, ch. x., note). Mr. Howorth, however, states that "the classes among us who teem with children are not the well-to-do and the comfortable." If this statement were absolutely true, it would be of little service to Mr. Howorth, since it is in the classes referred to that prudential restraint acts with the greatest force, and the effects of this restraint, both direct and indirect, would have to be taken into account before his conclusion could be admitted. He further asserts that "a state of debility of the population induces fertility," since "where mortality is the greatest there is much the greatest fecundity." That births should be most numerous where the mortality is greatest, requires for its explanation no hypothesis respecting the fertilising power of debility. "The fact," says Malthus, "may be accounted for without resorting to so strange a supposition as that the fruitfulness of women should vary inversely as their health. . . . When a great mortality takes place, a proportional number of births immediately ensues, owing both to the greater number of yearly marriages from the increased demand for labour, and the greater fecundity of each marriage from being contracted at an earlier, and naturally more prolific, age" (vol. i., pp. 472, 473, 5th edit.). Man's reproductive power is always in civilised life more or less checked, and ready to be more or less exercised in proportion to the lessening by death of the restraining pressure.

THOMAS TYLER

MR. WALLACE, in replying to Mr. Howorth's objections to the theory of Natural Selection, points out that that gentleman first misrepresents Darwinism, and that having done so he does not employ the distorted doctrine as premisses to a further conclusion. But the second part of the criticism is not quite just. Mr. Howorth, after stating the Darwinism theory, introduces us to an order of facts which is at variance with that theory as apprehended by him; and not only does he do so, but he places an interpretation upon these facts which is utterly irreconcilable with the Darwinian theory as understood by its most able expositors. It is true that Mr. Howorth does not bring his interpretation of the facts he adduces and the theory of natural selection into such juxtaposition as to show their mutual contradiction; but a little consideration will enable Mr. Wallace to supply the missing links, and to see that in any generous construction of Mr. Howorth's letter, the real questions at issue are the correctness of the facts he adduces and the validity of the generalisation he makes from these facts. My object in writing is to direct Mr. Howorth's attention to Mr. Herbert Spencer's profound discussion of this subject, as it appears to have escaped his notice. This is the more surprising, since, on p. 111, vol ii. of "The Variation of Animals and Plants under Domestication," and to which Mr. Darwin refers him, there is the following marginal note:—"Since this MS. has been sent to press, a full discussion on the present subject has appeared in Mr. Herbert Spencer's 'Principles of Biology' vol ii., 1867, p. 457, *et seq.*" He is a bold man who undertakes to enlighten the public on a subject which Mr. Spencer has fully discussed, without first ascertaining what view that profound and original thinker adopts; and most certainly a fresh writer coming into the field ought to take up the discussion where an author of such eminence has left it. If Mr. Howorth will look at Mr. Herbert Spencer's "Principles of Biology," he will find in sections 78 and 79, an explanation of the process adopted by gardeners of cutting the roots, and "ringing" the bark of fruit trees. Section 355 explains the fact that fatness is often accompanied by barrenness. In a footnote at p. 483, vol. ii., he will find Mr. Doubleday's doctrine specially noticed, and the fallacies upon which it is based exposed; while in the chapters "On the Laws of Multiplication," vol ii., p. 391, *et seq.*, he will find the whole subject treated with a fulness and exhaustiveness which leaves little to be desired. Mr. Howorth will notice that Mr. Spencer does not deny Mr. Doubleday's facts, but that he places upon them an interpretation which brings them into harmony with the general theory of evolution, and with the special part of organic evolution which constitutes the Darwinian theory.

Newchurch, July 17 JAMES ROSS

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I HOPE you will allow me a few lines to reply to Mr. Howorth.

I had thought Mr. Doubleday's essay was among the things of the past. There can be no question that his conclusions are not the conclusions of accomplished naturalists like Mr. Wallace, whose assertions are certainly as good, if not far better, than those of Mr. Doubleday.

Quoting Mr. Chadwick, Mr. Howorth again puts cause for effect. There can be no doubt that the death rate increases in a crowded country *pari passu* with the crowding, and that the crowding is the result of fertility. It by no means follows that the crowding produces fertility.

There is one way in which poverty and overcrowding tend to increase the birth rate. Many of the children of the poor die during the first few months of life, and hence the mother, being relieved of her offspring, ceases to secrete milk, and soon again falls pregnant. It is the death of very young children in crowded districts which so largely increases the mortality, and this, as we have seen, may tend to increase the birth rate.

The large percentage of deaths in early life amongst the ill-nourished and weakly renders these less likely to bear children than the strong. With regard to the large families of the poor so often quoted, I have grave doubts of the fact. I have for many years seen hundreds of poor families every year in the exercise of my profession of surgeon, and although I know many instances of ten or fifteen children having been born of one mother, in the majority not more than two or three reached adult age, and hence these produced no offspring in the second generation.

The most remarkably prolific woman who has come under my notice has had twenty-two children in twenty years, and she is still continuing to present her husband with blessings. She is one of the fattest women I know.

Amongst the rich and the well-to-do it is no uncommon thing for eight or ten children to grow to man's and woman's estate and to rear families. I know as many well-to-do persons with large families as poor people, and the living percentage is far greater in the former.

I am not aware that consumptive patients are so extremely prone to breed as Mr. Howorth thinks, certainly their children do not live to produce a second generation as a rule.

Examples of fecundity and barrenness amongst wild tribes are not much to the purpose, because there are so many disturbing influences. To take, however, Mr. Howorth's case, the Red Indian feeds ill enough and is thin enough, yet he is not fertile. The backwoodsman, with his vegetable diet, would be far more likely to grow fat, and is certainly far better fed and far stronger than the Indian, yet he is more fertile than the Indian, although by no means fertile. He has many hardships to undergo.

With regard to the Patagonian women and their belief that bleeding produces fertility, evidence is wanting as to the truth of their belief. We know many wide-spread beliefs are erroneous, for instance, most savages believe in rain-makers.

In conclusion, Mr. Howorth thinks that wild animals in captivity are sterile from over-feeding. If he will try and make them fertile by starving them, I think I may assert positively he will fail. Hence, I suspect, we must look for a deeper cause of barrenness in them. B. T. LOWNE

99, Guilford Street

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## Recent Neologisms

IN using the word Mr. Ingleby objects to as hideous, I was not aware that I was coining a new one. If so, it was quite unconsciously on my part; but a word was wanted to express the property of being prolific, and if the choice lies between "prolificness" and "prolificacity," as I think it does, I am inclined to believe that the former will survive, as being the shorter, the easier to pronounce, and perhaps the less hideous, even though it may not be constructed on the best etymological principles. "Fertility" and "fecundity," which are often used, do not quite answer the purpose, although the latter has very nearly the same meaning. Our language must and will grow; and its growth will be determined by convenience rather than by grammatical rules. ALFRED R. WALLACE

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DR. INGLEBY is in error as to the recent introduction of "survival," "impolicy," and "prolificness." All these words will be found in Chalmers's abridgment of "Todd's Johnson" (1820); the first with a reference to Sir George Buck, the second with one to Bishop Horsley, and the third with one to Scott (not Sir Walter). "Indiscipline" does not occur, but "indisciplinable" does, Hales being cited as the authority.

R. G.

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IN his excellent custom of "registering the first appearance of new words and new phrases," Dr. C. M. Ingleby is surely very careless or superficial. He quotes "survival" as a new word introduced, he thinks, by Darwin. I have been familiar with it as long as I remember, and my life of careful observation has exceeded a quarter of a century. "Impolicy" is equally familiar, having had currency at least twenty years before the Franco-Prussian war, to which Dr. Ingleby accredits it. He will find both words, as well as "indiscipline," in "Webster's Dictionary," edition 1852, and probably much earlier on careful search. "To telegram" is clearly a vulgarism, rarely heard I imagine, and never seen in print. G. W. S.

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## Fertilisation of the Bee Orchis

MR. DARWIN, in his "Fertilisation of Orchids," states his belief that the Bee Orchis presents a physiological difference from all other British orchids, and is habitually self-fertilised. I had, yesterday, an opportunity of observing a number of these plants in one of its abundant localities in Surrey, and at a time when fertilisation must have been completed. In every plant almost all the capsules were considerably swollen, and were loaded with apparently fertilised ovules. In most of the withered flowers, the remains of the pollinia were still visible in the position described by Mr. Darwin, hanging down before the entrance to the nectary, in immediate proximity to the stigma, and rendering it almost impossible to believe that the flower had ever been entered by any insect of considerable size, which must inevitably have carried away the pollinia with it. The fact that the Bee Orchis, the most "imitative" of all our native plants, is never visited by insects, is a very suggestive one. If, as might well have been assumed, the object of the "mimicry" is the attraction of bees, the device appears to have signally failed.

London, July 17 ALFRED W. BENNETT

### Saturn's Rings

As Lieut. Davies has thought it necessary to refer to your remarks about the satellite theory of Saturn's rings—and in so doing has named my work upon Saturn (which you had *only* referred to without naming) it may be as well for me to mention, that I nowhere in that work claim the theory as mine—and that, whenever I have seen it referred to as mine, I have as publicly as possible disclaimed all title to it.

Permit me to add, that, whatever opinion we form of Lieut. Davies's views, he deserves our thanks for bringing out a treatise so full of work, from cover to cover, as his "Meteoric Theories." Such examples are a good deal needed in these days.

8, Wellington Villas, Brighton RICHARD A. PROCTOR

### Ocean Currents

I FIND that Dr. Carpenter does not consider his experimen probative. Judging from the air of triumph with which, both in his lectures and writings, he has announced its success, I had certainly imagined that he did. But if not probative, what is it? Dr. Carpenter says it is only intended to be illustrative. What does it illustrate? It does not illustrate any currents formed in the ocean by differences of temperature; for it does not illustrate the differences of temperature to which he attributes these currents. In his letter in NATURE of July 6, he proposes an unwieldy modification of his former well-known experiment, but which still, I would submit, in no way avoids the difficulty to which I have called attention. He describes a strong freezing mixture applied to the surface through one-tenth of the length of a trough half a mile long, and heat applied to the surface also through one-tenth of the length, measured from the other end: between the cold and the hot surface there is, then, an intervening space of four-tenths of a mile, or 2,112 feet; that is to say, there is a thermometric gradient of about 50° in 2,000 feet, or 1° in forty feet. This is small enough, and we may perhaps doubt whether such a gradient could give rise to any appreciable movement; but it is 15,000 times greater than the gradient observed in the ocean, which is about 1° in 100 nautical miles; and any movement shown by an experiment which, in its details, bears no reasonable proportion to the reality, cannot be accepted as an illustration of a movement in the ocean.

Mr. Proctor, in the same way, speaks of his proposed experiment as an illustration; and, in the same way, I would say that the distortion produced by magnifying 6,000,000 times that particular detail on which he wishes to lay an emphasis, precludes our accepting it as an illustration at all. Mr. Proctor says that it is intended specially to throw light on the easterly and westerly movements; it is surely unnecessary for me to point out to him that any easterly or westerly movements, as illustrated in a cylinder such as he describes, revolving continuously and uniformly, are direct consequents of the outward or inward movement due to the differences of temperature, and are, therefore, in the strictest sense, dependent on the thermometric gradient. If, with a thermometric gradient of $\frac{1}{600000}$ of a degree in one foot, and with an angular velocity of 360° in 24 hours, Mr. Proctor succeeds in showing any appreciable movement, I and (I think I may add) many other readers of NATURE will be glad to learn the result. But this is, after all, the point I raised in my last letter (NATURE, June 29), and which Mr. Proctor considers would require many columns for its full discussion. I do not myself see that there is any room for discussion at all; and any difference of opinion that may exist can only be met by experimental demonstration.

Dr. Carpenter appears to wish to support his theory on "authority," and especially on that of the recent letter of Sir John Herschel. This is a point on which I touch with great reluctance; but I would point out, in the first place, that "authority" in matters of science carries very little weight; and, secondly, that Sir John Herschel, in the letter referred to, merely admits what he and everyone else have all along admitted, that hot water and cold, in juxtaposition, will establish a circulation. It was not for him, in a letter of private courtesy, to enter again on a discussion of the infinitesimal nature of the gradients—a discussion which he had already worked out very fully in his "Physical Geography."

But, leaving this consideration on one side, I maintain that, at the present time, the *onus probandi* rests with the supporters of the temperature theory. Its opponents have offered what is, at any rate, a rational, consistent, and tolerably complete explanation of all the known ocean currents; and they say, in so many words, that the explanation offered, in accordance with the theory of temperature and specific gravity, is neither complete, nor consistent with itself or with geographical observation. The theoretical objection of the infinitesimal nature of the thermometric gradients and of the differences of specific gravity, which has, indeed, formed the subject of these letters, is not one which I was inclined to put forward in any prominent degree. I preferred, and still prefer, to base my objection on the utter discrepancy between fact as observed, and fact as described by Captain Maury and Dr. Carpenter, in accordance with their theory.

I have elsewhere dwelt on this at great length, and do not intend to go over the same ground here, even if you were willing to afford me the space to do so; but this discrepancy, which actually and very markedly exists, does call attention to the thermometric gradients in the ocean; and when we find the same discrepancy between observation and description in the case of aërial currents, it leads to the conclusion that the infinitesimal nature of the thermometric gradients is as sound an objection to the temperature theory of atmospheric circulation, as it is to the temperature theory of oceanic circulation. I refer here to the last sentence but one of Mr. Proctor's letter. The last sentence, I must confess, I do not understand. I do not see what effects solar light can produce, or even be supposed to produce, on the depths of ocean, to which no light penetrates; still less do I see how to integrate them.

J. K. LAUGHTON

### Formation of Flints

IN your report of the discussion that followed the reading of my paper on Flint, before the Geologists' Association on June 2nd, Prof. Morris is said to have asserted that the views I suggested were first propounded by Dr. Brown of Edinburgh. I think the Professor must have been slightly misrepresented in this; at all events I must most decidedly decline to be coupled with Dr. Brown, or to allow myself to be associated with his very remarkable statements. These may be found in the Trans. Roy. Soc. Edinb., vol. XV. He asserts that carbon is transmutable into silicon; at p. 229 he says, "Carbon and silicon are isomeric bodies, and that the former element may be converted into a substance presenting all the properties of the latter." At p. 244, "3·04 grains of silicic acid were extracted from 5 grains of paracyanide of iron;" at p. 245, "5·4 grains of silicic acid were procured from 30 grains of the ferrocyanide of potassium," and "there were obtained 9,334 grains of silica from 3,240 grains of ferrocyanide, although some of the product was lost in two of the operations." The view I advocated as explanatory of the formation of flints was the *substitution* of silicon for carbon, not a transmutation, and I distinctly showed the source from which the silicon was derived. Dr. Brown's statements are so extraordinary that I could scarcely believe them serious. I find, however, in the same volume of the "Transactions" that they were most patiently examined and confuted by Dr. George Wilson and Mr. John Crombie Brown, and they say, "We tried the greater number of Dr. Brown's processes, and rejected them one after another without pursuing their investigation further, on finding they would not yield quantitative proofs of the conversion of carbon into silicon. The limited time, which from various circumstances we could devote to the subject, obliged us to follow this course; and the confident expectation we entertained till a recent period that each new process would supply what the rejected ones had failed to afford, led us to neglect noting many particulars of our early trials which otherwise we should have recorded. . . . In conclusion, we need scarcely say that we have been unable to supply any proof of the transmutability of carbon into silicon."

I have one more objection to make to the report. I did not say that flints were merely silicified sponges. I believe that such is the case with *some* flints, but certainly not with all. I hope you will find space for this rectification of manifest errors.

M. HAWKINS JOHNSON

379, Euston Road, July 11

### Affinities of the Sponges

MR. PARFITT seems to think that Mr. Carter has done Prof. Greene some injustice, because he has not referred to him as an original investigator of the Sponges, and he bases his opinion on the figures in Prof. Greene's "Manual of Protozoa," urging that the only difference between the forms figured by Carter in 1871, and those by Greene in 1859, is "the want of the funnel-shaped mouth, which seemed to have escaped the observation of Prof. Greene, probably owing to want of definition in the instrument used in the investigations." Allow me to point out that there is no pretence of originality in Prof. Greene's useful manual, that the figures alluded to are acknowledged (p. 85) to be copied from those illustrating the papers by Williamson and Dobie, and to express the opinion that much further research is necessary before the affinities of the sponges can be regarded as satisfactorily settled. When that day comes there is little doubt that a good deal of what is now guess work will require to be completely sponged out. W.

### Sun-Spot

WHILE watching the sun set over the hills to the west of Halifax, on the evening of July 17, my attention was called to an intensely black spot upon its southern hemisphere, almost vertically below the centre of the disc, which was visible to the naked eye. I may add that the evening was fine, but a thin mist was rising from the valleys, and that it was about five minutes before the sun touched the horizon that the spot was first seen. THOMAS PERKINS

## EDOUARD RENÉ CLAPARÈDE

AT the early age of thirty-nine, one of the most skilful, laborious, and honoured of European zoologists has been lost to Science in the person of Edouard Claparède. For the last three years his health has been such that his friends continually feared to receive the sad news which has at length come from Italy. In spite of a complication of pulmonary and cardiac disease, his indomitable spirit had kept the man at work to the last. Having taken up his residence in Italy for the benefit of his health, he produced during the last three years of his life a series of memoirs, so richly illustrated, and exhibiting such astonishing industry, that one would have fancied a man in full health and vigour was unequal to such abundant fertility. He once remarked to a friend, who expressed surprise that a man in his precarious state of health should work so hard, that he felt work was the only thing which kept him to life, if he left off working he should die at once. Claparède was a native of Switzerland, and a pupil of that great master of great zoologists, Johannes Müller. He could write French and German equally well, and consequently some of his researches are to be found published in the German periodicals, others in French in the Transactions of the Academy of Geneva. His earliest published work of large size is the "Recherches sur les Infusoires," which he produced in conjunction with his friend Lachmann, who unhappily died before it was completed. Though now to a great extent superseded by the later researches of Stein, Zenker, Cohn, and others, working with more accurate instruments, this treatise is one of classical importance, and forms the foundation of modern views on the Infusoria. Not long after the publication of this work, Claparède came to England, where he made the acquaintance of Dr. Carpenter, and spent a portion of the summer in his company in the Hebrides, working with the microscope, chiefly at the lower worms and annelids. From this expedition resulted a quarto publication, illustrated with plates (published by the Geneva Academy), giving accounts of new marine worms allied to the earth-worms, and many valuable observations on the Turbellarian worms. In conjunction with Dr. Carpenter, he also published some observations on the curious *Tomopteris onisciformis* in the Linnean Transactions. Attracted by the limicolous annelids, Claparède continued his observations on the forms of this group inhabiting the streams around Geneva; and his "Recherches sur les Oligochêtes," also published by the Geneva Academy, furnished zoologists with a very complete account of many of the anatomical and systematic differentiæ of these worms, till then almost entirely neglected and misunderstood. In this work the homology of the segmental organ with the reproductive ducts was demonstrated. The circulation of spiders, which he studied in the transparent young of the genus *Lycosia*, and the development of the freshwater gasteropod, *Neritina fluviatilis*, also about this time furnished occupation for his pen and pencil; and an elaborate work on the development of the Nematods, in which the important questions of the signification of the parts of the egg are discussed, was completed by him. In the collections of miscellaneous observations, always finely illustrated, which he from time to time published, such as "Glanures zootomiques," "Beobachtungen über wirbellosen Thiere," &c.," he recorded observations principally on the Annelids and free-living worms, which he made from year to year on the coasts of Normandy or the shores of the Mediterranean, and many strange forms, paradoxical marine larvæ, and unsuspected annectant genera, are briefly figured and described, which excite the interest of the zoologist, and awaken the desire to know more of them; whilst in other cases new modes of reproduction, new anatomical details, or physiological observations are related (for Claparède was no narrow zoologist) of rare and little known forms. The great work which he took in hand after his health had compelled him to reside in a warm climate during winter, was the study of the Annelids of the Bay of Naples. Under this title he has left two thick quarto volumes, illustrated by more than fifty coloured plates, consisting of anatomical and enlarged coloured drawings of these beautiful worms. Many new and curious forms were added by one winter's work to the known species of the Annelida; but his work is even more valuable for the anatomical and histological observations which are there recorded, and for the great critical ability displayed in dealing with the perplexing question of synonymy. M. Claparède appears to have found especial pleasure in doing justice to Delle Chiaje, who preceded him in the investigation of the fauna of the Bay of Naples; whilst he does equal justice to M. de Quatrefages, whose errors in a recently-published "Histoire des Annelées" he does not hesitate repeatedly in the course of his book to expose; whilst dedicating the first volume of his work to that distinguished French naturalist, and naming many new species in his honour. Whilst this splendid work on the Neapolitan Annelids was in press, M. Claparède also gave to the world some very interesting studies on Acarids (published in German), in which many new facts are detailed, and the Darwinian theory, in the manner of Fritz Müller, is shown to furnish a satisfactory explanation of the modification of dissimilar parts in different genera, to form identical organs. During the same period he also published in the *Zeitschrift für wiss. Zoologie* a memoir on the histology of the earth-worm, illustrated with nine coloured plates, which is certainly the most minute and careful piece of work which he ever produced. The structure of the nervous system and of the three "riesige Rohren-faden," soon to become very celebrated in zoological circles, are here for the first time fully described; and, indeed, the subject had been so slightly handled before that the whole work abounds with new matter. M. Claparède's last published paper appeared this year in the *Zeitschrift*, and as if to show that he did not intend to abandon himself to the study of one group, consisted of observations on the anatomy and reproduction of some marine polyzoa, illustrated by three coloured plates, drawn with his accustomed facility and grace. He has, we understand, left behind him ready for publication a large work on the Embryology of Insects, and an immense collection of microscopic preparations, of great value, of Annelids. Perhaps the most striking discovery recorded in any of M. Claparède's writings (which should however, be judged by the accumulated value of their immense number of anatomical observations) is one among those relating to the Annelids of the Bay of Naples. Claparède found that the *Nereis Dumerilii* lays eggs, sexually fertilised, which, on hatching, produce a worm which had been placed in quite a distinct genus (*Heteronereis*), and this worm lays similar true eggs, which produce sometimes a second kind of *Heteronereis*, or at other seasons the original form *Nereis Dumerilii* again. The difference between *Heteronereis* and *Nereis* is very great, and one extending into such details as the form of the setæ of the feet. At present this appears to be the only *real* case of alternation of generations on record, if, by "generations," we understand "sexual generations."

Whilst working so largely as an original observer, M. Claparède occupied himself also in reviewing the labours of others from time to time in the *Archives Suisses* published at Lausanne. Though holding the title of Professor in the Academy of Geneva, we believe he never (certainly not of late years) gave any public lectures on zoology; yet that he was admirably fitted for such work, had he thought fit to devote his time to it, is evident from the admirable style of his writings, especially the reviews and criticisms published in the *Archives Suisses*. His criticism of Mr. Wallace's views on the Descent of Man is known to our readers. Having access to the French world of science as a speaker and writer of the French language, and being thoroughly familiar with German writings and thought, both from education and continued association, M. Claparède appears to have taken an honest delight in every now and then dealing a severe blow at some one or other of the French naturalists who might venture to exhibit superficiality or dishonesty in his field of study. Dujardin is roughly handled in the "Recherches sur les Infusoires;" Rouget also, who appears to have personally resented the correction. Balbiani's researches on the development of the aphides are made the subject of special investigation by M. Claparède, who, three years since, studied the embryology of a species of aphis at Naples solely with the view of testing some extraordinary statements then recently advanced by the French doctor, and came to the conclusion that they were utterly unfounded, and that M. Balbiani had not done justice to the work of his predecessors, which conclusions he stated in very plain language. The attack on M. de Quatrefages, gracefully made and richly deserved, was perhaps the most entertaining. For M. de Quatrefages, charged to present to the French Academy the work which was dedicated to him, and in which, while his good work was appreciated, his errors were exposed, thought it advisable to reply to some of M. Claparède's criticisms, and displayed some temper, and even hinted that the dedication was objectionable. The sequel to this is to be found in the dedication of the second volume of the "Annélides du Golfe de Naples." It is dedicated to Delle Chiaje. Perhaps, says M. Claparède, were he alive he would object to this dedication; he would see with regret many of his errors pointed out; although so much of his work is here confirmed, human vanity would suggest to him to refuse the dedication of a work, to which, however, posterity considers he is justly entitled. It is, he concludes, easier sometimes to dedicate a book to a dead than to a living man.

The ardent naturalist, the accurate observer, the brilliant artist, the keen critic, the lucid exponent, has ceased his work, but has left a name which may well cheer the most faint-hearted among us—even those who feel to want the physical vigour of their fellows—for it is to be remembered that the works which do most honour to the name of Edouard Claparède were the labours of a dying man.

E. R. L.

## ALEXANDER KEITH JOHNSTON, LL.D.

A MEMOIR of Mr. Johnston would be the record of a life laboriously and successfully devoted to the spread and popularisation of a single science. Mr. Johnston's first maps, the result of a walking excursion through the north of Scotland, appeared in 1830, and were issued in a Traveller's Guide-Book. His first large work was the "National Atlas," folio, on which he was assiduously engaged for upwards of five years, having projected and drawn the greater part of the maps (forty-five in number) and written nearly all the names they contain with his own hand. This work went through many editions, and was considered the best of its time.

Having, in the course of his residence in Germany, been much interested in the writings of Ritter, Humboldt, and Berghaus, on Physical Geography, and having learned that Humboldt had expressed a desire to see an English physical atlas constructed in a manner suited to the taste of the British public, and on a scale sufficient to admit of entering fully on the details of physical phenomena, Mr. Johnston visited Germany in 1842, travelling from Hamburgh to Vienna, collecting materials for such a work, and making arrangements for an extensive correspondence.

Previous to the commencement of Keith Johnston's Atlas, Physical Geography was an unknown term in Britain. Hence it was predicted that the work would be a failure, and it required great faith to enable him to persevere in his self-imposed task. He was unfortunate in his first publisher, who was not able to do much with so expensive a work; however, the first edition was sold off, and a second was called for, and published in 1856. The two editions occupied Mr. Johnston ten years of the best period of his life. These writings procured for him, in 1850, a Fellowship in the Royal Society of Edinburgh.

In 1850 appeared the first edition of his "Dictionary of Geography, Descriptive, Physical, Statistical, and Historical," 1 vol. 8vo., on a new plan, embracing numerous facts in the different branches of science not before noticed in similar works.

In 1851 the author constructed a Physical Globe of the earth, thirty inches in diameter, showing in colours its Geology, Hydrography, Meteorology, &c. &c. For this, the first Physical Globe ever drawn, the medal of the Great Exhibition of 1851 was awarded. The globe was not intended to be published.

Between 1851 and 1855, he constructed and published for educational purposes four Atlases, royal 8vo.—namely, General, Classical, Physical, and Astronomical, and one Elementary Atlas, small 4to. All these have been improved, some of them re-engraved, and as many as from five to thirty editions of each have been published, of 1,000 copies each. In 1852 he prepared an Atlas of Military Geography to accompany Alison's "History of Europe," 1 vol. 4to. This work was most favourably reviewed, and commended by military men.

In 1855 was commenced the "Royal Atlas of Modern Geography," on which the author brought to bear the geographical experience gained during the labours of a quarter of a century.

In 1865 the University of Edinburgh conferred on him the Honorary Degree of Doctor of Laws.

During the last four years Mr. Keith Johnston was engaged in the production and increase of a complete series of geographic works for schools.

From the brief notice which appeared in our last number, it will be seen that Mr. Johnston may be said to have died in harness, his active labours having been carried on till the close of his life.

## PAPERS ON IRON AND STEEL

### V.—THE BESSEMER PROCESS (*continued*)

In the previous papers I have described the phenomena presented during the different stages of the blow, and have endeavoured to explain the chemical actions upon which they depend. The next stage, that of adding the molten spiegeleisen to the iron which has been fully acted upon by the blast, also presents some interesting phenomena which have not hitherto been fully examined.

In a paper on "Burnt Iron and Burnt Steel," read before the Chemical Society 6th April last,* I showed that the "burnt iron" of the workman is really what its name implies, viz., iron which has been more or less oxidised throughout its substance, and that "burnt steel" is quite different,—that the presence of combined carbon in sufficient quantity effectually protects iron from oxidation by heat.

These conclusions are strikingly illustrated in the Bessemer process. In spite of the excessively high temperature and the abundant supply of oxygen during the blast producing most violent combustion of the material in the converter, I have found no "burnt iron" during the early or middle stages of the blow. This only appears at quite the latter stages when the carbon is nearly all burnt out. At the termination of the blow, the material left in the converter is burnt iron of a very exaggerated type in all cases where the burning out of the carbon has been carried to its full extent.

Mr. Bessemer failed in his attempts to produce malleable iron by his process, and all subsequent attempts have equally failed, even when the very finest qualities of hæmatite pig-iron have been used. I am not aware that any explanation of this has yet been given, but have no doubt that it depends upon the principle above stated, viz., that some combined carbon is absolutely necessary to preserve the iron from oxidation, and thus, as the carbon is removed, the iron begins to oxidise throughout, and we have an incoherent mixture of iron and particles of oxides, which crushes under the hammer or the rolls, is neither malleable nor ductile.

The degree of rottenness depends upon the extent to which the blow has been carried, and the iron thus produced varies from a quality which simply cracks at the edges when hammered or rolled, to a mass that crushes into granules like a piece of coarse sandstone. If inattention or some hitch in the machinery prevents the immediate turning over of the converter, and the blow is continued a few minutes too long, the amount of oxidation is so considerable that the mass in the converter loses its fluidity, and becomes a spongy and pasty mixture of melted iron and infusible oxide, which is rather troublesome to the manufacturer.

By the simple method described in the paper above-referred to, I have been able at once to detect the presence of entangled particles of oxide in the midst of the iron remaining in the converter at the end of the blow. They are even visible on the fracture of overblown iron.

The presence of this free oxide explains some otherwise inexplicable phenomena which accompany the pouring of the spiegeleisen. A furious ebullition of the molten metal takes place, jets of burning carbonic oxide spurt up violently from all parts of the surface; the converter is filled with the blue flame which pours forth from its mouth, producing the weird illumination I have already described. The outpouring flame so completely occupies the whole dimensions of the mouth of the converter, that no air can possibly enter, and thus all the oxygen required for the combustion which is going on must be derived from the material inside the converter. Some of the carbon of the spiegeleisen is thus burning at the expense of the oxide of the original charge, and this oxide is thereby reduced.

The sole function usually attributed to the spiegeleisen is that of converting the iron into steel; but if the above be correct, it performs, in addition to this, the important service of reducing the free oxide of the rotten burnt iron, and thereby rendering it malleable. We shall now understand why Mr. Bessemer and others have failed to produce malleable iron by directly oxidising the silicon, carbon, &c., of the pig-iron in the converter. It may be asked how then does the puddler remove the carbon from pig iron? My answer is simply that he does it by a far less violent process of oxidation; that towards the end of his work when the iron is "coming to nature," *i.e.*, when the proportion of protecting carbon has become very small, he takes especial precautions by closing the dampers, and otherwise diminishes the rate of oxidation as much as possible, and thus he is able to work down to less than $\frac{1}{10}$ per cent. of carbon without burning his iron.

The more violent oxidising agency of the Bessemer blast demands a greater quantity of carbon for the protection of the iron, and accordingly it is found that about 0·25 per cent. is the minimum limit of carbon which is practically obtainable without sacrifice of malleability. I have determined the carbon of many hundreds of samples of Bessemer steel which has been specially made as "mild" as possible, where it was a primary object to reach the minimum proportion of carbon, and have never found any sound metal to contain less than 0·20 per cent. The usual range of this (which is sometimes called "Bessemer metal" being scarcely steel although not true iron) is from 0·25 to 0·30 per cent. of carbon. I do not here speak of the limits of absolute possibility, but of the practical limits of the process as at present conducted.

I should add that, in the course of subsequent working the proportion of carbon is reduced, but the extent of this reduction is very variable, depending on the number of re-heatings, the amount of surface exposed, and the kind of furnace in which the reheating is conducted. By using a reducing flame the oxidation of the carbon may be wholly prevented, but in the ordinary reheating or mill furnace and in the exposure of rolling, &c., a certain amount of oxidation commonly occurs. Rails and tyres usually contain two or three hundredths per cent. less than the ingots from which they were made, thin plates and sheets lose a larger proportion, even as much as one-tenth per cent. in extreme instances. I have removed the whole of the carbon from the surface of a hard steel plate by exposing it for several days to the low red heat of an annealing furnace.

W. MATTIEU WILLIAMS

* An abstract of this paper will be found in NATURE, April 20, p. 497.

## THE CAUSE OF LOW BAROMETER IN THE POLAR REGIONS AND IN THE CENTRAL PART OF CYCLONES

In none of the treatises on Meteorology or Physical Geography is there to be found any satisfactory explanation of the observed low barometer in the polar regions, or in the centre of a cyclone. Observations show that in the Antarctic region there is a permanent depression of more than one inch below the average height nearer the equator, and in the Arctic region a depression of about half that amount; and also that for several days frequently the barometric pressure of the central part of a cyclone is one or two inches less than that of the exterior part. Mr. Buchan, in his excellent treatise on Meteorology, attributes the low barometer in the polar regions to the effect of the vapour in the atmosphere. If the amount of vapour in the polar regions was greater than in the equatorial, this effect, so far as it would go, would be in the right direction; but just the reverse is the case; for it is well known that the amount of vapour in the warm equatorial region is much in excess of that in the cold polar regions. Attempts have also been made, without success, to account for the depression in cyclones by the effect of centrifugal force.

By whatever cause so great a difference in the barometric pressure in the different regions might be produced, it may be shown from the principles of dynamics that the equilibrium would be restored in a very short time, if there was not some constant force tending to drive the atmosphere from the polar regions towards the equator, or from the centre of the cyclone to the exterior, and to keep it in that position. Such a force may be found in the influence of the earth's rotation. In a paper by the writer in the *Mathematical Monthly* in 1860, published in Cambridge, U.S., a full abstract of which was also published in the January No. of *Silliman's Journal* for 1861, the following very important principle was demonstrated:—In whatever direction a body moves on the surface of the earth, there is a force arising from the influence of the earth's rotation, which tends to deflect the body to the right in the northern hemisphere, and to the left in the southern hemisphere. This force, which is the key to the explanation of many phenomena in connection with the winds and currents of the ocean, does not seem to be understood by meteorologists and writers on physical geography. We see it frequently stated that the drift of rivers and currents of the ocean running north or south always tends to the right in our hemisphere, and that a railroad car running north or south presses to the right; and this is the case. But the same is true, and to exactly the same amount, of a current or of a railroad car running east or west, or in any other direction.

The amount of this deflecting force, when the velocity of the body is small in comparison with that of the earth's rotation, is expressed by $2 \cdot \frac{1}{289} \cdot \frac{v}{n} \cos \theta \, g$; in which $v$ is the lineal velocity of the body relatively to the earth's surface, $n$ that of the earth's rotation at the equator, $\theta$ the angle of polar distance, and $g$ the force of gravity. If the velocity is expressed in miles per hour, the expression in round numbers becomes $\frac{v \cos \theta}{150{,}000} g$; that is, for each mile of velocity per hour, the force is $\frac{1}{150{,}000}$ of gravity, multiplied into the cosine of the polar distance. Hence a railroad car on the parallel of 45° north, running in any direction at the rate of forty miles per hour, presses to the right with a force equal to about $\frac{1}{5{,}000}$ part of its weight.

The effect of this deflecting force upon what Mr. Stevenson calls the barometric gradient is easily estimated. Since the strata of equal pressure of the atmosphere, so far as this force is concerned, must be perpendicular to the resultant of this force and gravity, the sine of inclination of any such stratum to the earth's surface must be $\frac{v \cos \theta}{150{,}000}$, and the change in barometric pressure for any given distance is equal to the weight of a column of atmosphere of a height equal to the change of level of the stratum of equal pressure, and of a density equal to that at the earth's surface. The barometric gradient, then, as expressed by Mr. Stevenson, for any distance $d$ expressed in miles is $\frac{v \cos \theta \, d}{5 \times 150{,}000} \times 30$ inches; putting five miles for the height of a homogeneous atmosphere, and thirty inches for the pressure at the earth's surface. Round numbers are used throughout, since it is only the order of the effects we wish to determine, and not their exact amount.

According to all observations, there is a steady and very strong wind blowing all around the earth in the middle and higher latitudes of the southern hemisphere, with a velocity of at least twenty-five or thirty miles per hour at the surface of the ocean, and this is perhaps much greater in the upper strata of the atmosphere. If at the parallel of 50° we suppose the velocity of the wind $v$ to be thirty miles per hour, the preceding expression of the barometric gradient for a distance $d$ of 5° or 350 miles, using the cosine of 40°, is 0·33 inches of mercury. By reference to § 113 of Mr. Buchan's Meteorology, it will be seen that the barometric gradient for that parallel is only 0·28 inches for 5° of latitude, and that this is about the maximum gradient in the southern hemisphere. Hence a velocity less than 30 miles per hour at the surface of the sea, especially if we suppose that it increases in the higher regions, is sufficient to account for this maximum barometric gradient; and, according to observations, 20 or 30 miles per hour for the wind in that region is no unreasonable assumption. The eastward velocity of the wind in the different latitudes being known, and, consequently, the corresponding barometric gradients, the difference of barometric pressure between any parallel near the pole and one toward the equator, is readily obtained by integration. As the wind near the equator is toward the west the deflecting force there is *toward* instead of *from* the pole, and hence the greatest barometric pressure is about the parallel of 30°, and there is a slight depression at the equator. The deflecting force and the consequent depression are small, then, on account of the small value of $\theta$ near the equator.

Since there is more land and mountain ranges in the northern than in the southern hemisphere to obstruct the eastward motion of the atmosphere, its velocity is not so great, and consequently the polar depression is much less there than in the southern hemisphere. According to Mr. Buchan the barometric depression in the Arctic regions is much greater in the northern part of both the Atlantic and Pacific oceans, than it is in the same latitudes on the continents. The explanation of this is, that the eastward velocity of the atmosphere over the oceans being much greater than it is on the continents, where it is obstructed more by friction and mountain ranges, the force driving the atmosphere from the poles toward the equator is less, and consequently the barometric pressure is less in the northern part of both oceans than it is on the continents in the same latitudes.

Upon the relative strength of the forces tending to drive the atmosphere from the poles towards the equator, depend the positions of the equatorial and the tropical calm belts. This force being strongest in the southern hemisphere on account of less resistance from friction and mountain ranges, the mean position of the equatorial calm belt is a little north of the equator, and the positions of the others a little farther north than they would otherwise be. The prime motive power also in both hemispheres being the difference of density of the atmosphere between the polar and the equatorial regions, arising from a difference of temperature and of the amount of aqueous vapour, during our summer when this difference is less than the average in the northern hemisphere, and greater in the southern, these calm belts are forced a little north of their mean positions. Of course, just the reverse of this happens during our winter; hence we have an explanation of the annual variations of the positions of these belts.

In the case of cyclones, the atmosphere at the earth's surface being forced in from all sides towards the centre, by the force arising from a difference of density of the atmosphere in the central and exterior parts, it cannot, on account of the deflecting force which has been explained, move toward the centre, without, at the same time, receiving a gyratory motion around that centre. Neither can it have a gyratory motion without also having a motion towards that centre, since in that case there would be no force to overcome the frictions of gyration. Hence, neither the radial theory of Espy, nor the strictly gyrating theory of Reid and others, can be true, though either of them may be approximately so in special cases. But the gyratory part of the motion is not caused by the motion of the atmosphere from the north and south only toward the centre of the cyclone, as stated by Mr. Buchan and others, but equally by the different parts moving in from all sides, since in whatever direction they move toward the centre there is the same deflecting force, either to the right or the left according to the hemisphere.

The motion of the atmosphere being in a spiral toward and around the centre of the cyclone, the deflecting force depending upon the earth's rotation, at right angles to the direction of motion, being resolved in the directions of the radius of gyration and tangent, the latter overcomes the friction of gyration, and the former causes a pressure from the centre, decreasing the height of the strata of equal pressure in the cyclone, and consequently diminishing the barometric pressure. The barometric gradient of a cyclone is estimated in precisely the same way as in the case of the hemispheres, using for $v$ the lineal velocity of gyration obtained by resolving the real motion into the directions of the tangent of gyration and of the radius. It has been seen that a velocity of 30 miles per hour gives a barometric gradient of $\frac{1}{3}$ of an inch in 350 miles on the parallel of 50. A gyratory velocity therefore of 100 miles per hour would give a barometric gradient of one inch of mercury in about 300 miles. The velocities of gyration being known at all distances from the centre of motion, and consequently the barometric gradients, the difference of barometric pressure between the centre and the exterior, so far as it depends upon the gyratory motion, may be obtained by integration. The effect of the centrifugal force of the gyrations is generally only a very small quantity of a second order, in comparison with the other, and the effect of it is entirely insensible, except in the case of small tornadoes, when the gyrations are very rapid close around the centre.

In all the preceding estimates of the barometric gradient, it should be understood that the results belong merely to the force depending upon the earth's rotation, and to this must be added the part belonging to a difference of density of the atmosphere, which in the case of cyclones increases the gradient, but diminishes it in the case of the hemisphere. For the general motions of each hemisphere form a cyclone, with the pole as a centre; but having the denser instead of the rarer portion of the atmosphere at that centre. Hence the motions in any vertical plane through the centre are reversed, and it becomes what has been called an anti-cyclone.

Cambridge, Mass. WM. FERREL

## RECENT MOA REMAINS IN NEW ZEALAND

II.

THE Moa's neck with the integuments attached, the discovery of which was announced in my communication dated April 3, has since then been forwarded to this Museum for examination by Dr. Thompson, and the following particulars may not be without interest to your readers.

The total length of the specimen is 16·5 inches, and includes the first dorsal and last six cervical vertebræ with the integuments and shrivelled tissues enveloping them on the left side. The surfaces of the bones on the right side, where not covered by the integuments, are free from all membranes and other tissues, but are quite perfect and in good preservation, without being in the least degree mineralised.

The margin of the fragment of skin is sharply defined along the dorsal edge, but elsewhere it is soft, easily pulverised, and passes into adipocere.

The circumference of the neck of the bird at the upper part of the specimen appears to have been about 18 inches, and the thickness of the skin about $\frac{1}{16}$ of an inch.

The only indication of the kind of matrix in which it had been imbedded was a fine micaceous sand, which covered every part of the specimen like dust, there being no clay or other adherent matrix. On removing this sand with a soft brush from the skin, it was discovered to be of a dirty red-brown colour, and to form deep tranverse folds, especially towards the upper part. The surface is roughened by elevated conical papillæ, from the apex of some of which springs a slender transparent feather barrel, never longer than half an inch. On the dorsal surface a few of these quills still carry fragments of the webs, some being 2 inches in length. From these it appears that the colour of the feather barbs was chesnut-red, as in Apterix Australis, but that each barrel had two equal plumules to each quill, as in the Emu and Cassowary, and in this respect differed from the Apterix, the feathers of which have not even an accessary plumule. On the other hand the barbs of the webs of the feathers do not seem to be soft and downy towards the base as in the Emu.

From the direction of the stumps of the feathers, it is evident that the portion of the neck which has been preserved is that contained within the trunk of the body, and which, in the natural position, has a downward slope, the conical end of the specimen being where the upward sweep of the neck of the bird commenced, which accounts for the absence of the trachea with its hard bony rings, none of which are found among the soft parts which have been preserved.

The integument was easily removed by dividing the few threads of dried tissue by which it was attached. The shrivelled-up soft parts thus displayed could not be clearly distinguished, but may be grouped as follows:—1. A strong band of ligamentous tissue connecting the spinous processes. 2. Inter-vertebral muscles and ligaments. 3. A sheath diverging from the lower part as if to enclose the thorax. The only bone besides the vertebræ was attached to this sheath by its tip, the other extremity being articulated to the first dorsal.

Respecting the nature of the circumstances to which this remarkable specimen owes its preservation, I can only conjecture that the body of the bird must for a considerable period have lain on its side in water or a swamp, and that the portion immersed was thoroughly macerated, while the exposed parts were desiccated and shrivelled up; and that subsequently the whole remains were embedded in dry sand.

As a fact of some interest connected with the history of the Moa, I should mention that in December last, Archdeacon Williams informed me of the discovery of a series of enormous bird-foot marks on the surface of a layer of sand beneath a bed of alluvium at Poverty Bay. The specimens he collected for me have unfortunately gone astray, but others have been placed in the Museums in Auckland and Napier, and I have just seen a pencil rubbing from the latter, taken by Mr. Cockburn Hood, which leaves no doubt that they are the footprints of a bird like the smaller-sized species of Dinornis, the largest of these footprints being about eight inches in length.

JAMES HECTOR

Colonial Museum, Wellington, New Zealand, May 15

[We exceedingly regret that we are unable to reproduce woodcuts of the beautiful illustrations by which Dr. Hector's article is accompanied.—ED.]

## NOTES

WE are glad to learn that our anticipations last week with reference to the Australian observations of the Total Solar Eclipse of December next are being realised. The Royal Society of New South Wales is organising an expedition to Cape Sidmouth, a little south of Cape York. The President of the Royal Society of London has arranged that a few instruments of the newest construction shall be sent out from this country.

IT perhaps is not so generally known as it ought to be that the Emperor of Brazil, now in this country, is an enthusiastic astronomer, and has an appreciation of the value of science which places him in the highest rank among reigning sovereigns. During the last week he has visited the Royal and Mr. Huggins's Observatory, and in a long interview with Mr. Lockyer has discussed the bearings of the recent solar discoveries.

The *Pall Mall Gazette* states that the Emperor Napoleon is about to visit Mr. R. S. Newall, whose magnificent refractor has already been described in these pages.

The Royal Commission on Scientific Instruction and the Advancement of Science adjourned on Tuesday last till November. We are informed that the publication of some of the evidence already taken may shortly be expected.

The first General Meeting of the approaching session of the British Association at Edinburgh will be held on Wednesday, August 2, at 8 P.M., when Prof. Huxley will resign the chair, and Prof. Sir William Thomson will assume the presidency, and deliver an address. On Thursday evening, August 3, at 8.30 P.M., there will be a Discourse by Prof. Abel on some recent Investigations and Applications of Explosive Agents; on Friday evening, August 4, at 8 P.M., a Soirée; on Monday evening, August 7, at 8.30 P.M., a Discourse by Mr. E. B. Tylor on the Relation of Primitive to Modern Civilisation; on Tuesday evening, August 8, at 8 P.M., a Soirée; on Wednesday, August 9, the concluding general meeting will be held at 2.30 P.M.

We are glad to notice the step recently taken by the Committee of St. Mary's Medical School, in establishing Scholarships in Natural Science, open to public competition. Through the very proper action of the Governors of the Hospital, the share of school fees formerly paid to the charity has been appropriated to the improvement of the school. By this means the Committee has been enabled not only to provide a tutor to assist the students in the practical portion of their work, but also to establish three Scholarships in Natural Science, each of the annual value of 40*l.*, and tenable for three years. The first of these, and an annual exhibition of 20*l.*, will be awarded by open competitive examination in September next. The tendency of these Scholarships will be to favour what we have so often advocated, the acquisition of a proper amount of scientific knowledge pre vious to entering upon a regular course of medical study.

The *Lancet* states that the Council of the College of Surgeons of England have withdrawn their opposition to that portion of the scheme of the College of Physicians which provides for the selection of examiners by a central Board composed of the representatives of the various licensing bodies and universities, and have agreed to give up the power of specially nominating examiners in special subjects. Thus the main difficulty in coming to an agreement upon the question of a conjoint examination has been removed. The Apothecaries' Hall will probably be left out in the cold.

The number of successful candidates at the recent Matriculation Examination at the University of London was 242, of whom only 30 passed in honours. This shows a larger proportion of failures than on any previous occasion, notwithstanding that the novel practice was introduced of optional questions, only a certain proportion being expected to be answered in some of the papers.

The Anniversary meeting of the Quekett Microscopical Club will be held on Friday the 28th inst., at 8 P.M., at University College, Gower Street.

The first number is issued of the "Journal of the Anthropological Institute of Great Britain and Ireland," being the first substantial result of the union of the two old societies, the Ethnological and the Anthropological Societies. We are glad to see this evidence of the concentration of power thus effected.

The Annual Meeting of the British Horological Institute was held on July 11, Mr. John Jones, vice-president, in the chair. The report of the Council for the past year was read by Mr. Henry Moore, resident secretary. One of the most interesting features of the report was the fact that the Baroness Burdett Coutts has, agreeably to that line of action for which she has been distinguished, volunteered a prize for the best essay on "The Balance Spring and its isochronal Adjustments." The Astronomer Royal, Sir C. Wheatstone, and Mr. James F. Cole will be the judges. The attention of the Lord Mayor, the head of the guilds of the City of London, bodies entrusted with power specially to promote purposes similar to those aimed at by the Institute, has been attracted by the efforts of the Institute, and he has promised to distribute the prizes to the successful students in horological drawing. Lectures were delivered in the past half year by Mr. Perrell, Mr. Herrmann, Mr. Charles Frodsham, and Mr. John Jas. Hall. The following are the chief officers elected for the ensuing year:—President: Mr. Edmund Beckett Denison, LL.D., Q C. Vice-presidents: Mr. C. I. Klaftenberger, E. D. Johnson, and John Jones. Treasurer: S. Jackson.

The Government of Bavaria has been long engaged in the publication of a History of Science in Germany. The following volumes have already appeared:—Bluntschli's History of Political Law; Kobell's of Mineralogy; Fraas's of Agriculture; Peschel's of Geography; Lotze's of Æsthetics; Benfey's of Philology; Raumer's of German Philology; Kopp's of Chemistry; and the following are in preparation:—Zeller's of Philosophy; Bursian's of Classic Philology; Bernhardi's of Military Science; Wegele's of History; Stintzing's of Jurisprudence; Karmarsch's of Technology; Gerhard's of Mathematics; Jolly's of Physics; Wolf's of Astronomy; Ewald's of Geology; Hirsch's of Medicine and Physiology; Carus's of Zoology. When may we look for anything of the kind from our enlightened Government?

With the July number of the Journal of the Franklin Institute of Philadelphia, Dr. W. H. Wahl becomes sole editor.

We have received the Catalogue of the Iowa State University for 1870-71. The students are divided into law, medical, normal, and academical departments, the students in all the departments, except that of law, being of both sexes, and some of the instructors being also ladies. The full course of instruction in the academical department occupies five years; during the first three years nearly the same course of study is followed by all the students; during the last two years the course is elective, either literary or scientific. The University is wholly sustained by endowments and state appropriations, the fees even for the medical classes being merely nominal. Good opportunity appears to be afforded for the practical study of natural and physical science, and the "School Laboratory of Physical Science," edited by Dr. G. Hinrichs, the Professor of Physical Science, is published at the University.

*Les Mondes* prints a list of the new taxes imposed by the French Government on articles of consumption and commerce.

An additional excursion of the Geologists' Association took place on the 10th and 11th inst. to Warwick and neighbourhood. The sections of the Lower Lias were examined, and the extensive quarries worked for material for hydraulic cement were visited. A special object of interest was the insect-beds occurring in the Lias at Wilmcote, near Stratford-on-Avon.

We learn from the *Journal de Méd. de l'Ouest*, and *Bull. Génér. de Thér.* that Dr. Weir Mitchell, from observations on the bite of the rattlesnake, and MM. Gicquain and Viaud Grand-Marais, from observations on that of the viper, have arrived at the conclusion that the application of carbolic acid immediately on the receipt of the injury prevents both local and general poisoning. The pure acid however, if applied in too great quantity, is liable to produce sloughing, and even dangerous symptoms; hence it is best used in the proportion of two parts of acid and one of alcohol. Given internally, or applied to the wound at a late period, it produces no effect. It is believed to act, not by neutralising the poison, but by causing contraction of the small vessels, and thus preventing its absorption.

THE following interesting account was published in *Notes and Queries* of August last year without eliciting any reply. Mr. Alexander Williams writes :—"As the Commissioner for Western Australia of the International Exhibition of 1862, I received from the Colonial Committee at Perth several specimens of native shields. The long narrow form of these implements of defence is common to all the Australian colonies I believe, but I cannot say whether the ornamentation is uniformly the same. But among the Swan River nation it consists of an oblong pattern (following the shape of the shield) composed of border within border, traced in different coloured paint. The late Mr. Christy called my attention to the exact similarity of these shields to those used by the natives of Central Africa—a similarity not only in shape and pattern but actually in the succession of colours in the pattern. How is this to be accounted for? It is possible (and no other theory seems admissible) that it is purely an accidental coincidence. It is perhaps not difficult of belief that the native mind in two races in all respects so utterly distinct should have hit upon the same shape and form of weapon to meet and throw off the common spear. It is even not very surprising that savages unacquainted with 'lines of beauty' should adopt the same crude form of ornamentation, but it is somewhat startling I think that they should have used apparently the same pigments, and very extraordinary as it appears to me that they should have adopted precisely the same succession of colours."

WHILST we have been literally overwhelmed with rain in this country for the last three months, it is interesting to hear that in Tientsin in China there was so little snow in the winter, and hardly any rain has fallen since, that the peasantry are complaining of the want of water, and consequent injury to the crops.

A SEVERE earthquake shock is reported from Brooklyn and Staten Island, New York, on the 19th of last month at about 10 P.M. No great amount of damage was done, and the motion appears vertical rather than horizontal in character.

THE *American Journal of Science* gives a long report of the severe earthquake at Oaha, Hawaian Islands, on February 18 of this year. It commenced at about 11 minutes past 10 P.M., and lasted about a minute. The direction of motion was vertical, with a rocking movement N.E. and S.W. The usual roaring sound preceded the earthquake and was heard far out at sea. No earthquake wave is reported from any quarter, although the earthquake itself seems to have been felt on all the islands more or less severely. No unusual volcanic action is reported. Slight shocks were also felt on the 22nd and 24th of the same month. It should be noticed that a severe earthquake is reported from Chile on the 25th, and shocks were noticed in Peru on the 22nd and 23rd of February.

THE existence of certain plants only in limited districts is one of the most remarkable points of interest in connection with the problem of the distribution of species. Mr. Moggridge, in his valuable "Contributions to the Flora of Mentone," figures a very elegant species of *Leucojum*, of which no drawing had hitherto been published. "It is believed to have but one habitat on the face of the earth, claiming only a small strip of rocky shore reaching from Nice to about two miles east of Mentone. *Leucojum hyemale* grows in a stony soil, and out of the cracks of the hardest limestone rocks at Port St. Louis, Cape Veglio, on the way to Monaco, and at some height on the Aggel mountain, besides other less abundant localities." We are not aware whether this species has been introduced to English gardens, but it would be a very desirable acquisition. At Mentone it flowers in April.

THE Ant-eating Woodpecker (*Melanerpes formicivorus*) a common Californian species, has a curious and peculiar method of laying up provision against the inclement season. Small round holes are dug in the bark of the pine and oak, into each one of which is inserted an acorn, and so tightly is it fitted or driven in, that it is with difficulty extracted. The bark of the pine trees, when thus filled, presents at a short distance the appearance of being studded with brass-headed nails. Stowed away in large quantities in this manner, the acorns not only supply the wants of the woodpecker, but the squirrels, mice, and jays avail themselves likewise of the fruits of its provident labour.

DR. GEORGE STUCKLEY gives an interesting account of the Western Mole (*Scalops Townsendii*), which occurs in the Oregon and Washington Territories. He kept a specimen for some time in a box, at the bottom of which was a quantity of rich black loam. When disturbed it instinctively endeavoured to escape by burrowing in the earth of the box, using its long-pointed nose as a wedge to pioneer the way. The excavation was performed by its broad stout hands, which, surmounted with their long sharp claws, seemed admirably adapted for the purpose. The fore paws were worked alternately as in swimming, the hind feet acting as propellers. Although the earth in the box was soft and friable, it was nevertheless a matter of astonishment to see how rapidly the little creature could travel through it. When he slept it was in a sitting posture, with the body curled forward and the neck strongly bent, so that the nose rested between the hind legs. He thus assumed the shape of a ball, evidently his ordinary position when asleep.

THE cultivation of the poppy in China, which has been more than once prohibited by Imperial edicts, appears to be increasing everywhere, and becoming a profitable trade. In Szechuen, where the climate is warm and the season early, two crops at least are produced on the same ground annually. The seed of the poppy is sown in February, the plants flower in April, and the fruits are so far matured by the middle of May, that the juice is collected, and the stalks removed and burnt directly after, but previous to this the second crop, which may be either Indian corn, cotton, or tobacco, is sown, so that almost by the time the poppy is cleared from the field the new crop makes its appearance. The profit derived from the cultivation of the poppy is not only the result of a fair market value and a ready sale, but also from the fact that much of the work in the plantation, especially the gathering of the juice, can be done by the children of the family. The scratchings or incisions being made in the capsules in the morning, the juice which has oozed out in the course of the day is collected in the evening, and after simply exposing it to the sun for a few days it is ready for packing. The seed not required for sowing is used for food.

## *ON THE RECENT SOLAR ECLIPSE**

I.

MY duty to-night, a pleasant one, although it is tinged with a certain sense of disappointment, is to bring before you the observations which were made of the recent eclipse in Spain and Sicily, to connect them with our former knowledge, and to show in what points our knowledge has been extended. In these observations, as you know, we had nothing to do with the sun as ordinarily visible, but with the most delicate phenomenon which becomes visible to us during eclipses. I refer to the Corona.

### *General Notions of the Corona*

Let me, in the first place, show you what is meant by this

* A Lecture delivered at the Royal Institution, Friday, March 17, 1871

term, and state the nature of the problems we had before us. I have here some admirable drawings, which I will show by means of the lamp, of the eclipse that was observed in 1851 by several astronomers who left England in that year to make observations in Sweden, where the eclipse was visible. You must bear in mind that the drawings I shall bring to your notice were made in the same region, at places not more than a few miles apart.* The first drawing was made by an observer whose name is a sufficient guarantee for its accuracy—I refer to Mr. Carrington—*and when the sky was absolutely free from clouds.* In the next diagram you will see the corona is changed. The bright region round the sun is no longer limited to the narrow border of light round the dark moon, as seen by Mr. Carrington, but it is considerably expanded. The third gives still a greater extension, although that picture was drawn within a quarter of a mile of the place where Mr. Carrington's was taken. And lastly, we have a drawing made by the present Astronomer Royal, of that same eclipse, *through a cirrostratus cloud* as unlike Mr. Carrington's as anything can possibly be. So that you see we began with a thin band of light about the moon, which would make the corona a few thousand miles high, and we end with a figure which Mr. Airy graphically likens to the ornament round a compass-card, and which gives the corona a height equal to about once and a half the sun's diameter.

I will next bring before you some drawings made during the eclipse of 1858, which was not observed in European regions, but in South America by two first-rate observers—one, M. Liais, a French astronomer, who was stationed at Olmos, in Brazil; the other, Lieutenant Gilliss, who was also there as a representative of the American Government, and observed some thousand miles away in Peru.

I will throw on the screen the appearances observed by these gentlemen, and I think you will acknowledge the same variations between their results, as to degree, while in one case we get a perfectly new idea of the phenomena—a difference in kind. I would especially call attention in the Olmos drawing to those extraordinary bundles of rays of wonderful shapes, which you see are so much brighter than the other portions of the corona. Such forms have been seen in other eclipses, but they are somewhat rare. The drawing made by Lieutenant Gilliss bears the same relation to that made by M. Liais as Mr. Carrington's did to the Astronomer Royal's; so that we may say that we not only get variations in the dimensions of the corona as seen at different stations, but that we furthermore get a strange structure introduced now and then in our drawing in regions where absolutely no corona at all exists in the other.

So much by way of defining the phenomena and giving an idea of the eye observations generally.

Let me now attempt to show you how the phenomena observed in the last eclipse bear upon the results which had been previously accumulated by means of telescopic and naked-eye observations, and by means of the polariscope and spectroscope.

## I.—Telescopic and Naked-eye Observations

### *a.—A Part of the Corona is undoubtedly Solar*

The first use I propose to make of the telescopic and naked-eye observations of last year is to show you a photographic copy of an admirable drawing made by Mr. Brett, who, though unfortunate enough to see the sun only for a very short time, was yet sufficiently skilled to make good use of that brief period. This drawing will bring before you the fact that even when a large portion of the sun remained unobscured by the moon, Mr. Brett was enabled to see a dim ring of light round the unobscured portion, which since the year 1722 has been acknowledged, beyond all question, I think I may say, to represent something *at the sun.* It was observed in 1722 round the uneclipsed sun, and in more recent times by Mrs. Airy in 1842, and by Rumker $1\frac{1}{2}$ minute before totality in 1860, not to mention other instances. Therefore we have one observation made during this eclipse, confirming the old one, that in the corona there is a region of some small breadth at all events which is absolutely solar, and which it only requires a diminution of the solar light to enable us to see. This, then, we may look upon as the known; now let us feel our way gradually outwards.

### *b.—Rays, or Streamers, are added at Totality*

The drawings made in all the eclipses which have been carefully recorded bring before us quite outside this narrow, undoubtedly *solar region*, observed before totality, as I have shown, and also by Mr. Carrington, and by Lieutenant Gilliss during totality in 1851 and 1858, extraordinary appearances of a different order. While in fact we have a solar ring from 2′ to 6′ high, we have rays of all shapes and sizes visible outside, in some cases extending as far as 4°, and in all cases brighter than the outer corona on which they are seen, the rays being different in different eclipses, and appearing differently to different observers of the same eclipse, and even at the same station. Here is a copy of a drawing made by M. Rumker of the eclipse of 1860, and I show it for the purpose of calling your attention to the fact that the two curious rays represented in it belong to a different order of things from those which we see in the rest of the corona. From the beginning to the middle of the eclipse the east rays were the most intense. In the next drawing, which was made by the same observer, you see something absolutely new; and now the western side of the corona is the most developed; we have a new series of bright rays, and altogether it is difficult to believe that it is a drawing made by the same observer of the same eclipse.

The third drawing is a representation of the same eclipse by M. Marquez, who observed with a perfection of minute care which has scarcely ever been equalled: I bring it before you to show that the rays he saw were altogether differently situated. We may conclude then that the rays, although extremely definite and bright—as bright or brighter than the other portions of the corona which are visible before totality, they being *invisible* before totality—appear different to different observers of the same eclipse, and to the same observer during different *phases.*

### *c.—They Change from Side to Side*

I have already said that M. Rumker observed that from the beginning to the middle of totality the rays on the *east* side of the sun were longest and brightest, and that from the middle to the end of totality the rays on that side of the sun where the totality ended were longest and brightest.

We will now carry this observation a step further, by referring to three drawings made by M. Plantamour in the same eclipse, that of 1860. In the first drawing we have the beginning of the total eclipse as seen in the telescope; with the naked eye naturally we should get the sun disappearing at the east or left-hand side, the moon moving from west to east; in the telescope things are reversed, and we have it right instead of left: and here we have the same thing that M. Rumker observed, namely, that when the eastern limbs were in contact, bright rays (M. Plantamour saw three) were visible on the side at which the contact took place. When the moon was half way over the sun, *two* rays of reduced brilliancy were observed on that side, not necessarily in the same position as *those* first observed, but one of *these* has been abolished altogether; and on the other side of the sun, where totality was about to end, we have three rays gradually suggesting themselves: at the end of totality the rays visible at the commencement are abolished, and now instead of them and of those seen at the middle of the eclipse, we have a bran new set of rays on the side of the moon from whence the sun is about to emerge.

This observation I need hardly say is of considerable importance in connection with the fact that from the year 1722 almost every observer of a total eclipse has stated that there is a large increase of brilliancy, and an increase of the size of the corona on the side where the sun has just been covered, or is just about to emerge.

Now, what was there bearing on this point in the recent observations? I have here three drawings, which, though roughly done, you will see are of great importance side by side with those of M. Plantamour. These are drawings which have been sent in to the Organising Committee by Mr. Gilman, who lives in Spain, and who took considerable interest in the eclipse, and sent the results of his observations to England with the eclipse party when they came home; and it is of importance that you should see everything that Mr. Gilman has done. If you agree with this explanation of the square form of the corona, which was observed in Spain this year, it will explainl the quadrangular form observed in the corona in a good

* Mr. Carrington observed at Lilla Edet, on the Grota River. The Astronomer Royal observed at Göttenburg. The second drawing referred to was made by Pettersen, at Göttenburg; the third by a friend of the Rev. T. Chevalier, at the same place; and I might have added another by Fearnley, taken at Rixhöft, in which the corona is larger than in any of the others. The series is most instructive. See Mem. R. A. S. vol. xxi.

many previous eclipses. Mr. Gilman says that at the commencement of totality—let me remind you, the commencement was determined by the disappearance of the sun at the east limb of the moon, which is east in Mr. Gilman's drawing, as he was observing with the naked eye—the commencement, he says, was determined by the corona flashing out very much like a capital D. You see on the black board exactly the outline, and you will at once mentally associate one half of the diagram with the rays observed by M. Plantamour, and the other half, in which there is a nearly perfect ring of light round the moon, with the corona observed by Mr. Carrington all round it in a cloudless sky. At mid-eclipse Mr. Gilman also observed the corona, sketched out its outline carefully, and found rays coming out on the opposite side, adding themselves on to the perfect ring first seen there. Opposite the two salient angles he observed at the commencement of totality—represented by the top and bottom of the upright stroke of the capital D—there were two others; *the corona now appeared square*, and then, just before the end of totality came on, the two corners first seen were observed to disappear altogether, leaving nothing but a perfect ring, and where, at the beginning of the eclipse, nothing was seen but a perfectly round ring, the two exactly similar forms on the opposite side shot forth, and you got a D reversed (ᗡ). Mr. Warrington Smyth, who drew a square corona, saw the light flash out into the corona before the end of totality, and believes that all the angles of the square were not visible at one and the same time.

Here, then, you have observations of exactly the same character as those of M. Plantamour, to which I have referred. In the drawings of both are shown the inner part of the corona, which you saw growing in the observations of 1851, to which were added the strange forms observed in 1858. You have these strange variations positively growing at the same place and the same time, in the same and in different eyes. Obviously there must be very much that is non-solar, call it personality, atmospheric effect, or what you will, connected with it. We have added to the stable the unstable. The question is, to what is this unstable portion due?

*d.—They are very variously represented*

I will now refer to other drawings of the late eclipse, which were made in Sicily. For some reason or other, which I do not profess to understand, the corona, which appeared in Spain to be square, and to Mr. Gilman like a D at the beginning, and like a D reversed (ᗡ) at the end,—to all those with whom I have conversed who saw it in Sicily, it appeared as round as you see it here, in this drawing made by Mr. Griffiths; and, instead of being square, we had sent to us all sorts of pictures, a large number of them representing a stellate figure. Here is a drawing made by a Fellow of the Royal Society, on board one of Her Majesty's ships (the *Lord Warden*) which were trying to save the poor *Psyche* at Catania. In this we have perfectly regular rays drawn from every region of the sun, some long, some short, but similar rays are almost invariably opposite each other; but in the interior, inside these rays, the corona is just as it was observed by Mr. Griffiths at Syracuse. I now show you a drawing made by an American gentleman at sea, between Catania and Syracuse, with one ridiculously long ray, a ray as long as was seen by Otto Struve in 1860. Other drawings were made, even on board the same ship, so unlike each other, and so bizarre, that I need only refer to them as showing that there at all events must be some personality. We have then to account for the variations between the observations made in Spain and those made in Sicily. I regret that we have not a third order of difficulties to contend with, as doubtless we should have had if observations had been made by Mr. Huggins' party in North Africa.

*e.—The Rays are accompanied by a Mass of Light.*

These changes of the rays from side to side are accompanied by, and are perhaps to a certain extent due to, the bursting forth of brilliant light in their neighbourhood, where the limbs are nearest in contact. This was first observed by Miraldi in the eclipse of 1724, and has frequently been recorded since. Mr. Warrington Smyth, to whom I have before alluded, states that he noticed this in the last eclipse, and the photographs, I think, have recorded it; but as there is some uncertainty on this point, I need only suggest it.

*f.—Long Rays are seen extending from the Cusps before and after Totality*

So far I have referred only to the rays visible during totality, but long rays were seen when a crescent of the sun was visible in 1860 and 1868 by Mr. Galton and Mr. Hennessy. Mr. Brett caught the same phenomenon last year; but as the sky was cloudy the commencements of the rays only were seen, appearing like delicate brushes in prolongation of the cusps. These observations are of great value, *as no one for one moment imagines that these rays are solar*, and yet they are very like those seen during totality.

*g.—Sometimes Dark Rays, called Rifts, are seen instead of Bright ones*

Those rays to which I have referred are, however, not the only kind of rays that are observed. At times are seen, as it were, openings in the corona; the openings being of the same shape as the rays, that is, expanding as they leave the dark moon, and opening more or less exactly as the rays do. Like the rays also they are sometimes very numerous; in other eclipses they are few in number. Let us take the eclipse observed in India in 1868. Several drawings made there showed the corona as square as it was drawn in Spain last year; others as round as it was seen in Sicily; but the eclipse was not observed only in India, it was observed at Mantawalok-Kelee by Captain Bullock, and at Whae-Whan, on the east coast of the Malayan Peninsula, by Sir Harry St. George Ord, Governor of the Straits Settlements. In the former place we had rifts expanding rapidly as they left the sun—one forms an angle of 90°, the sides of another being *parallel*—separating patches of corona, which in some places extends $2\frac{1}{2}$ diameters of the moon from the sun.

At Whae-Whan we are told that at one particular moment of the eclipse "it was noticed that from several points in the moon's circumference darker rays emanated, extending to a considerable distance into space, and appearing like shadows cast forth into space by something not very well defined;" these dark rays afterwards "diminishing."

Now let us pass on to the eclipse of 1869. In two drawings made by Dr. Gould, in which the changes in the bright bundles of rays come out in a most unmistakable way, we get similar rifts, which changed as violently as did the rays; while in another drawing made by Mr. Gilman, the whole corona is furrowed by narrow rifts in all regions lying between violet, mauve-coloured, white, and yellowish white rays!

Now, what have we bearing on this point in the recent observations? No rift was *seen* in Sicily; one rift was recorded by the sketchers in Spain, but more than one rift was photographed in both places. We must remember, however, in thus bringing eye-sketches and photographs into comparison, first that the eye too often in such observations retains a general impression of the whole phenomenon, while the plate records the phenomenon as it existed at the time at which it was exposed; and secondly, that we know that the plates record chemically, while the eye records visually. We are dealing with two different kinds of light.

I will show you two photographs on the screen. Although the lucid intervals were very rare, we were fortunate enough to get one photograph of the coronal regions in Syracuse, and one in Spain. I now show you the photograph made by the American party in Spain. You see here that, probably owing to a cloud, we get a certain amount of light driven on to the dark moon, and you also see the indications of the rifts. This photograph was taken with an instrument with a small field of view, so that the most important parts of the corona were rendered invisible by the instrument itself.

Lord Lindsay, who also photographed in Spain, recorded no rifts.

In the other photograph, taken at Syracuse, the result is better. We have the equivalent of the rift in the photograph I showed you before. The instrument was extremely unsteady, and the definition not so good as it would have been if Mr. Brothers had had a good opportunity of displaying his skill. We get other fainter indications of other rifts here and there, and the question whether these rifts agree in the photograph taken in Spain with those in that taken in Syracuse is one of great importance; and it is to be hoped that before long it will be set at rest. Some observers think they agree; others think they do not.

But there is an important consideration based on that photograph, to which I must draw your particular attention. I have shown you the photograph as it may be thrown on the screen; but in the photograph itself there are delicate details which it is impossible to reproduce. The dark portions in the corona indicated in the copy I have shown you are merely the bases of so many dark wedges driving out into space, like their prototypes in the Indian eclipse. It is Mr. Brothers's opinion, I believe, that all you see on the screen round the dark moon, all that enormous mass of light, nearly uniform in texture, and these beautiful broad rays between the rifts are really and absolutely parts of the solar corona. I confess I do not wish to commit myself to such an opinion. We want more facts, and the *onus probandi* lies with those who insist upon that view, and I have yet to hear an explanation of them on that basis.

*h.—The Corona sometimes seems to be Flickering or Rotating.*

We now come to the next point. Time out of mind, that is, for the last two centuries, the corona has been observed to be flickering, waving, or rotating, moving in every conceivable way and direction. In 1652 it was described as "a pleasant spectacle of rotatory motion." Don Antonio Ulloa remarked of the corona observed in the eclipse of 1788, "It seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning round its centre." The terms whirling and flickering were applied in the eclipse of 1860. This extraordinary condition of things was also thoroughly endorsed by the late observations. It certainly exists, and is among the observations we have to take into account. When I saw an officer of one of the ships at Catania, I asked him if he had taken a drawing of the corona. "No," he said. I asked him, "Did you see any rays?" "Yes." "Then why did you not make any drawing of them?" His answer was, "How on earth could you draw a thing that was going round and round like a firework?" This was not the only observation of the kind, and the tendency of such observations I need hardly say is to strengthen a belief in the unstable, and therefore uncosmical, nature of their rays.

Is this variation of light due to the brilliancy of the corona, and the rapid change of the rays, which is one of the results which comes out clearest? In 1842 the brilliancy of the corona was stated to be insupportable to the naked eye. A similar remark was made to me by several of those officers who saw the last eclipse in Sicily. J. NORMAN LOCKYER

(*To be continued.*)

## SCIENTIFIC INTELLIGENCE FROM AMERICA*

PROF. LEIDY has lately announced to the Philadelphia Academy of Natural Sciences the existence of some new fossil mammals from the Tertiary formations of Wyoming Territory. One was a tower jaw, discovered by Dr. J. Van A. Carter in the vicinity of Fort Bridger. The animal to which it belonged was as large as a hog, but was more nearly allied to the rhinoceros or tapirs. It was especially remarkable for the possession of a large pair of front teeth, resembling, both in form and construction, the incisors of the beaver. The name proposed for it was *Trogurus castoroideus*, or the beaver-toothed gnawing-hog. Another of the fossils indicates a carnivorous animal, a contemporary of the former, and about the size of the gray fox. The animal was related to the weasel and canine families, and was called *Sinopa rapax*, the former name being that applied by the Blackfeet Indians to a small fox. Prof. Leidy also exhibited photographs of the lower jaw of the American mastodon, recently received from Prof. W. C. Kerr, State Geologist of North Carolina. The jaw was found in Lenoir County of that State. It belonged to a mature male, and was of special interest from its retaining both tusks, as well as the molar teeth.—Among objects of great ethnological import are the aboriginal inscriptions or carvings upon rocks, which are met with in North America and elsewhere, and are sometimes of a very remarkable character. Ordinary copies of such inscriptions, unless they be photographs, are rarely of sufficient accuracy to be of much value; and those of our readers who are likely to come across such inscriptions may like to know a method by which an absolutely perfect fac-simile can be made. This process has been applied with much success in copying carvings in Egypt and other places, and it will be equally serviceable in our own country. For this purpose the inscription is to be first well cleaned from dust or mud by means of a hard, stiff brush; stout, unsized paper is then to be wetted rapidly, but uniformly, in a tub of water, and applied to the inscription, and forced into the irregularities by repeated and forcible strokes with a hard brush, an ordinary clothes-brush being as good as any for the purpose. If the stone be clear of dust, the paper adheres, and, when dry, falls off, forming a perfect mould of the inscription. If the carving be deep or broad, it is sometimes advisable to apply several sheets of paper, one after the other, brushing over the surface of one with glue or gum before applying the next, so as to obtain, when dry, a firm body. By making a plaster cast of the paper relief thus prepared a fac-simile of the inscription will be obtained.—The present year seems to be marked with a great deal of activity and enterprise in researches connected with the natural history and physics of the deep seas, especially on the coast of America. We have already referred to the enterprise proposed by the Coast Survey, of sending a steamer, especially adapted to this purpose, around Cape Horn to the California coast, on a ten-months' journey, to be accompanied by Professor Agassiz and Count Pourtales, and a corps of assistants, all prepared to make observations and collections on the most perfect scale. The expense of the scientific work will, it is understood, to the amount of 15,000 dollars, be defrayed by Mr. Thayer (the same gentleman who supplied the funds for Professor Agassiz's expedition to Brazil), a sum which will probably enable Professor Agassiz to accomplish his object in the most perfect manner.—Professor Verrill and party, from Yale College, will also, it is expected, prosecute an exhaustive research into the deep sea and littoral fauna of the Vineyard Sound and the adjacent waters, in connection with the inquiries of the United States Commission of Fish and Fisheries relative to the decrease of the food fishes of our coast. Corresponding researches will also be carried on in the deeper waters of Lake Michigan, where, it may be remembered, the interesting discovery was made last year of crustaceans and fish of marine types at a depth of 300ft. and over. The inquiries this year will be under the immediate direction of Dr. Stimpson and Mr. Milner in a still deeper part of the lake, and it is not at all improbable that discoveries of the highest interest will be made.—The Arctic expedition of Captain Hall will also undoubtedly do its part in the general work, as the naturalist of the party, Dr. Emil Bessels, has had large experience in such labours, and is practically conversant with the fauna of the arctic seas from his connection with the Spitzbergen expedition of 1869.—At the June meeting of the California Academy of Sciences the subject of inviting the American Association for the Advancement of Science to meet in San Francisco in 1872 was discussed, and the treasurer was instructed to call upon the trustees, and to solicit the co-operation of the Chamber of Commerce in taking measures toward this object. The meeting for the present year will be held in August next in Indianapolis, and a large attendance is expected, especially of Western members, to whom the places of meeting in the East have generally proved too remote to suit their convenience.

* Communicated by the Scientific Editor of *Harper's Weekly*.

## SCIENTIFIC SERIALS

THE *American Naturalist* for June contains no article of very striking value, though several of interest in special subjects. Dr. Elliott Coues contributes an account of the yellow-headed blackbird, *Xanthocephalus icterocephalus*, first described by Prince Buonaparte in his continuation of Wilson's Ornithology.—An article on Cuban Seaweeds, by Dr. W. G. Farlow, includes outline drawings of a number of distinct types.—Dr. Lebaron describes a new species of moth, the larva of which is extremely destructive to young apple trees, which he calls *Tortrix malivorana*, or the Lesser Apple Leaf-folder.—Mr. E. L. Greene contributes June Rambles in the Rocky Mountains, with special reference to their flora.—From Dr. Henry Shimer we have "Additional Notes on the Striped Squash Beetle," and from Prof. W. H. Brewer, "Animal Life in the Rocky Mountains of Colorado."—A larger space than usual is occupied by Reviews, among which is one of Mivart's "Genesis of Species," comparing the views of the author with those of the American writers, Cope and Hyatt.

THE first article in the *Journal of Botany* for June is an important one, by Prof. A. H. Church, on Sugar in Beet-root, with a record of investigations on the effect of the amount of rainfall in the development of the sugar.—Dr. Henry Trimen discusses the question, "Is the Sweet Flag, *Acorus calamus*, a Native?" showing that it was unknown in this country before 1596, and that it was not till about 1660 that it was reported as a wild plant from Norfolk. The plant appears to be originally a native of south-east Europe.—Prof. Dickson has an article on the Phyllotaxis of *Lepidodendron*, and the allied, if not identical, genus *Knorria*.—Mr. A. G. More continues his Supplement to the "Flora Vectensis;" and the Rev. Jas. M. Crombie his additions to the British Lichen-Flora.

The number for July contains Mr. Ernst's "Jottings from a Botanical Note-book," and concludes Mr. A. G. More's "Supplement to the Flora Vectensis." Dr. Trimen contributes some notes on plants observed in Jersey and Guernsey in April. There are several other short papers and notes of special interest to British botanists.

OF the *Bibliothèque Universelle et Revue Suisse*, one of the most valuable of continental periodicals, whether we consider the quality of its original articles, or the admirable extracts of scientific memoirs which it contains, we have just received the part published on May 15, which forms the commencement of a new volume. The first and most important of the three papers contained in it is on the action of magnetism on gases traversed by electrical discharges, by MM. A. de la Rive and E. Sarasin, in which the authors describe a long series of experiments made by them, leading to the following conclusions:—1. The action of magnetism exerted only upon a portion of an electric jet traversing a rarefied gas, causes an augmentation of density in this portion. 2. This action exerted upon an electric jet placed *equatorially* between the poles of an electro-magnet, produces in the rarefied gas an *augmentation* of resistance proportional to the conductivity of the gas itself. 3. On the contrary, it causes a corresponding *diminution* of resistance, when the jet is directed *axially* between the two magnetic poles. 4. When the action of the magnetism is to impress a continuous movement of rotation upon the electric jet, it has no influence upon the conductivity if the rotation be in a plane perpendicular to the axis of the iron cylinder detaining the rotation, and diminishes it considerably if the rotation takes place so that the jet describes a cylinder round the axis. 5. These effects do not seem to be due to variations of density, but to perturbations in the arrangement of the particles of the rarefied gas.—A second paper is an excellent abstract and discussion by M. Emile Gautier, of the observations of solar protuberances, made at Rome by Prof. Respighi; and the third consists of an account of geological, meteorological, and archæological explorations made in the province of Constantine (Algeria), by M. Tissot.

THE first part of the twenty-third volume of the *Zeitschrift der deutschen geologischen Gesellschaft*, containing the proceedings of that society for the months of November and December 1870, and January 1871, includes one paper which will be of especial interest to British geologists, namely, "Some Geological Sketches from the East Coast of Scotland," by Prof. F. Zirkel, extending over 124 pages of text, illustrated with four plates. In this paper the complicated geology of the islands of Arran, Mull, Iona, Staffa, and Skye is discussed in considerable detail, and the author winds up with a description of the east and west section of the north of Scotland. Another long paper is the first part of a geological description of the annular mountain of Santorin, by M. K. von Fritsch.—M. C. Struckmann describes the *Pteroceras* beds of the Kimmeridge formation at Ahlem, near Hanover, which he divides into three series (upper, middle, and lower), indicating the characteristic fossils of each deposit. M. R. Richter publishes a fourth notice on the Thuringian slates, for which he claims an Upper Silurian age, an opinion here supported chiefly on the evidence of Graptolites. The author discusses the affinities of the Graptolitidæ, and adopts an opinion expressed by Leuckart (MS.) that this group is to be regarded as nearly allied to the Bryozoa. The author describes a new genus, *Triplograptus*, the chief character of which is that the canal has three vertical rows of alternating cells, of which the type is *T. nereitarum* (Richt.), and also as new species *Diplograptus pennatulus* and *Monograptus crenatus*. These and some other species are figured in the plate accompanying the memoir. A new species of *Nautilus* (*N. veles*) is also described and figured in this paper (p. 243). From M. Emanuel Kayser we find a notice of the occurrence of *Rhynchonella pugnus* with traces of colour in the limestone of the Eifel (Devonian), to which is appended a tabular list of those fossil shells on which traces of colouration have been observed.

## SOCIETIES AND ACADEMIES

### LONDON

Geological Society, June 21.—Joseph Prestwich, F.R.S., in the chair.—R. J. Watson, W. T. Scarth, Gen. A. C. Bentinck, and John Brooke were elected Fellows of the Society.—"On some supposed Vegetable Fossils," by William Carruthers, F.R.S. In this paper the author desired to record certain examples of objects which had been regarded, erroneously, as vegetable fossils. The specimens to which he specially alluded were as follows:—Supposed fruits on which Geinitz founded the genus *Guilielmites*, namely, *Carpolites umbonatus* Sternb., and *Guilielmites permianus* Gein., which the author regarded as the result of the presence of fluid or gaseous matter in the rock when in a plastic state; some roundish bodies, which, when occurring in the Stonesfield slate, have been regarded as fossil fruits, but which the author considered to be the ova of reptiles, and of which he described two new forms; and the flat, horny pen of a Cuttlefish from the Purbeck of Dorsetshire, described by the author as *Teudopsis Brodiei*, sp. n. Mr. Seeley remarked on the compressed spheroids found in so many rocks, that there was a difficulty in accepting the view of their originating in fluid vesicles, though he was unable to suggest any other theory by which to account for them. He observed that the eggs from the Stonesfield slate closely resemble those of birds, and that it was of the highest interest to find such eggs in strata containing so many remains of ornithosaurian forms, such as *Rhamphorhynchus* and *Pterodactylus*, of which genus probably these were the eggs. Prof. Rupert Jones fully recognised the ingenious explanation of the bubble-formed limited slickensides, that looked so much like possible fossil fruits, and Mr. Carruthers's masterly treatment of the other specimens. But he wished that the author would take up the subject exhaustively, and define the nature of other supposed vegetable fossils, such as the so-called fucoids, *Palæochorda*, *Palæophyton*, *Oldhamia*, &c., many, if not all, of which Prof. Jones thought to be due to galleries and other tracks made by Crustaceans. Prof. Ramsay had known many instances of such blunders as those pointed out, made, not by experienced geologists, but by those unacquainted with the science. Though he had never regarded the flattened spheroids as fossils, he was unable to account for their presence in the clay-beds of different ages. Mr. Hulke inquired whether Mr. Carruthers considered the limited slickensides common in the Kimmeridge shales as due to gaseous origin. He remarked on the rarity of Pterodactylian remains as compared with those of other Saurians in the Wealden beds, in which the presumed eggs of Pterodactyls were found. Mr. Seeley did not regard the Wealden egg as being that of a Pterodactyle. Mr. Carruthers, in reply, remarked that the local slickensides mentioned by Mr. Hulke differed in character from those to which he had referred.—"Notes on the Geology of part of the County of Donegal," by A. H. Green, F.G.S. In this paper the author described the geological structure of the country in the neighbourhood of the Errigal Mountain, with the view of demonstrating the occurrence in this district of an inter-stratification with mica-schist of beds of rock, which can hardly be distinguished from granite, the very gradual passage from alternations of granitic gneiss and mica-schist into granite alone, and the marked traces of bedding and other signs of stratification that appear in the granite, to which the author ascribed a metamorphic origin. He also noticed the marks of ice-action observed by him in this region, and referred especially to some remarkable fluted bosses of quartzite, and to the formation of some small lakes by the scooping action of ice. Mr. Forbes stated that none of the facts of this communication were new, but he dissented altogether from the conclusions arrived at by the author in regarding these rocks as originally of sedimentary origin, and for the following reasons: (1) That this district has been studied in detail by Mr. Scott and Prof. Haughton, who declare the rock to be undoubtedly intrusive, as it not only sends out veins into the neighbouring strata, but also encloses fragments of the rocks through which it has broken. (2) Because the author starts from the idea that if such rocks are found to lie conformably on beds of undoubted sedimentary origin, it is a proof of their being themselves sedimentary or stratified,—a conclusion which is totally unwarranted, since there are innumerable instances, not only of beds of lava or other igneous rocks being conformable to fossiliferous strata, but of their also being found intercalated with such beds even for considerable distances. (3) The strata, so far from being proved by him to be of truly sedimentary origin, are of a most questionable origin, since they are neither in themselves fossiliferous, nor can they be correlated with any containing fossils as proofs of true sedimentary deposition; and the description of his section is sufficient to show this; for although it looks well on paper on a scale of three feet to the mile, the author has so little confidence in it that he is not even certain as to which is the top or bottom of the section on which so much generalisation is based. (4)

That a parallel structure equally, if not better developed than any occurring in the gneiss of Donegal, is common to many volcanic rocks, as in a specimen laid before the meeting, in which this parallel foliated structure due to crystallisation-layers is so well developed as to make it appear exactly like a stratified rock, and even split along these lines, and this, although the product of volcanoes still active is found for great distances both overlying conformably and intercalated between beds of the Cretaceous and Oolite formations. Mr. Scott was unwilling to accept the section given by the author as satisfactory. He agreed, however, as to the bedded appearance of the granite, and to the masses lying in general conformably with the lines of stratification of the country. The nearest spot at which fossiliferous rocks occurred was separated from the beds described by the whole width of the county of Tyrone, though some presumed Eozoonal forms had been found at a less distance. He was not prepared to believe in the original absolutely fused condition of granite, nor in there being two distinct forms under which it occurred.—"Memoranda on the most recent Geological Changes of the Rivers and Plains of Northern India, founded on accurate surveys and the Artesian well-boring at Umballa, to show the practical application of Mr. Login's theory of the abrading and transporting power of water to effect such changes," by T. Login, The author commenced by referring to the general conditions of the surface of the country under consideration, and to the evidence afforded by it of a great decrease in the amount of rainfall, and a great change in the nature of the rivers. His object was to show that the superficial deposits of the plains of India were formed by the action of mountain streams, the deposits being irregular transversely, but exhibiting a uniform section longitudinally, in a curve which the author believed to be a true parabola, as indicated by Mr. Tylor. The connection of this with the author's theory as to the transporting power of water was indicated. The author also showed that the beds of the large Indian rivers are rising rather than being lowered, and pointed out that this was in accordance with his theory.

### Halifax, Nova Scotia

Institute of Natural Science, May 8.—Mr. J. Matthew Jones, F.L.S., president, in the chair. Mr. Frederick Allison read a paper entitled "Results of Meteorological Observations at Halifax, Nova Scotia, for 1870." The temperature of January had not been approached since 1863. Mean pressure was great. Cloud was scanty, and winds strong, N.W. prevailing. Very large total precipitation, due to heavy rain, the snow-fall being deficient. No fog, and but five days' sleighing during the whole of January. Strong east gales at the close of the month. February was nearer to normal temperature. Mean pressure very light. Cloud far exceeded that of January and its own average. Prevalent wind, N.W., strong. Great precipitation, nearly doubling the average amount, and especially large in rain. One fog, and sleighing from the 1st to 25th. On the 9th strong east gale in morning, and blowing at night from the west. March bore much the same relation to normal temperature as did February; but the minimum of the year, 6°, occurred on the 12th of that month. Pressure still extremely light. Cloud in decided defect. Prevalent wind N.N.W., with mean force great. Precipitation, both of rain and snow, small. Only one fog. Eleven days of sleighing. Three gales, all more or less eastwardly. Wild geese (*Anser Canadensis*) passed over on their northerly migration on the 19th. Peach, trained against a south wall, blossomed on the 24th. The American robin (*Turdus migratorius*) appeared on the 30th. April was warm. Pressure 29·743, but ·001 below an eight years' average. Cloud still deficient. A peculiar direction of wind was prevalent—E.S.E. Mean force small. Precipitation close to average; rain being abundant, but snow only one inch. Five fogs recorded. First thunder and lightning this year on the 12th. One short gale from E.S.E. Frogs (*Hylodes Pickeringii*) first heard on the 8th; and May flowers (*Epigæa repens*) in full bloom on the 12th. The mean temperature of May was a little less than average. On the 30th 80°·2 was reached. Mean pressure a little light. A very bright month, with only 3·19 inches of rain, the average being 4·33. Snow inappreciable, the latest falling on the 24th, and melting as it fell. Four fogs. Thunder and lightning on the 9th and 12th. The garden cherry blossomed on the 23rd, and the humming-bird (*Trochilus colubis*) was first seen on the 18th. June was slightly cool, somewhat low in pressure and decidedly bright. Only 1·69 inches of rain fell. Mean velocity of wind but 8·8 miles per hour; direction W.S.W. Three fogs noted. No frost after the 24th of the preceding month, either at five feet high or on the surface of the earth. Thunder and lightning twice. The apple blossomed on the 6th, and red clover same date; horse-chesnut on the 2nd; wild strawberries ripe on the 20th. Grass mowing began about Halifax on the 30th. July temperature was 1°·85 above the average. On the 24th 91°·5 was marked. The mean of six equidistant observations on 25th, 75°·27, being the warmest day recorded at this station for at least twelve years. Mean pressure low. Great want of cloud. Light winds; direction S. 59° W.; velocity 8·1 miles per hour. Rain, 3·21 inches, being much above average. Four fogs. Thunder and lightning twice. August was warm also. Mean pressure almost identical with July, being 29·659. Very little cloud. Wind, resultant direction, N. 77° W. Mean velocity 10·5 miles per hour. Rain scanty, giving but 2·20 inches. Fogs three. Thunder and lightning thrice. September mean temperature 57°·20, having fallen 7°·60 below August. On the 30th exactly 32° was registered by grass minimum; but atmosphere never descended to freezing point. Mean pressure still low, and cloud also deficient. Wind, resultant direction N. 15° W., and mean velocity only 10·6 per hour. Rain, half an inch less than average. Three fogs. Hoar frost on the 30th. Thunder once. Lightning twice. Three gales. October had a mean temperature of 48°·14. Mean pressure 29·825. Still a quantity of cloud, though October is frequently a bright month in Nova Scotia. Resultant direction of wind N. 42° W., and mean velocity 12·45 miles per hour. Heavy rainfall, and eight inches of snow. One fog. Three gales from N.W., S.S.W., and S. First frost, five feet above ground on the 26th; temperature having been above 32° 155 days. Measurable snow on 31st. The mean temperature of November remained above the average. The whole pressure again small. The month was rather less cloudy than usual. Resultant direction of wind N. 87° W., and mean velocity only 10·75 miles per hour. Rainfall large, 5·67 inches, and snow depth great, 7·7 inches. Three fogs. Three gales, N.W., S S.E., and S.E. Meteors on the night of the 14th. December was very mild. Mean temperature 30°. Pressure very low. Much cloud. Resultant direction of wind N. 76° W., and mean velocity 11·6 miles per hour. Rain was heavy, and snow small, though containing larger amount of water than average. One fog. Four days' sleighing. On Christmas Eve thermometer reached 4°·6, minimum of month. After noting the cyclone of the 3rd and 4th of September, Mr. Allison proceeded to connect it with the gale of the 7th moving in the Bay of Biscay, in which the *Captain* foundered. Giving the following figures from a mass of observations, to show the storm path:—S.S. *Robert Lowe* at sea, lat. 43°2′ N., long. 65°3′ W., September 4, 4 A.M., bar. 28·700; wind 25lb. per square foot. Halifax N.S., lat. 44°39′ N., long. 63°36′ W., September 4, 9.30 A.M., bar. 28·952, 6 to 7 A.M.; wind velocity 65·7 per hour, and reaching 70 miles in gusts, fully 24·5lb. per square foot. Glace Bay N.S., about 250 miles from *Robert Lowe*, E.N.E., bar. 3 P.M. 29·333; wind 3 P.M. 86 miles per hour. This storm was travelling at direct rate of about 23 miles per hour in this longitude, its speed being accelerated as it progressed eastward. It would be due, with its south-eastern edge, in diminished force probably, in Bay of Biscay on evening of 6th of September. From these and other data a world-wide system of telegraphic storm warnings was urged.—Another interesting paper, "On the Meteorology of Glace Bay, Cape Breton, N.S.," by Mr. Henry Poole, was also read.

### Paris

Académie des Sciences Morales et Politiques, June 24.—M. Jules Simon in the chair. Notice was given of the death of M. Ramon de la Sagra, a Spanish gentleman who had been a great traveller in America, and was well known as a botanist.—M. Egger read some pages of his great work "On the Progressive Development of Infants."

July 8.—M. Paul Janot in the chair. Notice was given of two letters received from M. Henry Martin, a member of the National Assembly, and M. Filon, an inspector of the Academy of Paris, both contending for the seat left vacant by the late M. Pierre Clement, who wrote, many years ago, a history of the *Revocation de l'Edit de Nantes*, vindicating Protestantism, and published many articles in the *Journal des Economistes*, in support of free trade policy. The contest will be severe, as M. Henry Martin is very popular, being the author of a History of France. M. Filon is a gentleman of a wider intellect, and has written a Comparative History of France and England. The election will take place on the 22nd.

**Académie des Sciences**, July 10.—M. Claude Bernard in the chair. Notification was received of the death of M. Haidinger, the keeper of the great aerolitic collection at Vienna and a correspondent in the section of mineralogy. —The public sitting, which, according to the rules, was held before the secret one, was rather long and interesting. M. Puiseux was unanimously elected a member of the section of geometry (this honour is very seldom paid to any member). M. Puiseux belongs to the scientific staff of the National Observatory. He was much praised many years ago by Cauchy for his calculations on variations of weight and of its effects. He was a contributor to Lionville's *Journal de Mathematiques*.—M. Boussingault described some experiments showing that water is not liable to freeze irrespective of the degree of cold to which it is submitted, as long as it is not allowed to expand in order to change into ice. It is the complement of the celebrated Florentine experiment. M. Boussingault exposed water to $-13°$ Cent. enclosed in strong steel tubes as used for rifled guns, without any congelation taking place. On unscrewing the steel end of the barrel, the congelation was instantaneous. The fluidity of the water was made manifest by small steel spheres, which moved freely inside the guns during the whole process, and would have been stopped by congelation. A very long conversation took place between M. Boussingault and several members who proposed many objections, to which he found ready answers.—M. Saint-Venant read a long report on a memoir presented by M. Maurice Levy on several Equations showing the internal movements of molecules when a ductile body is submitted to external pressure.—M. Faurneyron was a French engineer of great reputation, known by the invention of "turbines" or hydraulic wheels. He bequeathed to the Academy a certain sum in the funds to give a 40*l*. prize to the best memoir on Practical Mechanics every two years. The Academy appointed a committee of five of its members to draw up a programme for the next competition. The competition is to be open to all, irrespective of nationality and qualification, except to the members of the different French academies.—M. Brown, the astronomer at the celebrated Trevandum Observatory, read a most important note on the "Diurnal Lunar Variation," which he proved has sometimes to exceed the solar variation. The law is illustrated by calculating the maximum Every day there are two maxima of lunar action. In June, when the moon is on the 6th and 18th horal meridian, in December on the 0th and 12th, and in the intermediate months on the intermediate meridians, according to progressive changes. The excursions are greater when the moon is nearer to us (perigee), and when the passage of the moon to the maximum meridian is by daylight. This difference is very great, the *nocturnal* max. reaching only $\frac{1}{3}$ of *diurnal* max. The law is worth the most serious consideration, as connections between variations of magnetism and temperature are becoming every day more and more frequent. It may lead to the discovery of the lunar influence on meteorology, which discovery will be *initium sapientiæ*.—M. W. de Fonvielle sent a note discussing certain singular phenomena which were observed in Scotland during the stormy periods of June 18 and July 18. The facts were quoted from the *Scotsman*, an Edinburgh paper. The note was printed in the *Comptes Rendus*. The author is anxious to see if "mirages," as observed on the Isle of Man, can be considered as having been a presage of the stormy weather. He wrote also upon certain accidents, showing that it is dangerous to move metallic objects during thunderstorms. M. Chapelas presented the results of observations made during twenty years (1848-1868) on 39,771 meteors, out of these 23,481 were observed in summer when the nights are short, only 2,145 in winter when the nights are long. The mean direction is S.S.E. The numbers of meteors vary in *inverse ratio* with their magnitude:—1st magnitude 2,497, 2nd magnitude 3,918, 3rd magnitude 7,137, 4th magnitude 8,847, 5th magnitude (an exception to the rule) 8,050, 6th magnitude 9,322 (very slight augmentation). He says, moreover, it shows that falling stars are more frequent in high altitudes. It is true, assuming falling stars to be essentially of the same magnitude, and differing only apparently from distance.

RIGA

**Society of Naturalists**, February 1.—Prof. Schell discoursed upon the importance of water-levels on the coasts of the Baltic provinces, and described some anemometers. — M. Schroeder communicated a notice relating to the avifauna of the Baltic provinces, in which he mentioned several species to be struck out of or added to the previously published lists. He made the total number of species, 272.—Baron F. Hoyningen-Huene communicated a continuation of his phænological observations, during the year 1870, containing a report on natural phenomena observed from March to October.

PHILADELPHIA

**American Philosophical Society**, April 1.—Prof. Cope made remarks on the Vertebrata obtained in the Port Kennedy bone cave by Chas. M. Wheatley, stating the number of species to be forty-two. The Mammalia were referred to orders, as follows:—Edentata, 6 species; Rodentia, 14; Insectivora, 1; Chiroptera, 1; Ungulata, 8; Carnivora, 4; total, 34, of which about half are new to science. Birds and Reptiles, 8 species. He made remarks on the nature and origin of the post-pliocene fauna, the origin of the caves, and possible topographical history of the country in that connection.—Pliny E. Chase read a paper on "Resemblances between Atmospheric, Magnetic, and Ocean Currents."—Lieut. Dutton presented some views on regional subsidence and elevation, and mentioned the physical changes produced by the metamorphism of rocks as an agent in changing the contour of the earth's surface. The obliteration in specific gravity produced by change of chemical constitution of interior rock strata was an important cause of the elevations and subsidences of the earth's crust, generally overlooked.

## BOOKS RECEIVED

ENGLISH.—Our Sister Republic; a Gala Trip through Mexico in 1869 70 (Trübner and Co.).

FOREIGN.—(Through Williams and Norgate)—Medizinische Jahrbüchen: S. Stricker, &c., vols. 1 and 2.—Naturwissenschaftliche Vorträge: J. R. Mayer.

## PAMPHLETS RECEIVED

ENGLISH.—National Health: H. W. Acland, M D.—How to Live on 6*d*. a Day: Dr. Nichols.—A Sanitary Inquiry: R. Weaver.—Art and Religion: J. Gilbert.—The Universal Change in Natural Elements: R. Mansill.—Fauna Perthensis, part 1: Lepidoptera: F. Buchanan White, M.D.—Proceedings of the Liverpool Field Club for 1870-71.—Transactions of the Chemical Society of Newcastle-on-Tyne, vol. 1, for 1868-71.—Mechanical Building: G Ryland.—Proceedings of the Geologists' Association for 1870.—A Key to the Natural Orders of British Flowering Plants: T. Baxter.—Natural History Transactions of Northumberland and Durham, vol. iv., part 1.—The Manufacture of Russian Sheet-Iron: J. Percy, M D.—The Quarterly Weather Report of the Meteorological Office.—Transactions of the Norfolk and Norwich Naturalists' Society, 1870-71.—Papers on the Cause of Rain, &c: G. A. Rowell.

AMERICAN.—Report of the Committee on Building Stores to the Board of Capitol Commissioners of the State of Iowa: Prof. Hinrichs.—The School Laboratory of Physical Science: Prof. G. Hinrichs.—The Principles of Pure Crystallography: Prof. G. Hinrichs.—Third Annual Report on the Noxious, Beneficial, and other Insects of the State of Missouri: C. V. Riley.—Bulletin of the Museum of Comparative Zoology at Harvard College, vol. iii, No. 1.—Preliminary Report on the Vertebrata discovered in the Port Kennedy Bone-Cave: Prof. E. D. Cope.

FOREIGN.—L'Académie des Sciences pendant le siège de Paris: G. G. de Caux, Paris.—Bulletin de l'Académie Imp. des Sciences de St Petersburg, vol. xv., No. 17-31, vol. xvi., No. 1-4.—Ricerche sulla propagazione dell' electricità nei liquidi: Dr. D. Macaluso, Palermo.

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ERRATUM.—Vol. IV. p. 203, first column, line 27, for "503° C." read "50 C."

THURSDAY, JULY 27, 1871

## *MR. CROOKES ON THE "PSYCHIC" FORCE*

WITH a boldness and honesty which deserve the greatest respect, Mr. Crookes has come forward as an investigator of those mysterious phenomena which have now been so long before the public that it is unnecessary to name them, more especially as their generally received name is very objectionable.

Two things have contributed to retard our knowledge of these strange events. In the first place, until lately few men of name have been associated with their occurrence, so that outsiders have not had the facts put before them in a proper manner. In the next place, we are inclined to endorse the remark of Mr. Crookes, that men of science have shown too great a disinclination to investigate the existence and nature of these alleged facts, even when their occurrence had been asserted by competent and credible witnesses.

Before adverting to the results obtained by Mr. Crookes, a few words may be said about our mode of procedure in accepting testimony.

Let us suppose that a man comes before us as a witness of some strange and unprecedented occurrence. Here it is evident that we are not entitled to reject his testimony on the ground that we cannot explain what he has seen in accordance with our preconceived views of the universe, even although these views are the result of a long experience; for by this means we should never arrive at anything new. Our first question is manifestly one regarding the man's moral character. Is he an honest and trustworthy man, or is he trying to deceive us?

Let us assume that we have convinced ourselves of his honesty; we are then bound to believe that *he thought he saw* what he described to us; not necessarily, however, that the occurrence which he described actually took place. Convinced, already, that he is not deceiving us, we next question whether he may not be deceived himself. Let us, however, assume that, upon investigation, the circumstances are such that collusion of any kind is out of the question, and that the man is neither trying to deceive us, nor that it is possible that he himself can have been deceived by others. Even yet we have an alternative in our judgment of the event. The phenomenon may be *subjective* rather than *objective*, the result of an action upon the man's brain rather than an outstanding reality. For nothing is more certain than the occasional occurrence of such strange impressions; and the cat or the dog or the skeleton by which the patient is haunted is frequently recognised even by himself as having no external existence. Of late years we have been able to produce instances of this depraved consciousness almost at will. The author of these remarks considers it certain that the electro-biologist has frequently caused them. The unimpeachable character of the patient, combined with the fact that he has sometimes pronounced water to be wine, or a snow storm to be taking place in a room, can only be accounted for on the supposition that he has been put into a peculiar state, during which his evidence of events is utterly worthless. But beyond the bare fact, we know next to nothing of the laws that regulate this action, nor can we tell under what conditions one man is capable of influencing another, or whether a man or body of men may not be capable of influencing themselves.

To come now to the class of events which Mr. Crookes has witnessed. It is greatly to his credit that he has come forward so frankly and honestly; and since he has begun to investigate the peculiar class of facts, we are sure that he will consider it his duty to continue the investigation in such a way as to convince those men of science who may not themselves be able to take up the question—outsiders in fact. Mr. Crookes will, we are sure, not object to a few critical remarks honestly made with the sole view of finding out the truth, and we would therefore express a wish that, in order to facilitate operations the experiments should in future be conducted by only by such men as Mr. Crookes himself, and that it should always be absolutely superfluous to investigate whether machinery, apparatus, or contrivance of any sort, be secreted about the persons present. We should thus start from a higher platform, and the investigation would gain in simplicity, although perhaps something might be lost in the marked nature of the results obtained.

Allowing, however (as we are disposed to allow), that things of an extraordinary nature are frequently witnessed on such occasions, yet we are by no means sure that these constitute external realities. The very fact that the results are uncertain, and that, as far as we know, they have never yet been obtained in broad daylight before a large unbiassed audience, would lead us to suspect that they may be subjective rather than objective, occurring in the imaginations of those present rather than in the outward physical world. Nor can this doubt be removed by any precision of apparatus; for what avails the most perfect instrument as long as we suspect the operator to be under a mental influence of the nature, it may be, of that which is witnessed in electro-biological experiments? The problem is, in fact, one of extreme difficulty, and we do not see how it admits of proof, provided the influence cannot be exerted in broad daylight and before a large audience. There is, however, a cognate phenomenon which admits of easy proof. We allude to clairvoyance, and have in our mind at the present moment a man of science who if not himself a clairvoyant has yet the power to command the services of one who is. Now, were he at once to communicate to a journal such as NATURE, in cipher if necessary, the knowledge derived through the influence, giving the proof afterwards when obtained in an ordinary manner, the public would soon be in a position to judge whether there is any truth in the influence or not.

It is, in fact, somewhat hard upon the writer of these remarks and some others who are disposed to allow the possibility of something of this nature, but have not the opportunity of investigating it, that those who have will not satisfy the public with a convincing proof.

B. STEWART

## *TYNDALL'S FRAGMENTS OF SCIENCE*

*Fragments of Science for Unscientific People.* By Prof. Tyndall, LL.D., F.R.S. (London: Longmans: 1871.)

THIS volume is a reprint of a number of detached essays, lectures, and reviews, by Prof. Tyndall, published at various times and in various places during the last ten years. Besides a few shorter pieces collected at the end, there are in all thirteen articles. These consist of two classes of a totally distinct nature. The larger number constitute considerably the greater bulk of the volume, deal entirely with scientific subjects, and are of a special scientific nature. The remainder deal either directly or indirectly with the question of the opposition or concordance of science and religion. To this question, Prof. Tyndall brings that same remarkable clearness and definiteness of statement which characterises his writings on purely scientific subjects. It is a highly desirable thing for all parties that it should be distinctly stated what are the issues, in their ultimate form, to which our various hypotheses may lead. Prof. Tyndall, from the scientific side, makes this statement clearly and distinctly. He views an hypothesis, so to speak, in its widest generalisation, and does not shrink from it or its consequences. If, he would say, you hold these or those views, then this is what they *must* imply, and what, if these views be true, you *must* come to; and so you need not be afraid, and if you hide it from yourself you only cloak the truth in the one case, or hinder the exposure of error in the other. As an example, let us take the statement of the Natural Evolution hypothesis in the lecture on "The Scientific Use of the Imagination" (page 163 of the present volume). Speaking of the evolution of the present world from a nebulous mass he says:—

"For what are the core and essence of this hypothesis? Strip it naked, and you stand face to face with the notion that not alone the more ignoble forms of animalcular or animal life, not alone the nobler forms of the horse and lion, not alone the exquisite and wonderful mechanism of the human body, but the human mind itself—emotion, intellect, will, and all their phenomena—were once latent in a fiery cloud. But the hypothesis will probably go even further than this. Many who held it would probably assent to the position that at the present moment all our philosophy, all our poetry, all our science, all our art—Plato, Shakspeare, Newton, Raphael—are potential in the fires of the sun. We long to learn something of our origin. If the Evolution hypothesis be correct, even this unsatisfied yearning must have come to us across the ages which separate the unconscious primeval mist from the consciousness of to-day. I do not think that any holder of the Evolution hypothesis would say that I overstate it or overstrain it in any way. I merely strip it of all vagueness, and bring before you, unclothed and unvarnished, the notions by which it must stand or fall."

"Fear not the Evolution hypothesis," he says further on, "steady yourselves in its presence in the ultimate triumph of that truth which was expressed by old Gamaliel when he said, 'If it be of God ye cannot overthrow it; if it be of man it will come to nought.'" This is the true scientific spirit; and the beautiful daring with which Prof. Tyndall launches upon an unknown sea trusting to this guiding principle, is an instance of that noble faith which has lived through all phases of the human mind alike in scientific and unscientific ages. To have a faith in something seems to be the ultimate necessity of all humanity. Let all of us beware how we call that faith, as it exists variously in each of us, false.

Prof. Tyndall always writes in a beautiful, clear, and pointed style. Not the least excellent part of it, and that which probably as much as anything else constitutes him the great scientific teacher he is, is his unbounded power of apt illustration. He carries this into every subject with which he deals. As an example, take the following from page 58 of the article on "Miracles and Special Providences:"—

"The mind is, as it were, a photographic plate, which is gradually cleansed by the effort to think rightly, and which when so cleansed, and not before, receives impressions from the light of truth."

Again, at page 101 we find the following:

"We live upon a ball of matter 8,000 miles in diameter, swathed by an atmosphere of unknown height. This ball has been molten by heat, chilled to a solid, and sculptured by water."

There is the touch of a master's hand in the way in which these few words "fling us the picture of the fight," and enable us vividly to realise that which they would have us realise.

Prof. Tyndall, however, has evidently given less attention to spiritual than to natural questions. Indeed, it is not to be wondered at that a man now-a-days should not have time to pay attention to everything. It is sometimes, however, to be lamented, though perhaps hardly to be wondered at, that a man should write about too much. The articles of a purely scientific character consist of two on "Radiative and Radiant Heat," one on "The Light of the Sky," and one on "Dust and Disease."

The articles on the "Life and Letters of Faraday" will well repay the perusal of those who have not already read them in the *Academy*, and will even well merit a re-perusal, as everything does which gives us any insight into the character of that great and child-like man.

The last of the series is a lecture on Magnetism, addressed to the teachers of primary schools, at the South Kensington Museum. Prof. Tyndall tells us, in a short introduction to it, that he had at first some doubts as to the propriety of its insertion. "But, on reading it," he says, "it seemed so likely to be helpful that my scruples disappeared." We are exceedingly glad that it has been so. The lecture is a beautiful example of true teaching, and of that excellent union of logic and experiment which is the true education which physical science is so well calculated to supply.

JAMES STUART

## *DALL'S BRACHIOPODA OF THE UNITED STATES COAST SURVEY*

*Report on the Brachiopoda obtained by the United States Coast Survey Expedition, in charge of L. F. de Pourtales, with a Revision of the Craniidæ and Discinidæ.* By W. H. Dall. (Bulletin of the Museum of Comparative Zoology, at Harvard College, Cambridge, Mass.) With two plates. (Cambridge, U.S., 1871, 8vo.)

THIS is another important instalment of the published results of the deep-water dredgings made by our Transatlantic cousins and friends in the Gulf of Mexico. The first was issued in 1869, and consisted of a Preliminary Report on the Echini and Starfishes, by Prof. Alexander Agassiz. A report by Dr. Stimpson on the Crustacea procured in the same expedition is announced as nearly ready; and that distinguished zoologist has also undertaken the still greater charge of a report on the Mollusca. It is impossible to over-rate the impulse which will be every where given by such explorations to the study of marine Natural History.

We are now entering on quite a new phase of research, and commencing a survey of the hitherto unknown world beneath the waters. Regarded not merely in a biological, geological, or physical aspect, but also as a basis of sound education, these investigations ought not to be neglected by any civilised nation, especially by Great Britain, which, it is hoped, will never cede her well-earned maritime prestige, and her laudable ambition of discovery. This has been forcibly urged as a duty on the Government in an admirable article which appeared in the *Spectator* of the 22nd of July. In the pages of NATURE (meaning, of course, the present periodical, and not the mythical book to which fanciful writers are wont to allude), some of the results obtained in our deep-sea explorations of the North Atlantic and Mediterranean have been already noticed; and next year will in all probability inaugurate an expedition on a more extensive scale, and worthy of this rich and intellectual country. Sweden has performed her part most nobly, by sending out, in 1869, the *Josephine* frigate for the exploration of the sea-bed lying between the coast of Portugal and the Azores, and this year a corvette and tender to Baffin's Bay and Davis's Straits. Russia despatched, last year, a frigate to New Guinea for a similar purpose, under the scientific charge of an experienced naturalist, Mr. N. M. v. Maclay. We are now informed on good authority that Drs. Noll and Grenacher, two German naturalists, are projecting a dredging expedition along the coasts of Portugal and Morocco to the Canaries. Even France, in the midst of her troubles, devoted some of her energy and vast resources to the peaceful object of dredging in the lower part of the Bay of Biscay, under the personal superintendence of the Marquis de Folin, the Commandant at Bayonne. In Canada a Government schooner has been lately placed at the disposal of the Natural History Society of Montreal for dredging the deeper part of the Gulf of St. Lawrence. But the United States, not content with the laurels she had gained in the Gulf of Mexico, has this year promoted two separate expeditions; one, under the charge of the veteran and celebrated Professor Louis Agassiz, and Count Pourtales, to proceed along the south-eastern coasts of the Atlantic from Bermuda, through the Straits of Magellan to the Galapagos and San Francisco, dredging all the way; and the other, under the charge of Mr. Dall, the author of the Report above cited, has already gone from California to the Aleutian Islands.

The Brachiopoda, which form the subject of the present Report, are usually considered an abnormal class of the Mollusca; although some systematists place them in another group or sub-kingdom, the Molluscoidea, along with the Tunicata and Polyzoa. Mr. Morse, an American naturalist, has recently endeavoured to show that the Brachiopoda are Annelids. This is a very debateable matter of classification. I am, for one, disposed to let the Brachiopoda remain among the Mollusca, to which they appear to be allied through the *Anomia* family. Their mode of reproduction, bivalve shells, and general habits, evince a much closer affinity to the Conchifera than to the Tunicata, Polyzoa, or Annelida. Other points of resemblance between the Brachiopoda and the three last-named groups may savour of analogy, not of homology. The author has executed in a most scientific and conscientious spirit the somewhat difficult task allotted to him; and he has contributed much valuable information to our scanty knowledge of the life-history of these remarkable animals. I regret that I cannot accept his conclusions as to the difference of certain so-called species (*Terebratula vitrea* and *cubensis*, *T. septata* and *floridana*), nor as to the generic value of *Terebratulina* and *Waldheimia*. But this is not the place for discussing such questions. That part of the Report which treats of the *Craniidæ* and *Discinidæ* is equally well done, and the plates are capital.

J. GWYN JEFFREYS

## OUR BOOK SHELF

*The Year-Book of Facts in Science and Art.* By John Timbs. Pp. 288. (London: Lockwood and Co. 1871.)

*Annual of Scientific Discovery, or Year-Book of Facts in Science and Art for* 1871. Edited by John Trowbridge, S.B., aided by W. R. Nichols and C. R. Cross. Pp. 349. (Boston: Gould and Lincoln. London: Trübner and Co. 1871.)

THE opinion that we expressed on a former occasion regarding the relative value of these Year-Books, remains unaltered. Mr. Timbs, as of old, still wields the scissors and the paste-brush with unabated zeal, and his Year-Book for 1870 presents all the faults of its predecessors. Considering that "Science and Art" are not the only subjects to which Mr. Timbs devotes his attention, but that a new book on (we may almost say *of*) "Popular Errors," or on "Curiosities," seems to be always springing from his fertile pen, his "Year-Book" does him no discredit, although non-critical readers may wonder at some of the "Facts," as well as at some of the omissions, which they encounter. Why he should place "The Entozoa Egg," (on which we suspect his ideas are somewhat obscure,) "Protoplasm," the "Germ-Theory of Disease," and "Sleep," under the head of "Natural Philosophy;" or "Snuff Taking: a Preventive for Bronchitis or [and?] Consumption," under that of "Chemical Science," we cannot pretend to say; but, possibly, the following paragraph, taken from the heading "Astronomy and Meteorology" may afford a clue to his mysterious system of classification:—"Dr. F. G. Bergmann has projected from his own consciousness the beings from which the human race developed itself. Their name is 'Anthropiskes,' and they lived in Central Africa. They developed out of apes," p. 265. The appalling idea cannot be repressed that the intellect o our venerable instructor in "Science and Art" must be fai ing from over-work, so as to lead him to confound Anthropology with Astronomy!

The American Annual has the great advantage over its British rival of being compiled by men who understand the subjects on which they are engaged. The editor, John Trowbridge, S.B., is Assistant Professor of Physics in Harvard College, and one of his assistants, W. R. Nichols, is Assistant Professor of Chemistry in the Massachusetts Institute of Technology. The subjects embraced in this volume are nearly the same as those included in Mr. Timbs's Year-Book: Mechanical and Useful Arts, Natural Philosophy, Chemistry, Natural History or Biology, Geology, and Astronomy and Meteorology, being common to both, while the present work has additionally Geography and Antiquities, and the English annual makes Electricity a separate subject.

Unless the editor enters more fully in future volumes into the subject of "Geography and Antiquities," we should recommend the suppression of this department. On the present occasion it is simply compiled from the proceedings of the Geographical Section of the British Association, and does not contain a reference to *Petermann's Journal*, to the French or German Geographical Societies, nor even to the American Geographical Society.

With this exception the "Annual of Scientific Discovery" is entitled to our earnest commendation. The editor and his assistants have done their work well, and the only editorial slip that we have noticed is the insertion of the same paragraph in two separate departments (see pp. 122 and 208). The "Notes of the Editor" at the commencement of the volume are, as in preceding years, especially deserving of praise, and indicate in a comparatively short space the progress of science for the year.

*Mycological Illustrations, being Figures and Descriptions of New and Rare Hymenomycetous Fungi.* Edited by W. Wilson Saunders, F.R.S., F.L.S., and Worthington G. Smith, F.L.S., assisted by A. W. Bennett, M.A., B.Sc., F.L.S. London large 8vo., tab. lith. pict. 24. (London: John Van Voorst, 1871.)

THOSE who have made the longest and most intimate study of Fungi are most sensibly alive to the fact that it is almost impossible to name species, especially those belonging to the genus Agaricus, without figures derived from the authors themselves to whom they are attributable, or at least made under their immediate inspection. It was therefore a great boon to mycologists when Prof. Fries, a student of some sixty years' standing, determined to deposit in the museum at Stockholm figures of a large portion of those species, described by himself, which have a softer texture, and are with difficulty preserved for the herbarium; copies of many of which, and frequently the original sketches, have from time to time been kindly transmitted to this country, while the illustrations themselves are in the course of publication. Five fasciculi have already appeared under the title "Icones selectæ Hymenomycetorum nondum delineatorum," containing fifty plates, several of which comprise two or more distinct kinds; and it is much to be hoped that increasing years will not prevent the venerable mycologist from continuing his indispensable work, supplementing, as it does so nobly, the "sveriges ätliga och giftiga svampar," which furnishes a hundred plates, of which several are critical species, though, from the nature of the publication, the greater number are well-known forms.

We have now before us a work of much importance in the same direction, which, though not sanctioned by so long a study or such numerous treatises, must ever be of considerable weight from the unusual artistic talent of Mr. Worthington Smith, to whom, in conjunction with Mr. Wilson Saunders, the illustrations are due. He has not, however, rested entirely on his own knowledge of the subject as regards the determination of species, but has very wisely obtained help where it was possible to do so. In general the species are very correctly determined, but we venture to make one or two observations where some doubt exists, a matter of no surprise in so very complicated a subject.

Fries has just published a figure of his *A. polius* which is very different from that in the work before us, and which agrees with what we have ourselves always considered that species. *Boletus pachypus* is certainly not the plant of Fries as figured in his work on esculent and poisonous fungi. We have no right to criticise *A. junonius*, as it has the sanction of Fries himself, but we cannot help remarking that it does not at all resemble the figure in the "Svensk Botanik." As regards Cortinarii it is most desirable that the young state should always be figured. *Cortinarius caninus*, for example, is much brighter in colour at first. The figure clearly represents an older condition. The least satisfactory figure is that of *A. hydrophilus*, which differs from the usual form in not having a fistulose stem. There are some errors, whether clerical or otherwise, which call for a stricter revision in future numbers of the Latin phrases.

Thirty species are illustrated in the twenty-four plates, the figures for the most part leaving nothing to be desired. Far the greater part of them have either not been figured before, or the published figures are not satisfactory. We may mention as peculiarly good *Cantharellus radicosus*, *Agaricus atro-cæruleus*, which reminds us of Gould's drawings of infant coots and waterhens; *A. lignatilis*, and *Gomphidius glutinosus*. We trust that this very useful and acceptable work will command such a sale as to ensure its continuance. The materials in the hands of the editors are almost inexhaustible, and are daily increasing.

Since the above was written, a letter has been received from Prof. Fries containing some kindly worded criticisms, the most important of which are subjoined. The least observation from a person of such wide experience must be welcome to every genuine mycologist, and to none, we are assured, more so than to the authors of the work before us. *Cortinarius callisteus* = *A. ferrugineus* Scop., agreeing exactly in habit with the plant of Fries but differing in colour. *A. polius* = *A. fumosus*. *Boletus pachypus* = *B. amarus* Fr. *C. cærulescens* = *C. cumatilis* Fr., *species valde variabilis*. He adds, "the price is so moderate that it excites my admiration. Your admirable work has been received with singular pleasure. It contains three interesting species quite new to me: *Cantharellus radicosus*, *Agaricus adnatus*, and *Agaricus polystictus*."

M. J. BERKELEY

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Mr. Howorth on Darwinism

WILL you allow me to reply to the various letters which appeared in your last number in answer to one from me? I gratefully welcome their general courteousness. Postponing the consideration of Mr. Wallace's letter, I come to Dr. Lionel Beale, the relevancy of whose arguments, and especially of the lugubrious moral attached to them, I fail to understand. It seems to me to be so incoherent and rhetorical that it is far beyond the reach of reply.

Mr. Tylor refers to the last census as disproving my position. He says the population has increased enormously, and yet our age is characterised by its luxury. These statements are correct. But the argument deduced from them has a missing link. The luxury of the upper strata of society has increased with its wealth, but the numbers of the pauper class have been increased in the same rate. In considering the published returns of the Poor Law Board, I am compelled to admit that the increased luxury has been limited to the surface of society, and that its lowest ranks have been correspondingly recruited, and to admit the force of Mr. Doubleday's argument, that the population of England under the Tudors was stationary because of the generally diffused wealth, while that of Ireland in the last century was increasing at an enormous rate, because it was steeped in poverty and want. I am not arguing about individual cases, but about general laws. Now, in Lancashire, where the increase has been so marked, I have it on the authority of owners of mills that the indigenous stock of the county, which is thrifty and well off, is not an increasing element, but is being replaced by the children of the Irish, or semi-Irish blood, from the poorer quarters of the large towns, among whom prudential restraint (which is surely a very visionary *causa causans* in any event) cannot be said to have much influence. At Rome, Venice, Basle, and in France, where the aristocratic class was not limited by primogeniture, it was always dying out, and was only recruited by fresh creations (see the details in Doubleday, chapter iv. *passim*). In all these cases we can appeal to figures, and not to a superficial survey of a Peerage, or the limited area of our own acquaintance.

The particular passage quoted by Mr. Tyler from Malthus has been conclusively answered by Doubleday (chapter vi.), and it is useless to repeat his arguments, which on this point I consider to be unanswerable.

Mr. Lownes repeats the odd charge of Mr. Tait against me, that I put the cart before the horse. The latter gentleman, whom

I have not yet answered, cited against me the elementary case of capons and other creatures of that ilk. They are entirely beside the question. It is as reasonable to quote them in this discussion as to conclude that all chaste people must be cowardly and effeminate because mutilated animals are so. He also said that I mistook the whole rationale of the question, and that it is infertile creatures that grow fat, and not fatness that causes sterility. The only test of the question is the one I have not shrunk from applying in this argument (which, by the way, has not to do so much with the fat as the hearty and strong). This test is that in a great number of cases we can make strong and vigorous but sterile plants and animals fertile by starving or bleeding them, which proves that it is not the organs that are defective, but that the creatures are too hearty.

The experience of Mr. Lownes on the fecundity of consumptive patients, and of the poorest classes as compared with the richest, is at issue with that of the doctors and midwives whom I have access to, and of all the authorities I know whose opinions are based upon statistics.

I am not sure that I understand the second and third paragraphs of his letter. Whichever way the problem is put, I am satisfied if it be admitted that in the more crowded and squalid portions of our towns, the population as a rule is more fertile than in the less crowded neighbourhoods. The case he cites of poor women losing their children early and ceasing to give milk, and, in consequence, soon becoming pregnant again, is counterbalanced by the fact that among the richest the proportion of those who suckle their children is small, and this not because of fastidiousness, but because they secrete little milk. Mr. Lownes once more drags out the Indian and the backwoodsman, but he has overlooked the answer I gave to Mr. Wallace in my former letter, which needs no alteration to meet the case as he has put it. It is the case of the meat-eaters against the vegetable-feeders, the strong and hearty and active against the comparatively stolid and low-conditioned, and as in such cases all the world over the former are not so fertile as the latter. Mr. Lownes objects to savages being cited, because of qualifying circumstances; he may as well say that it is not fair to test natural selection by wild animals, but only by domesticated ones. His treatment of the case of the Patagonian women is convenient but flippant. Mr. Lownes' experience in breeding both cattle and sheep and fowls and in rearing plants must be extremely limited, or he would hardly have made so rash an assertion as that contained in his last sentence. The starving of plants and animals to induce them to breed is one of the elementary axioms of both gardeners and stockkeepers.

I now come to Dr. Ross's letter, which, although somewhat patronising in parts, is altogether more to my taste than some others. He has properly referred me to Mr. Herbert Spencer, but I am afraid of venturing into his book, for fear that I should open upon myself the floodgates of Evolution. It is not the general problem of Evolution about which we are now at issue, but that limited form of it called Natural Selection. It is satisfactory, however, to find that, according to Dr. Ross, Mr. Herbert Spencer admits the main facts upon which my argument is founded. His doing so is quite a relief after the jaunty manner in which some of your correspondents have spoken about the matter. To speak of its being late in the day to be now defending Mr. Doubeday, to tell one that "what one says is ludicrous," "a monstrous error," &c., &c., is surely a sign that the crowing of the Gallic cock has been mistaken for more substantial arguments. I am very sorry that Mr. Spencer's book is not in my library, and that I cannot meet with it at the Manchester Free Library or Mudie's, so that until I am aware of Mr. Spencer's arguments I cannot say how far they affect the position I maintain. If the facts are admitted, as Dr. Ross says they are, I confess that I cannot see any other interpretation of them than the one given by Mr. Doubleday. Will Mr. Ross do me the favour of pointing out what other explanation they are capable of?

Mr. Wallace has misunderstood me if he thinks me capable of sneering at the good and sound work that has been done by himself for many years, the value of which I am as conscious of as I am of the worthlessness of mere Olympian dogmatism. Sneers are only justifiable in answer to contempt, and if he feels aggrieved with any of my words I withdraw them.

Mr. Wallace says my criticism of the phrase Survival of the Fittest is satisfactory. In regard to the phrase I used, and for which I was severely flouted by Mr. Wallace, he says it is unknown to Darwinians; that may be, but it can hardly be said to be unknown to Mr. Darwin himself. Speaking of the problem of the conversion of varieties into species, the latter says: "The inevitable result is an ever recurrent struggle for existence. It has been truly said that all nature is at war, the strongest ultimately prevail, the weakest fail, and we well know that myriads of forms have disappeared from the face of the earth" ("Variation of Animals and Plants under Domestication," i. 5). Let me especially commend this extract to Dr. Lionel Beale, for whom I entertain the profoundest respect, notwithstanding his vituperation of myself.

I find a difficulty in meeting Mr. Wallace's latest arguments, because they are entirely *a priori*, and Mr. Wallace asks me to admit as premisses the very thing I dispute, namely, the relative sterility of strong and hearty animals and plants. I cannot see the relevancy of his quotation of the effects of cross-breeding to the present argument, unless he means to infer that crosses are more vigorous and stronger than pure bred animals, on which position I should like to be furnished with a little evidence. Again, I cannot test the supposititious problem put by Mr. Wallace as to the strongest individual of an animal's progeny eventually being the stem-father of the race. He takes for granted that it is, and in doing so begs the question. I can only say the only experiments I know do not favour Mr. Wallace's *a priori* view, and that in the cases we can experiment upon, not the least satisfactory of which is the case of man himself, the condition most favourable to fertility, as I have quoted many examples to show, is that of comparative depletion.

Mr. Wallace, as before, is spare of instances. I can only extract two *bonâ fide* ones from his letter. He tells us the strongest bull leads the herd; this proves nothing, unless we are to infer from it that his progeny is the most numerous, and that the biggest and strongest therefore survive. I prefer to quote Mr. Darwin himself where I can. If Mr. Wallace's instance be worth anything, how does he account for the following: "The decrease in size of the Chillingham and Hamilton cattle must have been prodigious, for Prof. Rutimeyer has shown that they are almost certainly the descendants of the gigantic *Bos primigenius*. No doubt this decrease in size may be largely attributed to less favourable circumstances. Yet animals roaming over large parks and fed during severe winters can hardly be considered as placed under very unfavourable conditions" ("Variation of Animals and Plants under Domestication" ii. 119). What Mr. Darwin says of the wild cattle is equally true of the reindeer kept by the Laplanders compared with the wild ones on the Samoyede tundras, of the red deer of our larger forests compared with the skeletons of red deer from the turbaries, and is, perhaps, generally true of semi-wild races where man has not intervened with the special object of increasing the size by breeding from the largest individuals only.

In regard to the carnivora, I know of no reliable facts. I am not proposing the monstrous paradox that those animals which are so weak, diseased, or decrepid that they cannot sustain life at all, are the only ones that keep up the succession of the animal world. The toothless tigress, who cannot kill her food and is starving, will most certainly not be the mother of a long race. She can do nothing but die. But I say that, judging from analogy, it is probable that the lean and comparatively ill-fed tigress will breed more freely than the man-eater supplied with regular and abundant food.

The banks of the Chinese rivers and the rough country in the south and south-west of Ireland are both inhabited by teeming populations, remarkable for their poverty and fertility, and remarkable further for sending out immense colonies, which supplant wherever they go, in Mantchuria, in Songaria, in Glasgow, in Manchester, in New York, the strong hearty, indigenous races. This being so (and I only quote these two as examples of a whole class), when Mr. Wallace asks the question, "How can weak and sickly parents provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves?" I can only reply that they actually do so: *Veni, vidi, et credi.*

I must correct a wrong impression that Mr. Wallace has got hold of. In this controversy I have no theory; my only theory is that Natural Selection is an ingenious but fallacious explanation of the varieties of life.

I cannot understand Mr. Wallace's last sentence if it be meant for an argument; while if it is only a *jeu d'esprit* and witticism, it requires a commentary to tell us where the point is.

Lastly, I will consider Mr. Wallace's reiterated complaint that I have only treated of what is in most cases the least important factor in determining the continuance of species. Let me turn very briefly to another of these factors put prominently forward by both Mr. Wallace and Dr. Beale, namely, "Obscure Colour."

We are not arguing about exceptional and individual cases, we are dealing with a general law, applicable or supposed to be applicable to the great majority of cases. Can it be said gravely that obscure colour has tended to the preservation of particular forms of life to the exclusion of others, not in a few exceptions, but as a general biological law?

Daylight, it will be admitted, is more likely to disclose an object than darkness. If we compare diurnal forms of life with nocturnal ones, we ought to find, if I read the tendency of the Darwinian argument rightly, that in the daylight when a sombre, obscure, or indifferent colour, would be of great service to hide an object, that there are a much smaller proportion of conspicuous forms of life abroad than at night when there would be no such need for obscurity, and a bright colour might be worn with impunity. Is such the fact?

Again, if we compare the animals and plants that live in tropical climates, where the light is intense, with those found in temperate and severe ones where the light is not so great and objects are not so prominent, do we find that the former has a comparative monopoly of conspicuous objects, or do we find rather that the reverse is the case, and that all the brightest objects we know in nature—the parrots, macaws, humming birds, butterflies, orchids, &c.—are found in the greatest profusion in the tropics, while we proverbially console ourselves for the absence of colour in our birds by boasting of their singing, and hang the beetles of Brazil in necklaces round our sisters' and wives' necks, while we crush our sombre representatives of the same class under our heels? Is it not equally true of the sea? In the Mediterranean, for instance, do not the brightly decked out gurnards and mullets far outnumber the dingier fish, while on the banks of foggy Newfoundland the sober tinted cod and ling are the prevailing types? In the former we have the clear blue water that washes round Sorrento pierced through and through by the blazing sun, while in the latter we have everything gloomy except the fisherman.

If we separate the animal world into flesh eaters and vegetable eaters, we ought to find, if this theory be true, that the former (which as a rule are not themselves the prey of other animals) are more conspicuous than the latter, since they have less reason for adopting a secret costume. But is it so? Are the hawks and owls and carnivorous beetles as classes more conspicuous than their victims? Is it a not fact that the most beautifully coloured creatures are as a rule the most helpless, weak, and accessible; that those animals which are supplied by nature with weapons of defence or are strong and can defend themselves, are as classes more obscure in colouring than those not so protected, and that the same rule applies to plants which are poisonous, nauseous, or protected by thorns? If these facts be true in the great majority of cases, we have another factor in Mr. Darwin's theory which is not satisfactory, and the cases quoted to support it become mere exceptions, which, by being exceptions, disprove the particular law he is maintaining. This letter has already exceeded reasonable limits, and I must postpone a further consideration of this and other objections to another occasion.

Derby House, Eccles HENRY H. HOWORTH

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MR. HOWORTH'S objections to the theory of Natural Selection have been fully answered. I therefore wish to direct attention to another objection which has been recently advanced, and which has not, so far as I know, been specially refuted. The objection is stated by its author in the following terms:—"And it has been affirmed that to 'the primitive properties of molecules' and 'Natural Selection' may be referred all the varying forms and structures known to us, as well as all the phenomena of the living world. But such terms explain nothing. By their use further inquiry is discouraged, and the mind bent upon investigating the secrets of Nature is misled at the very outset. Can any one of these very pretentious phrases be resolved into anything more than the statement of a fact or facts in the form and language of an explanation? Natural Selection is the formation of species, and species are produced by Natural Selection. Crystallisation is the formation of crystals, and crystals are produced by the operation of crystallisation."

This passage is extracted from p. 58 of "The Mystery of Life"—a little work by Dr. Beale, which was published a few months ago. Dr. Beale has a keen appreciation of the "ludicrous." He thinks Mr. Howorth's misrepresentation of the Darwinian theory "very curious and even ludicrous," and in the closing sentence of his letter in NATURE, he appears to have a bit of fun to himself which ordinary mortals cannot understand; and if he can prove that Natural Selection is a mere abstract statement of the fact that species are in some way or other formed, the Darwinian theory is the most "ludicrous" ever presented to mankind. Probably Mr. Wallace may take a different view of the subject, and he may even think that the objection is more ludicrous than the theory; at any rate, no harm can result from bringing Dr. Beale and the champions of Natural Selection face to face, so that stricter tests than the "ludicrous" may be applied to ascertain whether the truth lies in the theory or in the objection. JAMES ROSS

Newchurch, July 24

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THE last paragraph of Mr. Howorth's letter in NATURE of July 13 reminds me of a fact which I have often noticed, and which is, I suppose, well-known to botanists, viz. that certain creeping plants which root at the joints, flower sparingly unless the sprays are so disposed that they cannot take root. I refer especially to the *Lysimachia nummularia* (larger moneywort or "Creeping Jenny"). This plant blossoms comparatively little when allowed to trail in the moist soil which is its natural *habitat*, and in which alone the leaves look healthy and thriving. A spray trained off the flower bed on to a flag-stone, or a plant grown in a pot so as to hang over the edge and not be able to take root, will look sickly, but will be covered with flowers. I think I have noticed the same thing in connection with the periwinkle.

Gardeners cut off the runners of strawberries and the suckers of fruit trees to increase the crop, because, as they say, runners exhaust the plant.

But is not the case, rather, that the possibility of continuing its own life by taking root at the runners makes the plant's constitution, as it were, lazy about propagating its kind?

It is, perhaps, worth noticing that the cutting off the runners or suckers does not in any way weaken the plant, or cause it to become sickly, but it does prevent the indefinite prolongation of the individual life.

THE OWNER OF A "WEED GARDEN"

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## Recent Neologisms

WRITING, as I did, from a little Midland village, where access to an English dictionary was impossible, I am not surprised to find that three words, which I treated as recent coinages, were only re-introductions. *Survival*, *impolicy*, and *indiscipline*, are all so naturally formed, that, whether old or new, they are "welcome to stay." My end was answered by putting a brand on Mr. Wallace's *prolificness*, by way of contrast. If he is bent on using that monster, he will help to naturalise it by spelling it with *ck* (instead of *c*) like *thickness*. But surely he is not driven between the Scylla and Charybdis of *prolifickness* and *prolificacity*, when *prolicity* is staring him in the face. For my part, I pray that the whole family will (to quote Sylvester again) "shake swift wing," and be no more seen. By-the-bye, I find the verb to *handwrite* in the *Quarterly Review*, April 1871, p. 332. That is a good, if not a new word, and well deserves re-introduction.

C. M. INGLEBY

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## The British Association and Local Scientific Societies

IT is to be regretted that the British Association does not exert its influence in stimulating local scientific societies towards greater efforts for the formation in their museums of collections representing the Geology and Natural History of their respective neighbourhoods, so that they might constitute local monographs. Such a system, combined with a central museum in London, representing an epitome of the collections throughout the country, would tend to the advancement of science with greater rapidity and accuracy than at present, when the provincial museums are little better than overstocked curiosity-shops, and with no recognised plan of arrangement which is greatly wanted. In general there is little space for additions of importance, from the fact that the museums already contain large miscellaneous collections, unconnected with the neighbourhood, and of little use to anybody. Many valuable private collections exist throughout the country, representing the geology, &c., of various localities, which are eventually too often dispersed and lost to the district where they would be most useful and instructive. Private collectors would probably show more public spirit, if greater zeal and better judgment were shown by local societies.

F. G. S.

### Science Teaching in Schools

In the number of Nature for April 20, there is an article containing an account of a "Plan for Teaching Science in Ordinary Schools, submitted to the London School Board by Mr. J. C. Morris."

I will ask you to give me a little space for some details respecting an educational experiment I made in 1867, 1868, and 1869. My object was to test the value of a plan much resembling that referred to. By means of circulars, addressed to more than a hundred of the London clergy, I obtained permission to have the children in seven large schools instructed in science. Four competent teachers put their services at my disposal. One of these gentleman is now chemist in iron works, two are art masters, and the fourth, having obtained one of the Whitworth Scholarships, is a student at Owen's College. I mention these facts to show the sufficiency of their knowledge. Three of them had had considerable experience in teaching. Twenty-two classes were formed, the total number of pupils exceeding 800. The principal subjects taught were chemistry, geology, physical geography, practical geometry, and mechanical drawing. The lessons were from one to two hours in duration on two days in the week at each school. But my plan differed from Mr. Morris's, inasmuch as thirty-five to fifty-five lessons were generally given in a subject before proceeding to a new one. He suggests that "a single teacher could get through three or four subjects annually, so that in two or three years he would have completed the full course in each school." This plan would give from twenty-two to thirty lessons per subject if I rightly understand his meaning. We fixed a small fee, but seldom obtained it, as we found that any attempt to press for payments would have reduced very materially the numbers in the colleges. The pupils were frequently examined, and those who appeared likely to satisfy the minimum requirements of the science department were sent in to the May examinations.

The following are some of the observations I made at the time :—

1. Few of the children appeared to obtain anything like sound and comprehensive knowledge of the facts the teachers put before them.

2. The great majority failed to express clearly on paper any ideas which an oral examination showed they had gained.

3. Most of them appeared to forget a subject within a few weeks after the discontinuance of instruction, or the substitution of another branch of science. The utter forgetfulness shown by whole classes was sometimes almost startling.

4. The papers worked by the girls at the examinations were superior to those produced by the boys, showing a more intelligent knowledge of the subjects they had been taught. This fact may, however, have resulted from accident, as comparatively few girls received instruction.

T. Jones

The College, Stony Stratford

### Ocean Currents

Mr. Laughton does not seem to observe that the subject of Ocean Currents involves several distinct issues, which may be discussed apart from each other. It is, of course, obvious that if the temperature explanation of the vertical circulation fails, then no illustration of the horizontal circulation, if founded on the temperature theory, can be really effective. But it is admissible to inquire separately whether the horizontal circulation *would* result from a vertical circulation such as the temperature theory suggests. For an objection has been urged against the theory on account of the nature of the horizontal circulation (see Herschel's "Physical Geography.") The express object of the experiment I have suggested is to show that this particular objection is unsound, or rather to illustrate the theoretical considerations argued in my essay on the Gulf Stream in the *Student* for July 1868.

But even in so far as my suggested experiment, like the similar one carried out by Dr. Carpenter, illustrates the production of a vertical circulation, I deny that Mr. Laughton's objection is valid. It is quite unnecessary to have a thermometric gradient resembling that in the terrestrial oceans. Whether Dr. Carpenter's view be correct, according to which the Arctic regions are the place where the Ocean Currents have their birth, or whether the view I have advocated be preferable, that the chief source of the oceanic circulation is to be recognised in the effects of tropical and subtropical heat, it is clear that we are rather concerned with the integrated effects of one or other cause (or of both causes combined) than with the amount by which temperature increases per mile of distance towards the equator. As I have already remarked, I conceive that any reasoning by which the contrary could be maintained would subvert the accepted and surely sufficient explanation of the trade and counter-trade winds. (The experiment described in illustration of this explanation in Daniell's Meteorology is open to much graver objections than Mr. Laughton has urged against Dr. Carpenter's experiment) And I note that here Mr. Laughton agrees with me, except that on the strength of his thermometric gradient he is as ready to give up one theory as the other, whereas I see no objection to retaining both.

The very word "gradient" should suggest the true answer to Mr. Laughton's reasoning. A gradient of one in ten (say) will produce little velocity in a rolling body traversing such an incline for a distance of only a few feet, but if the incline be a few miles long the body rolling down it would acquire a velocity exceeding that of our swiftest express trains. Or again, suppose Dr. Carpenter, desiring to illustrate the subject of springs of water, employed a conduit-pipe inclined 45 degrees to the vertical, would it be any valid objection to the illustration to urge that in most natural springs the water gradients are very much less? He could surely answer that the principle of his illustration was in no way affected by this circumstance, for if the water-gradients in nature are small, they act over a much longer range than could be employed in his experimental illustration. So with Mr. Laughton's temperature-gradients; they are very small indeed, but their action extends over a very great distance; and as in the two former cases the total fall measured vertically is to be looked upon as the true cause of the resulting motions, so I conceive that the total difference of temperature between Polar and Equatorial waters is to be considered in discussing the temperature theory of oceanic circulation.

I note, by the way, that "solar light" (by misprint or through a *lapsus calami*) was substituted for "solar heat" in my former letter. I did not think it necessary to correct this earlier, as I imagined the error would mislead no one. Like Mr. Laughton I "do not see what effects solar light can ever be supposed to produce," on the ocean, at least, in producing circulation.

I venture to remind Mr. Laughton that Dr. Carpenter's position in this matter is very different from his or mine. *We* have theorised on this subject, whether with more or less soundness time will show. But Dr. Carpenter has brought striking and important facts to our knowledge; and *if* there has been "an air of triumph both in Dr. Carpenter's lectures and writings" about ocean currents, he has had better cause for triumph than the mere success of a lecture-room experiment could have afforded him.

Richard A. Proctor

Brighton, July 21

### Western Chronicle of Science

I would beg to be allowed one or two remarks with reference to the very favourable review of the "Western Chronicle of Science" which appeared in last week's Nature.

It is not a "common Cornish habit to hang heavy jackets, great-coats, &c., on the lever of the safety-valve," and the farmers do not, *as a rule*, "mix guano with lime a few days before applying the manure." The editor has seen both these absurdities performed, and has used them as beacons to warn young men what to avoid. I may also remark that Mr. Williams's Paper is on Scientific *Mining* and not Scientific Nursing.

Falmouth, July 22 J. H. Collins

### Formation of Flints

Nothing can be more annoying to a reporter than to find he has not satisfied those whose statements it has been his duty to condense. I have therefore carefully examined the report to which Mr. Johnson takes exception in his letter to you of the 11th inst., and I regret that I am unable to acknowledge any error.

If Mr. Johnson will be good enough to consult some of those who were present at the meeting to which he refers, he will, I think, be more inclined to admit the accuracy of the report.

The Writer of the Report

## *NOTES*

THE arrangements are now completed for the session of the British Association, to commence on Wednesday next; and we may fairly expect a successful meeting. The large number of foreign *savants* who have announced their intention of being present will add greatly to the interest of the meeting, and the inhabitants of the pleasant Scottish capital seem determined to display to the utmost their well-known hospitality, both in a public and private capacity. The President's Address, as we have already announced, will be delivered on Wednesday evening at 8 o'clock; and at the first meeting of the General Committee, at 2 P.M. on the same day, the Presidents, Vice-presidents, and Secretaries of each Section will be appointed. On Thursday morning at eleven, the different sections will assemble in the rooms appointed for them, for the reading and discussion of reports and other communications; and the sittings will be resumed at the same hour each day till Tuesday, August 8. All further information may be obtained by those wishing to be present from the local secretaries, 14, Young Street, Edinburgh.

THE Emperor of Brazil has signified his intention of being present at the approaching meeting of the British Association.

THE Indian Civil Engineering College at Cooper's Hill will be opened by the Secretary of State and members of the Council of India on Saturday, August 5th.

WE have great pleasure in recording the inauguration of an effort to raise a memorial to the memory of the late Prof. William Allen Miller, and desire to call thereto the attention of all our readers who appreciate the valuable contributions to science for which we are indebted to that eminent chemist. The committee consists of Dr. Miller's fellow-professors at King's College and fellow-labourers in science, with the Rev. Principal Barry as chairman, Profs. Bentley and Bloxam, and Messrs. Cunningham and Tomlinson as secretaries, and Prof. Guy as treasurer. The intention is to raise a fund to be devoted, first, to the preparation of a bust or portrait of the late Dr. Miller, and, secondly, to the institution of a prize or scholarship in connection with King's College, and bearing his name. The ordinary amount of subscription is to be one guinea, and the list of subscribers will be published without any statement of the amounts subscribed.

THE International Congress of Prehistoric Anthropology and Archæology, which was last year postponed on account of the war, will be held this year at Bologna under the presidency of Count Gozzadini, and with Prof. Capellini as organising secretary. The sittings will commence on the 1st of October, and will continue during the following week. Mr. John Evans, F.R.S., of 65, Old Bailey, has consented to receive the subscriptions of English members, the amount of which has been fixed at ten shillings, and the payment of which entitles the member to the volume of proceedings.

THE President and Council of the Royal Geographical Society have addressed a letter to the Vice-Chancellors of the Universities of Oxford and Cambridge on the subject of the teaching of Physical and Political Geography. They observe that in the scheme now under the consideration of the Universities for the examination of boys between the ages of sixteen and eighteen from all the first-grade schools of England, neither branch of geography is included in the list of subjects out of which the boys are at liberty to choose any five for examination. They point out that geography has always been regarded as an essential, though subordinate, element of liberal education, and that it has been more and more frequently selected as their subject by candidates who pass the examination of the Science and Art Department of South Kensington. They hope that the Universities may see reason to repair the omission in the scheme above alluded to, and that they, by this and other means, will not only rescue geography from being badly taught in our schools, but will raise it to an even higher standard than it has yet attained.

THE examiners in the School of Law and Modern History at Oxford have given notice that at the next examination in December, Geography will form an important branch, and that papers will be set in the Honours Examination on this subject alone.

BY the appointment of Mr. Alexander Herschel to the Professorship of Experimental Philosophy at the Newcastle College of Physical Science, a vacancy occurs in the chair of Natural Philosophy at Anderson's University, Glasgow. Applications must be sent to the secretary by the 26th of August.

IN a letter to the *Athenæum*, the widow of the late Prof. De Morgan invites those who possess letters or other mementoes of the illustrious mathematician to lend them for the purpose of preparing a biography.

PROF. MARSH, of Yale College, has just started out on a second expedition for scientific exploration and discovery in the far West, which we trust will be still more fruitful in interesting results than the first one which brought to light so many extraordinary forms of fossil animals, that have been briefly described by him in the *American Journal of Science*, and referred to from time to time in our pages. His party for the present season will consist of thirteen besides himself, embracing quite a number of his companions of last year, and it is his intention to spend five or six months in searching the cretaceous and tertiary strata of the Rocky Mountain region and the Pacific coast for vertebrate fossil remains. With the experience of the past year and ample facilities, he expects to make very extensive collections.

THE New York Commissioners of Fish and Fisheries seem unwearied in their efforts to stock the waters of the State with the best varieties of fish. Among other results obtained by them, has been the hatching out during the past season of 3,000,000 shad eggs, or three times the total catch of the Hudson River. They have also bred several millions of white-fish, a million of salmon-trout, while of such fish as the black bass, pike, perch, and other varieties, they have supplied large numbers to those who would take and protect them. The period of their appointment will expire in the course of a year; but by that time, even if the commission should not be renewed, they will have made a most important impression upon the subject of the production of the fresh-water food supply.

COLLECTORS of scarce works in Natural History, curiosities, stone implements, rare specimens, &c., should not neglect the opportunity of inspecting the collection of a well-known collector, which will be sold at Thurgood and Giles's Auction Room, 7, Argyll Street, Regent Street, on July 31st and three following days; and will be on view two days before the first day's sale.

THE old adage about civilisation, or at least science, softening manners, is certainly being exemplified just now in France. M. Paul de Saint-Victor having given utterance to a violent tirade of undying hatred against Prussia, M. de Quesneville thus replies in the *Moniteur Scientifique*:—"L'humanité veut qu'on oublie; l'interêt des peuples, qui sont tous frères, la raison, le bon sens, tout nous dit que dans cette guerre qui vient de finir, la France, qui a succombé, doit chercher sa revanche, non dans la puissance de la force brutale, mais dans sa régénération sociale, et qu'elle doit demander à son génie de prouver sa supériorité dans les sciences, dans les lettres, et dans les arts, et que ce doit être là sa seule vengeance. C'est par là que la France est vraiment invincible, c'est par là qu'elle doit rester la grande nation, la nation aimée et préférée, et non dans une lutte d'obus et de chassepots." Noble words these, and full of the most rare form of generosity, that of the vanquished towards the victors; a fitting response to the note of reconciliation given forth by the venerable Baron Liebig, to which we referred some weeks since.

NORFOLK has always been noted for its devotion to ornithology. The "Transactions of the Norfolk and Norwich Naturalists' Society for 1870-71" contains several interesting and useful papers, among which we may especially mention "On the Ornithological Archæology of Norfolk," by T. Southwell, "On a Method of Registering Natural History Observations," by Prof. Newton, "A Natural History Tour in Spain and Algeria," by J. H. Gurney, and "On Certain Coast Insects found existing inland at Brandon, Suffolk." The author of this last paper believes that these species must have survived for several thousand years, since the great valley of the fens was submerged. The insects found are peculiar to coast sand-hills, the nearest of which are at a distance of forty miles; and yet, "in spite of their isolation and alteration of condition, the species are as true and as clearly defined as those of our present coast."

MR. W. G. M'IVOR, Superintendent of the Cinchona Plantations of the Bengal Government in British Sikkim, has published a lengthy report, of which the following is an abstract:—"The plantations are situated in the Valley of Rungbee in the Himalayas, about thirteen miles from Darjeeling, which seems admirably adapted for the growth of cinchona. The climate is very moist, being rarely free from rain. Nevertheless the state of the plantations is reported as very unsatisfactory; the plants have nothing like the luxuriant foliage which characterises those grown in Southern India on the Nilgheries. They seem to thrive for three or four years at the most, and then become diseased." Mr. M'Ivor says that trees of equal height do not produce so much bark as in the South of India, being of more slender growth, and the bark being thinner.

A GREAT demand for the English sparrow in various parts of the United States has induced their importation from England and Germany in large numbers; but in many instances where this has been done in large cages, most of the birds have died on the passage. In one instance, where four hundred were placed in two cages, only seven were safely landed in New York. Persons who have given this subject their attention, advise that the importations be made in long low cages, known as store cages, which are two or three feet long, about nine inches high, and twelve from back to front, with perches within two inches of the bottom. In a cage of this kind three or four dozen can, it is said, be readily transported, provided they be supplied with proper food, as well as with sand and fine gravel and plenty of water.

M. WURTZ has announced to the French Academy of Sciences that a young chemist in his laboratory has succeeded in transforming lactose, or the uncrystallisable sugar of milk, into dulcose or dulcine, the sugar of mannite, which may easily be obtained in very beautiful crystals, by the successive reaction of hydrochloric acid and sodium-amalgam.

M. FELIX PLATEAU has recently undertaken a number of experiments to determine the question whether the cause of the death of fresh-water animals when removed to sea water, and of marine animals when removed to fresh water, is the difference in the density or in the chemical constitution of the water. His observations were made mostly on various species of Articulata; he found that those fresh-water species which possess an aërial respiration can survive the change to salt water, while those which possess only a branchial and cutaneous respiration die quickly. By experimenting on water made denser by the solution of sugar, M. Plateau came to the conclusion that the density of the water is not the destructive agent, but a portion of the salts held in solution. The chlorides of sodium, potassium, and magnesium, he found to be very quickly fatal to fresh-water species, while the sulphates of magnesium and calcium had no prejudicial effect. In the same manner the death of marine animals in fresh water appeared due to the giving off of sea-salt from their bodies to the surrounding fluid. All these facts he believes explicable from the laws of endosmose and diffusion.

"A KEY to the Natural Orders of British Wild Flowering Plants," by Thomas Baxter, is designed to provide an "easier, although perhaps less scientific, method of identifying the orders of British Wild Flowering Plants than is generally found in analytical keys." There is no royal road to botany, and we doubt whether it is any real advantage to the student to sacrifice scientific in favour of superficial characters.

A CORRESPONDING member of the Glasgow Natural History Society, having been lately in Panama, has contributed to a local journal in the latter city an interesting account of the ants of the country. He describes a curious covered way or tubular bridge. In tracing one of these covered ways he found it led over a pretty wide fracture in the rocks, and was carried across in the air in the form of a tubular bridge of half an inch in diameter. It was the scene of busy traffic. There was nearly a foot of unsupported tube from one edge of the cliff to the other.

MR. THWAITES, in his "Enumeration of Ceylon Plants," says that from the large extent of forest land which has been and is now being appropriated to coffee cultivation, there is little doubt that some of the indigenous plants will in time become exceedingly rare, if not altogether extirpated, or exist only in the Botanic Garden, into which as many as possible are being introduced. The obtrusive character, too, of a plant brought to the island less than fifty years since is helping to alter the character of the vegetation up to an elevation of 3,000 feet. This is the *Lantana mixta*, a verbenaceous species introduced from the West Indies, which appears to have found in Ceylon a soil and climate exactly suited to its growth. It now covers thousands of acres with its dense masses of foliage, taking complete possession of land where cultivation has been neglected or abandoned, preventing the growth of any other plants, and even destroying small trees, the tops of which its subscandent stems are able to reach. The fruit of this plant is so acceptable to frugivorous birds of all kinds that, through their instrumentality, it is spreading rapidly, to the complete exclusion of the indigenous vegetation from spots where it becomes established.

## *METEOROLOGICAL OBSERVATORIES*

IN the part of the Quarterly Weather Report of the Meteorological Office just issued, for January—March, 1870, the following information is given with regard to the observatories from which the observations are recorded, accompanied by the illustrations which the courtesy of the committee enables us to reproduce. As correct an idea as possible is thus given of the value of the thermometrical and anemometrical observations published by them, and the local influences which may exert an effect in each case.

VALENCIA.—The observatory is situated close to the shore on the south side of the island, about three miles from the open sea.

The anemograph is on the roof of the house, which is two stories high. Its exposure is fairly good, for although it is situated in a valley, with hills of the height of about 1,000 feet to the south and south-east of it at a distance of three miles, and with a slight hill about 700 feet high distant three-quarters of a mile on the north-west of it, the country towards the other points of the compass is quite open, and the situation for wind is as favourable as can be obtained on that very rugged coast. The only point from which the wind is materially deflected or checked by local influence is the north-west. The house is an ordinary dwelling house of small size.

The thermograph is on its north side, facing due N.W. ½ N., and on the first story. The bulbs of the instruments are at a height of twelve feet above the ground, and about twenty feet above the sea level. The exposure is very good, as there are no buildings or trees in the vicinity to affect the readings.

ARMAGH.—The observatory is on a rising ground close to the town; it is situated in the centre of an ordinary garden and pleasureground, containing trees and shrubs of moderate size.

The anemograph is erected on the roof of the house, and raised seventeen feet above it, and is thoroughly well exposed to all points, excepting that the country about is undulating and fairly well wooded, which has the effect of retarding the motion of the air.

The thermograph screen is erected on the north side of the meteorological observatory; the bulbs are at the distance of four feet from the ground, and about 206 feet above the sea level. The exposure of the screen is good, though there are trees and shrubs about it. However,

VALENCIA

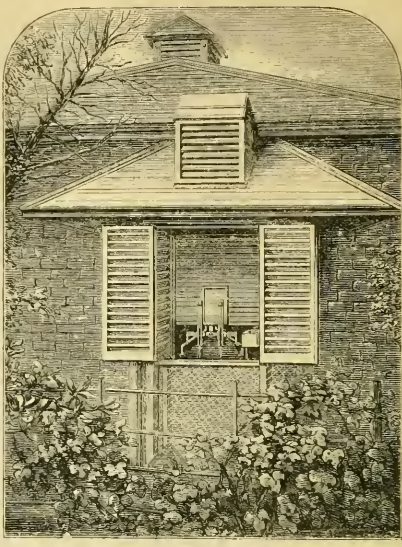
Dr. Robinson has satisfied himself by an independent series of observations that the record taken in the screen gives the true temperature of the place.

GLASGOW.—The instruments are at the astronomical observatory, which is placed on a slight rising ground at the west side of the town, and commands a clear view of the horizon in all directions. It occupies a central position in the valley of the Clyde, which is about 16 miles in breadth at that place. The bounding hills to the north are about 800 feet in height, those towards the south are about 400 ft. high.

The prevailing south-westerly winds sweep along the estuary of the Clyde and reach the observatory without much interruption.

The exposure both of the anemograph and of the thermograph screen is very satisfactory. The former is on the roof of the building, the latter is attached to the north wall of the tower in which the equatoreal is placed. The bulbs are 7 ft. above the ground, and about 190 ft. above sea level.

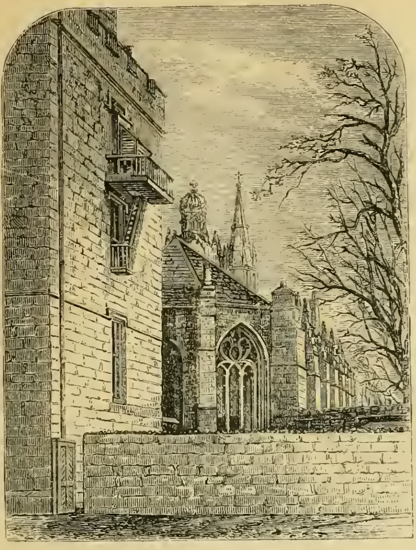
ABERDEEN.—The observatory is at King's College in Old Aberdeen, and lies on a plane gradually rising from



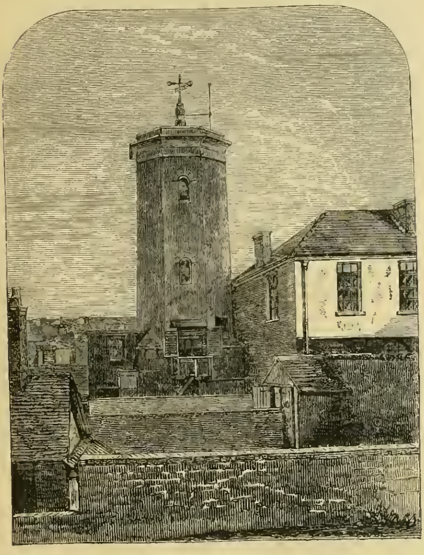
ARMAGH

GLASGOW

the sea, from which it is distant about a mile. There are no irregularities of surface in the vicinity, excepting the two river valleys of the Dee and Don, which are not of great importance. The ground immediately about the buildings is 46 ft. above the mean sea level.



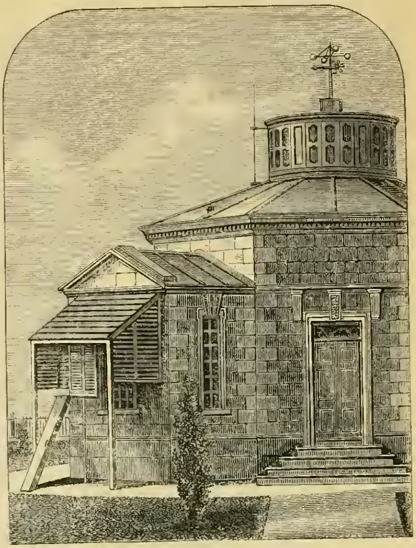
ABERDEEN



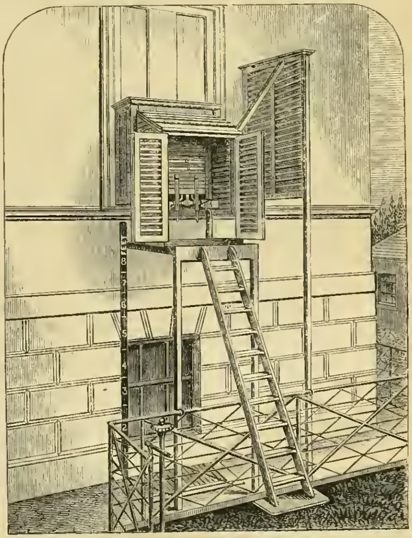
FALMOUTH

The anemograph is erected on the roof of the building, at a height of 72 ft. from the ground. It is well exposed on all sides.

Great difficulties were encountered in obtaining a site for the thermograph screen. The north side of the college



STONYHURST



KEW

is almost entirely occupied by the chapel. One wall of the building in which the physical cabinet and lecture-room are situated also affords a north aspect, but unfortunately there are trees growing at a short distance from it, which would entirely check the free circulation of air about the instruments were the screen set up at the usual elevation of about 6 feet above the ground.

Accordingly the window on the second story of the building was selected. It affords a free exposure to the north, but is at a level of 41 ft. above the ground, and about 87 feet above the sea level.

This elevation will of course exert a considerable influence on the thermometrical observations recorded.

FALMOUTH.—The establishment of an observatory at this station was beset with considerable difficulties; the building in which the Royal Cornwall Polytechnic Society holds its meetings was unsuited to the purposes of a meteorological station. Accordingly a tower was erected at the south-east corner of the bowling green, on the top of one of the hills on which the town is built.

The anemograph is on the summit of the tower, well exposed on all sides; but from the fact that the ground in the neighbourhood is uneven, the hill sloping rapidly down to the harbour, it seems probable that the force of the wind is not quite true, especially when it is easterly.

The position of the thermograph screen is far from being quite satisfactory; however, a better exposure could not be obtained. The screen is attached to the north wall of the tower, at an elevation of 11 feet above the ground, and about 200 feet above sea level.

It will be seen that there is the wall of a dwelling house at no great distance to the westward, which may possibly affect the instrument by radiation, and also interfere with the free circulation of the air.

STONYHURST.—The observatory stands in the centre of the college garden, which is on a gentle slope facing S.S.E., 381 feet above sea level. The anemograph stands on a cylindrical roof 12 feet in diameter and 4 feet 5 inches in height. The total height of the cups above the ground is 30 feet.

The country around, including the college grounds, is wooded, but not very thickly so, and the trees are in general small.

The nearest trees whose height could materially influence the anemograph are at a distance of about 200 yards, bearing from N. by W. to N. by E.

The main building of the college is placed at the N.W. of the observatory, at a distance of 193 yards, its angular height above the roof of the observatory being 1° 37′, and bearings from N. by W. to W.N.W.

The nearest hill is the Longridge Fell, whose nearest point is about two miles from the college. It extends from N. by W. to W. by N., and its highest point is 4° 1′.

Pendle Hill is at five and a half miles distance E.N.E.; height 2° 5′. Between these hills the country is very open. To the eastward there are hills at about four miles distance, height about 1°. To the S. and S.W. the land is low.

It will be seen from this that the anemograph is fairly well exposed to the different points of the compass.

The thermograph screen is attached to the north wall of the observatory, the bulbs are at an elevation of 7 ft. above the ground. The exposure is good.

KEW.—The observatory is situated in the old Deer Park at Richmond. It is a small building, which is well exposed to the wind, excepting on the west side, where there is a row of trees distant about 150 yards, which must materially affect the velocity of the wind. The country about is also well wooded.

The anemograph is placed on the dome.

The thermograph screen is attached to the north wall of the observatory within ten feet of the west end of that wall, at a height of ten feet above the ground, and about fifty above sea level. Its exposure is good.

We hope to take another opportunity of reviewing the volume itself.

## ON THE RECENT SOLAR ECLIPSE*

(*Continued from page* 233)

### II.—POLARISCOPIC OBSERVATIONS

WITH regard to the polarisation experiments, by the kindness of Mr. Spottiswoode I am enabled to show you, in a very clear way, the *raison d'être* of the polariscopic observations made during this and former eclipses; but the polariscopic ground is a wide one, and it is not my intention to cover it to-night.

I have had this arrangement of lamp, reflector and prisms made so that you may see how the polariscope can determine the percentage of reflected light at different angles, and the direction of reflection. Assume this lamp to represent the sun, let this reflector close to the lamp represent a particle near the sun, reflecting light to us, we shall naturally have the light reflected at a much larger angle than if the reflector were close to the screen representing a particle in our own air. Having this idea of the angle of reflection in your minds, and the fact that the larger the angle under these conditions the more the polarisation, if you take this lamp, as I have said, to represent the sun, and this mirror to represent any particle, of whatever kind you choose to imagine, it is clear that in order to get the maximum polariscopic effect from that particle, you must have it so situated that it will reflect light at a considerable angle to the beam coming from this lamp.

Now it is clear that in order to polarise the beam most strongly, I must place the reflector close to our imaginary sun. If I so place it as to represent a particle in our own atmosphere, the angle will be so small that the polarisation of the light will hardly be perceptible.

Here is our sunlight, which we will polarise at as great an angle as we can, by placing the reflector close to the imaginary sun, and send it through this magnificent prism which Mr. Spottiswoode has been good enough to place at our disposal; and in the path of the beam I will place an object so that you determine whether there is polarised light. [Experiment.] You see there is considerable brilliancy in those colours; their brilliancy depending upon the amount of polarisation.

Now if, instead of having our reflector close to our imaginary sun to represent a particle in the sun's atmosphere, we place it near the screen to represent a particle in our own, in which case the angle is extremely small, the brilliancy of the colours will entirely disappear. You see it has disappeared. The colours, as colours, are distinguishable, but their brilliancy has gone.

That is the rationale of the polariscopic observations, which have been made on the occasion of the last eclipse with more elaboration than they ever were before. If we found the corona to be strongly polarised, this was held to be a great argument in favour of the corona being a real solar appendage, an argument strengthened if the polarisation was also found to be radial. At present, however, a great many of the observations that have been made have not been received, and those that have been received are as discordant as those obtained in former eclipses, and therefore my account is an imperfect one, because I have not had an opportunity of discussing all these observations. Indeed, if I had, I should hesitate to give an opinion on the subject. When Mr. Carrington saw that small corona in 1851, and Mr. Gillis saw that small corona in 1858, neither of them traced any polarisation whatever; but when M. Liais saw that large corona in 1868, which was invisible to Mr. Gillis, he in his turn saw an immense amount of polarisation, which led him to believe that the corona was solar, the whole of it, rays and everything included, and that we had an indication of a solar atmosphere two or three times higher than the diameter of the sun; that is, an atmosphere two or three millions of miles in height. This observation is not in accordance with the general conclusions from the drawings I have shown you; and let me add that the assumption of reflection at the sun is not without its difficulties, and that we have not yet traced reflected sunlight, even when the strongest polariscopic effects have been observed.

### III.—AIRY'S AND MÄDLER'S CONCLUSIONS AS THE RESULTS OF THE PRE-SPECTROSCOPIC OBSERVATIONS

Before passing to the spectroscopic observations, I will state the conclusions at which the Astronomer Royal and M. Mädler arrived after the observations of 1860 had been gathered together.

The Astronomer Royal, in a lecture delivered before the British Association at Manchester in 1861, stated that the assumption of an atmosphere extending to the moon explained the observation of Plantamour, which could, he thought, be explained

* Lecture delivered at the Royal Institution, Friday, March 17, 1871.

in no other way, and he held also that the polarisation experiments seemed to show the same thing. The Astronomer Royal was content to find the reflection, which so many now insist must be at the sun, taking place somewhere between the earth and moon.

M. Mädler's verdict is in the same direction, and though he does not perhaps express so decided an opinion, he maintains that the atmosphere plays a principal part in the phenomenon; and after detailing experiments to show this, he remarks of the solar and atmospheric portions, "Both cover each other and unite in one phenomenon, so that the corona is a mixed phenomenon."

I shall shortly show you that the spectroscope, leaving the telescope out of consideration, has taught us that this is true; though I shall not be able to show you that it is the whole truth; we are not yet in a position to do that. Mädler concludes his observations by remarking:—"We cannot share the doubts of those who are afraid to surround the sun with too many envelopes; neither do we find anything unnatural in the statement that the sun has as many atmospheres as Saturn has rings; but we gladly admit that we cannot yet say anything positive. We have here a large field of probabilities, and the decision may yet be distant."

We can speak with more certainty now!

### IV.—Spectroscopic Observations

*a.—Spectrum of the Corona first observed by Tennant, Pogson, and Rayet*

We now come to the consideration of those observations in which we are aided by a most powerful and our most recent ally, the spectroscope, first used on the eclipsed sun, as you know, in the eclipse of 1868. You all know that in that year the question of the nature of red flames was for ever settled by M. Janssen, Major Tennant, Captain Herschel, and others, who observed that eclipse in the most admirable manner; but we have nothing to do with the red flames now, we have to do with something outside them.

Now, most of you are under the impression, and it was mine until the day before yesterday, that the only thing we learnt about the corona in the eclipse of 1868, was that its spectrum was a continuous one; and I need not tell anyone in this theatre that the assertion that it was continuous was one that was extremely embarrassing, and implied that we had something non-gaseous outside the red flames, which seemed very improbable to those who know anything about the subject. But some of you will no doubt remember that, besides Major Tennant, who made this observation, we had a French observer, M. Rayet, who gave us a diagram of the spectrum of one of the prominences, and Mr. Pogson, who has now been for some time in India, and is a well-known observer, who gave us, nominally as the spectrum of a prominence, a spectrum with some curious variations from M. Rayet's diagram.

I exhibit a copy of M. Rayet's diagram of the spectrum of a prominence, as he called it. At the bottom is what he considered as the spectrum of the lower portion of the prominence; while in the higher portion, where we get fewer lines, as he considered, is the spectrum of the higher portion of the prominence. The spectrum of the lower portion contains the lines B, D, E, and F, and some other lines, in all nine, while the spectrum of the upper part of the prominence, as he thought it, only contains three lines. It was at first difficult to account for these observations. In the first place, one could not understand the line B being given, because I soon found that the line B was not seen as a bright line in the chromosphere spectrum; it was clearly the line C that was intended. Hence doubt was thrown on the other lines; it seemed as if M. Rayet was wrong about his elongated lines D, E, and F, and probably meant C near D and F. And so it was explained—I am ashamed to say by myself—that there was no particular meaning in these elongated lines, except that the spectrum of the prominence some distance away from the sun was simpler than it was nearer the sun, as happens in all prominences, as we may now determine any day we choose to look at the sun by means of the spectroscope.

Now let us hear Mr. Pogson. He gave a diagram showing five lines in the spectrum of what he thought a prominence, and he writes:—"A faint light was seen (in the spectroscope), scarcely coloured, and certainly free from either dark or bright lines. While wondering at the dreary blank before me, and feeling intensely disappointed, some bright lines came gradually into view, reached a pretty considerable maximum brilliancy, and again faded away. Five of these lines were visible, but two decidedly superior to the rest. . . . . The readings of the two brightest were secured. It struck me as strange that these brightest lines should appear at a part of the spectrum not corresponding to any very conspicuous dark lines in the solar spectrum. . . . . [These lines are a little less refrangible than E.] The third line seen in order of brilliancy must have been either coincident with, or very near the place of the sodium line D, but it was much fainter than the two measured, while the fourth and fifth lines were extremely faint." [They were very faint and DOUBLED, and near F. I have seen F give way to a double line in our hydrogen experiments, though I am not prepared to say this is an explanation of Mr. Pogson's observations.]

The fact that we have here the first observations of the spectrum of the sun's corona is one beyond all doubt; and why M. Rayet and Mr. Pogson thought they were observing prominences when they were observing above them, is explained by a remark made by Captain Tupman, of the Royal Marine Artillery, who acted as jackal to Prof. Harkness, and picked out the brighter spots of the corona for his observation. Prof. Harkness observing the prominence bright lines, said to Captain Tupman, "You have turned the telescope on to a prominence; I want the corona." "No," said Captain Tupman, "I am giving you the corona as well as I can." It was certainly the corona in both cases. Here you see, dimly and darkly, the first outcome of the spectroscope on the nature of the corona; a record as fairly written as anything at the sun can write it; and I am more anxious to lay stress on these observations, since they have lain fallow for two years, and show the importance of observations, not only in extending our knowledge, but in explaining prior observations; and it is an additional reason for never rejecting an observation. What was, however, dim and dark in 1868, shone out brightly in 1869, thanks to the skill of the American observers of the eclipse of that year.

*b.—Laboratory Experiments bearing on these Observations*

But before I proceed to refer to the admirable observations made in America during this eclipse, I wish to introduce you to some work which was commenced in 1868, and has been done quite independently of eclipses. In a lecture which I delivered here about two years ago, I described to you some of the facts observed by the spectroscope in the bright-line region which had been spectroscopically determined to exist all round the sun, and which, as in it all the various coloured effects are seen in total eclipses, I had named the Chromosphere. It was clear that by the new method of observing this without any eclipse, by partially killing, so to speak, the atmospheric light, we got a percentage only of the phenomenon, as the atmospheric light could only be killed by an amount of dispersion which enfeebled and shortened the chromospheric lines; so that although we could say that an envelope of some 5,000 or 6,000 miles in height existed round the sun, we could not fix this as a maximum limit. Further, when we examined the spectrum of this envelope we got long lines and short lines; and I told how the short lines indicated a low stratum, and how a long line indicated a higher one. To explain this, I will show you an observation made long before the new method was thought of. Even before that time we had abundant evidence of such strata, if we could not determine their nature: we had distinct evidence either of one thing *thinning* out, and then another, or that various substances were situated at different levels, under different conditions; on the first hypothesis, at the extreme outside of the chromosphere the last thing would thin out, and then there would be an end of all things as respects the sun.

I will show you a drawing made by Prof. Schmidt of the eclipse of 1851. I do not wish to call your attention to the strange shape of the large prominence, but to the fact, that as the moon passed over this region we get a thin red band, first along the edge of the dark moon, and after the moon had passed over still further, we see this red layer, *suspended as it were in the chromosphere*, with a white layer below it. This is the explanation of the long and short lines visible in the spectrum of the chromosphere; in the red layer we have hydrogen almost alone; below, its red light was conquered by other light with bright lines in all parts of the spectrum, and we get white light.

Lord Lindsay tells me he has a distinct indication, written by the sun himself, that in one particular part of the chromosphere, as recorded photographically in Spain, there were three such layers. And over and over again we find recorded white light close to the sun, then red alone, or red mixed with yellow, then violet, and lastly green. And M. Mädler remarks on this very admirably, "The violet band is the link between the prominences and the corona."

Before going further, I will show you the difference in the appearance of what we may term hot hydrogen and cold hydrogen, that is, hydrogen which we drive into different degrees of incandescence by means of the spark. After Dr. Frankland and myself were able to determine that the pressure in these solar regions was small, we came to the conclusion that outside the hot hydrogen there must be some cooler hydrogen, in order that the phenomena we observed, both in the laboratory and in the observatory, should agree.

I have in this tube hydrogen at a certain pressure, and here we have a coil which will enable us to send a spark through it; you see we get a certain amount of redness in that tube, and if you look on one side or above you will see a sort of bluish-greenish light. Now that redness represents the condition of the hydrogen in the region of the sun where Dr. Schmidt gave us that extremely thin red ring, and the combination of the blue and red would give you something very like violet.

But here I have hydrogen under a different condition. In the tube its rareness is not excessive; but in this globe, of which I am about to speak, you have the nearest approach to a vacuum ever obtained through which a spark will pass; and I beg to call your attention to what will now happen. This globe contains the same chemical element prepared at the same time as the chemical element you have in the tube, but you see that, so far as colour goes, we have something perfectly different in this case. Now we send the spark through it. I would beg Prof. Tyndall, if he will be good enough, to observe the spectrum of this hydrogen in this globe. [Prof. Tyndall did so.] You will see that there is one line? [*Prof. Tyndall:* Yes.] And a continuous spectrum? [*Prof. Tyndall:* And a continuous spectrum.] Cool hydrogen gives us only the bright line F, plus a continuous spectrum, and many of you will know the extreme importance of that observation. It accounts for the F line being observed without the C line in 1868 and last year, and also for he continuous spectrum observed in the Indian eclipse.

### *c.—The American Eclipse*

When we come from the Indian to the American eclipse with the considerations to which I have drawn your attention, namely, the existence of these different layers due to the different elements and conditions of the same element thinning out, we shall see the extreme importance of the American observations, for they establish the fact that outside the hydrogen layer there was a layer giving only a line in the green, the line which Rayet and Pogson had observed associated with the hydrogen spectrum and the spectrum of the yellow substance. Here obviously we have, I think, merely an indication of another substance thinning out, in spite of the extraordinary suggestion which was put forward that the corona was nothing but a *permanent solar aurora.*

I need hardly tell you that the idea of a permanent aurora anywhere was startling, and that of a permanent solar aurora more startling still; but what I claim is, that during last year's observations we made this very startling idea into a most beautiful fact, namely, that this outer layer of the chromosphere is in all probability nothing more nor less than an indication of an element lighter than hydrogen, although this is not yet absolutely established, for the line is coincident with one of the lines in the spec rum of iron.

### *d.—The layers increase very rapidly in Density. Reproduction of the Coloured Phenomena*

Dr. Frankland and myself were early drawn to consider the solar nature of the large coronas, to which I have called your attention, as extremely questionable, even on the supposition of cool hydrogen, because we did not see how, with its temperature and pressure, it could extend very far: and an experiment which I have to make here will probably make that clearer.

We have in these glass vessels hydrogen a little more brilliant now the spark passes through it than that you saw in the globe, because I have been compelled to mix with it a certain amount of mercury vapour. Below, we have at the present moment sodium vapour being generated from metallic sodium in one tube, and mercury vapour in the other. I hope, if the experiment succeeds, you will see that a good many of the coloured phenomena seen in the chromosphere during eclipses may be easily reproduced by such experiments as this; and not only the coloured phenomena but *the increase of brilliancy* accompanied by changes of colour recorded. You can now all see the yellow tinge at the bottom of one tube, and the green tinge at the bottom of the other; and if there were time to continue this experiment by increasing the density of the vapours now associated with the hydrogen, I could make the bottom portion of each tube where the vapours are densest shine out almost like the sun, while the cool hydrogen at the top would remain not more brilliant than it is at present. We should have as it were a section of the chromosphere.

### V.—Conclusion

I will proceed now, if you will allow me, to some of the general results obtained during the last eclipse.

I think that, although the work has been very unfortunately interrupted, still the result has been most satisfactory. By putting together observations here and observations there, I consider our knowledge of the sun is enormously greater than it was a few months ago. For instance, we are enabled to understand the long-neglected observation of Rayet, and the equally long-neglected observation of Pogson; and we know that outside the hydrogen there is, in all probability, a new element existing in a state of almost infinite tenuity. And we are sure of the existence of cool hydrogen above the hot hydrogen, a fact which seemed to be negatived by the eclipse of 1869.

I think if we had merely determined that there was this cool hydrogen, all our labour would not have been in vain, as it shows the rapid reduction of temperature. But there is more behind. I told you that M. Mädler, in summing up the observations made up to 1860, came to the conclusion that part of the corona was certainly solar, and that whether the outer portions were or were not solar, was a matter of doubt. I do not say that we have settled that absolutely, but we have firm evidence that *some* of the light of the corona is due to reflexion between the earth and the moon. The outer corona was observed to have a rosy tinge over the prominences, and the spectrum of the prominences was detected many minutes above them, as well as on the dark moon. It could not have got this colour *at the sun*, for its intrinsic colour is green, and the red light of the hydrogen supplied at the sun is abolished altogether, is absorbed, and can only reach the corona *at the sun*, so to speak, as dark light.

It is a great fact that we are sure, as far as observation can make us sure, that there is a glare round the hydrogen which gives us the spectrum of hot hydrogen on the corona, *where we know that hot hydrogen does not exist.* Assume the hot hydrogen which gives us the red light to be only two minutes high, the spectroscope has picked it up eight minutes from the sun! The region of cool hydrogen is exaggerated in the same way. We get it where there is no indication of the cool hydrogen existing. And then with regard to the element which gives us the line of the green, we get that twenty minutes or twenty-five minutes away from the sun. Well, no man who knows anything about the matter will affirm that it is certain that the element exists at that distance from the sun.

Therefore I think we have absolutely established the fact that as the sun—the uneclipsed sun—gives us a glare round it, so each layer of the chromosphere gives us a glare round it. That is exactly what was to be expected, and that it is true is proved by the observation—a most important observation made in Spain—that the air, the cloud, ever between us and the dark moon, gives us the same spectrum that we get from the prominences themselves.

Given, however, the layers and elements in the chromosphere extended as far as you will, and apparently increased or not by reflection *not at the sun*, we have still to account for rays, rifts, and the like. If anyone will explain either Mr. Brothers's photograph or Mr. Gilman's picture of the eclipse of 1869, containing those dark bands starting from the moon and fading away into space, and the bright variously-coloured rays between them, on any solar theory, he will render great service to science. But in the meantime I must fall back upon M. Mädler's opinion of 1860, with the addition to it that I have stated that we have found, at all events, that *some* of the doubtful light is non-solar; we have turned the opinion into a fact.

Bear in mind that close to the sun you have a white layer composed of vapours of many substances, including all the outer ones; outside this is a yellow region; above that a region of hydrogen, incandescent and red at the base, cooler, and therefore blue, higher up, the red and blue commingling and giving us violet; and then another element thinning out and giving us green. Take these colours in connection with those which are thrown on our landscapes or on the sea during eclipses, each region being lit up in turns with varying, more or less monochromatic light, and that light of the very colour composing the various layers, each layer being, as I have shown, so much brighter than the outer ones that its light predominates over them. Is it too much to suggest to those who may be anxious to attempt to elucidate this subject, that probably if they would consider all the conditions of the problem presented by that great screen, the moon, allowing each of these layers by turn to throw its light earthwards, the inequalities of the edge of the *globular* moon allowing here light to pass from a richer region, here stopping light from even the dimmer ones, they would be able to explain the rays, their colours, variations, apparent twistings, and change of side? I do not hesitate to ask this question, because it is a difficult one to answer, since the whole question is one of enormous difficulty. But difficult though it be, I trust I have shown you that we are on the right track, and that in spite of our bad weather, the observations made by the English and American Government Eclipse Expedition of 1870 have largely increased our knowledge.

With increase of knowledge generally comes a necessity for changing the nomenclature belonging to a time when it was imperfect. The researches to which I have drawn your attention form no exception to this rule. A few years ago our science was satisfied with the terms prominences, sierra, and corona, to represent the phenomena I have brought before you, the nature of both being absolutely unknown, as is indicated by the fact that the term *sierra* was employed, and aptly so, when it was imagined the prominences might be solar mountains! We now know many of the constituent materials of these strange things; we know that we are dealing with the exterior portion of the solar atmosphere, and a large knowledge of solar meteorology is already acquired, which shows us the whole mechanism of these prominences. But we also know that part of the corona is not at the sun at all. Hence the terms *leucosphere* and *halo* have been suggested to designate in the one case the regions where the general radiation, owing to a reduced pressure and temperature, is no longer subordinate to the selective radiation, and in the other, that part of the corona which is non-solar. Neither of these terms is apt, nor is either necessary. All purposes will be served if the term corona be retained as a name for the exterior region, including the rays, rifts, and the like, about which doubt still exists, though it is now *proved* that some part is *non-solar*, while for the undoubted solar portion the term Chromosphere—the bright-line region—as it was defined in this theatre now two years ago, exactly expresses its characteristic features, and differentiates it from the photosphere and the associated portion of the solar atmosphere.

Here my discourse would end, if it were not incumbent on me to state how grateful I feel to Her Majesty's Government for giving us the opportunity of going to the eclipse; to place on record the pleasure we all felt in being so closely associated in our work with the distinguished American astronomers who from first to last aided us greatly; and to express our great gratitude to all sorts of new friends whom we found wherever we went, and who welcomed us as if they had known us from our childhood.

J. NORMAN LOCKYER

## *ON THE DISTRIBUTION OF TEMPERATURE IN THE NORTH ATLANTIC* *

AT the request of the Council of the Scottish Meteorological Society, I beg to bring before you a sketch of the more recent results of investigations into the causes of the abnormal climate of the surface of a great portion of the North Atlantic Ocean, and of the lands which form its north-eastern borders; and especially the results of the deep-sea exploring expeditions of the last three years, in which I have taken a part, so far as they bear upon this point.

In a recent valuable report on the Gulf Stream in the "Geographische Mittheilungen," of last year, Dr. Petermann severely and, I think, too justly, reflected upon us students of ocean temperatures for giving ourselves up to wild and gratuitous speculation. I wish, if possible, on the present occasion, to avoid all risk of such impeachment, by limiting our inquiry rigidly for the few minutes I have at my disposal to the present condition of our knowledge of facts, and to such deductions from these as may be fairly considered proved.

* Address delivered to the Meteorological Society of Scotland at the General Meeting of the Society, July 5.

Let us then first inquire for a moment what the phenomena are which we are called upon to correlate and to explain. There is no dispute about these facts, and a glance at the chart will at once recall them to your recollection. In the first place, the lines of equal mean annual temperature, instead of showing any tendency to coincide with the parallels of latitude, run up into the North Atlantic and into the North Sea, in the form of a series of long loops. This diversion of the isothermal lines from their normal direction is admittedly caused by surface ocean-currents conveying the warm tropical water towards the polar regions, whence there is a constant counter-flow of cold water beneath to supply its place. This phenomenon is not confined to the North Atlantic. A corresponding series of loops, though not so well defined, passes southwards along the east coast of South America, and a very marked series occupies the north-eastern angle of the Pacific, off the Aleutian Islands and the coast of California. The temperature of the land is not affected directly by the temperature of the sea in its immediate neighbourhood, but by the temperature of the prevailing wind, which is determined by that of the sea. Setting aside the still more important point of the equalisation of summer and winter temperature, the mean annual temperature of Bergen, lat. 60° 24′ N., subject to the ameliorating influence of the south-west wind blowing over the temperate water of the North Atlantic, is 6·7° C. while that of Tobolsk, lat. 58° 13′, is − 2·4° C.

But the temperature of the North Atlantic is not only raised greatly above that of places on the same parallel of latitude having a continental climate by this interchange of tropical and polar water, but it is greatly higher than that of places apparently similarly circumstanced as to a general interchange of water in the Southern Hemisphere. Thus, the mean annual temperature of the Faroe Islands, lat. 62° 2′ N. is 7·1° C. nearly equal to that of the Falkland Islands, lat. 52° S., which is 8·2° C., and the temperature of Dublin, lat. 53° 21′ N., is 9·6° C., while that of Port Famine, lat. 53° 8′ S., is 5·3° C. Again the high temperature of the North Atlantic is not equally distributed, but is very marked in its special determination to the north-east coasts. Thus, the mean annual temperature of Halifax, lat. 44° 39′, is 6·2° C., while that of Dublin, lat. 53° 21′ is 9·6 C., and the temperature of Boston (Mass.) lat. 42° 21′ is exactly the same as that of Dublin.

We thus arrive at the well-known general result, that the temperature of the sea bathing the north-east shores of the North Atlantic is greatly raised above its normal point by currents involving an interchange of tropical and polar water; and that the lands bordering on the North Atlantic participate in this amelioration of climate by the heat imparted by the water to their prevailing winds.

We shall now examine this distribution of ocean temperature a little more minutely. During the last many years a prodigious amount of data have been accumulating with reference to the detailed distribution of heat on the surface of the North Atlantic basin, and last year M. Petermann, of Gotha, published in his "Geographische Mittheilungen" a series of invaluable temperature charts embodying the results of the reduction of upwards of 100,000 observations derived mainly from the following sources:—

1st. From the wind and current charts of Lieut. Maury, embodying about 30,000 distinct temperature observations.

2nd. From 50,000 observations made by Dutch sea captains and published by the Government of the Netherlands.

3rd. From the journal of the Cunard steamers between Liverpool and New York, and of the steamers of the Montreal Company between Glasgow and Belleisle.

4th. From the data collected by our excellent secretary, Mr. Buchan, with regard to the temperature of the coast of Scotland.

5th. From the publications of the Norwegian Institute on sea temperatures between Norway, Scotland, and Iceland.

6th. From the data furnished by the Danish Rear-admiral Irminger on sea temperatures between Denmark and the Danish settlements in Greenland.

7th. From the observations made by Lord Dufferin on board his yacht *Foam* between Scotland, Iceland, Spitzbergen, and Norway.

And finally from the recent observations collected by the English, German, Swedish, and Russian expeditions to the Arctic Regions and towards the North Pole.

Dr. Petermann has devoted the special attention of a great part of his life to this question, and the accuracy of his results in every detail is beyond the shadow of a doubt. Every curve of equal temperature, whether for the summer, for the winter, or for the whole year, instantly declares itself as one of a system of curves which are referred to the Strait of Florida as the source of heat, and the warm water may be traced (and this is not begging the question, for the temperature is got by dipping the thermometer in the water), in a continuous stream, indicated where its movement can no longer be observed by its form, fanning out from the neighbourhood of the Strait across the Atlantic, skirting the coasts of France, Britain, and Scandinavia, rounding the North Cape, and passing the White Sea and the Sea of Kari, bathing the western shores of Novaja Semla and Spitzbergen, and finally coursing round the coast of Siberia, a trace of it still remaining to try to find its way through the narrow and shallow Behring's Strait into the North Pacific. Now it seems to me that if we had these observations alone, which are merely detailed and careful corroborations of many previous ones, and could depend upon them, without even having any clue to their *rationale*, we should be forced to admit that whatever might be the amount and distribution of heat derived from a general oceanic circulation, whether produced by the prevailing winds of the region, by convection, by unequal barometric pressure, by tropical heat, or by arctic cold, there is besides this some other source of heat at the point referred to by these curves sufficiently powerful to mask all the rest, and, broadly speaking, to produce of itself all the perceptible deviations of the isotherms from their normal course.

But we have no difficulty in accounting for this source of heat. As is well-known, about the equator, the north-east and south-east trade winds reduced to meridional directions by the eastward frictional impulse of the earth's rotation, drive before them a magnificent surface current of hot water, the equatorial current, 4,000 miles long and 450 miles broad, at an average rate of thirty miles a-day. This current splits upon Cape St. Roque, and one portion trends southwards to deflect the isotherms of 21° 15·5°, 10°, and 4·5° C. into loops, thus carrying a scrap of comfort towards the Falklands and Cape Hoorn. While the remainder, "having made the circuit of the Gulf of Mexico, issues through the Straits of Florida, clinging in shore round Cape Florida, whence it issues as the Gulf Stream, in a majestic current upwards of 30 miles broad, 2,200 feet deep, with an average velocity of 4 miles an hour, and a temperature of 86° Fahr." (Herschel.)

I need scarcely follow the course of the Gulf Stream in detail, it is generally so well known. After leaving the Strait of Florida, it strikes in a north-easterly direction conformable generally to the easterly impulse given by its excess of diurnal rotation, towards the coast of Northern Europe. About 42° N. a large portion of it, still maintaining the high surface temperature of 24° C., turns eastward and southward, and, eddying round the Sargasso Sea, fuses with the northern edge of the equatorial current, and rejoins the main circulation. The main body, however, moves northwards. Mr. Croll, in a very suggestive paper in the *Philosophical Magazine* on Ocean Currents, estimates the Gulf Stream as equal to a stream of water fifty miles broad and 1,000 feet deep, flowing at a rate of four miles an hour, with a mean temperature of 18° C. I see no reason whatever to believe this calculation to be excessive, and it gives a graphic idea of the forces at work.

The North Atlantic and the Arctic Seas form together a basin closed to the northward, for there is practically no passage for a body of water through Behring's Strait. Into the corner of this basin, as if it were a bath, with a north-easterly direction given to it, as if the supply pipe of the bath were turned so as to give the hot water a definite impulse, this enormous flood is poured day and night, winter and summer; almost appalling in its volume and the continuity of its warmth, and its blueness, and brilliant transparency *in secula seculorum!*

The hot water pours, not entirely from the Strait of Florida, but partly from the Strait and partly in a more diffused current outside the islands, with a decided, though slight, north-easterly impulse on account of its great initial velocity. The North Atlantic is with the Arctic Sea a *cul-de-sac*. When this basin is full—and not till then—overcoming its northern impulse, the water tends southwards in the southern eddy, so that there is a certain tendency for the hot water to accumulate in the northern basin. It is to this tendency, produced by the absence of a free outlet to the Arctic Sea, that I would be inclined to attribute the special excess of the warmth of the north-eastern shores of the North Atlantic.

When ascertaining with the utmost care and with the most trustworthy instruments, by serial soundings, the temperature of the area surveyed by the *Porcupine* in 1869, we found at a depth of 2,435 fathoms in the Bay of Biscay, that down to 50 fathoms the temperature of the sea was greatly affected by direct solar radiation; from 100 to 900 fathoms the temperature gradually fell from 10° C. to 4° C., and from 900 fathoms to 2,435 the fall of temperature was almost imperceptibly gradual from 4° to 2°·5 C.

The comparatively high temperature from 100 fathoms to 900 fathoms I am certainly inclined to attribute to the northern accumulation of the water of the Gulf Stream. The radiant heat derived directly from the sun must of course be regarded as a constant quantity superadded to the original temperature of the water derived from other sources. Taking this into account, the surface temperatures in what we were in the habit of calling the "warm area" coincided precisely with Petermann's curves indicating the northward path of the Gulf Stream.

It is scarcely necessary to say that for every unit of water which enters the basin of the North Atlantic, an equivalent must return. From its low velocity, the Arctic return current or indraught will doubtless tend slightly to a westerly direction, and the higher specific gravity of the cold water may probably even more powerfully lead it into the deepest channels; or possibly the two causes may combine, and in the course of ages the currents may tend to hollow out deep south-westerly grooves. At all events, the main Arctic return currents are very visible on the chart taking that direction, indicated by marked deflections of the isothermal lines. The most marked is the Labrador current, which passes down inside the Gulf Stream along the coasts of Carolina and New Jersey, meeting it in the strange, abrupt "cold wall," dipping under it as it issues from the Gulf, coming to the surface again on the other side, and a portion of it actually passing under the Gulf Stream as a cold counter-current into the deeper part of the Gulf of Mexico.

Fifty or sixty miles out from the west coast of Scotland, I believe the Gulf Stream forms another through a very mitigated "cold wall." In 1868 Dr. Carpenter and I investigated a very remarkable cold indraught into the channel between Shetland and Faroe. In a lecture on deep-sea climates, which was published in NATURE, in July last, I stated my belief that the current was entirely banked up in the Faroe channel by the Gulf Stream passing its gorge.

Since that time I have been led to suspect that a part of the Arctic water oozes down the Scottish coast much mixed, and sufficiently shallow to be affected throughout by solar radiation. About sixty or seventy miles from shore the isothermal lines have a slight but uniform deflection. Within that line types characteristic of the Scandinavian fauna are numerous, and in the course of many years' use of the towing net, I have never met with any of the Gulf Stream pteropods, or of the lovely Polycystinæ and Acanthometrinæ, which absolutely swarm beyond that limit. The differences in mean temperature between the east and west coasts of Scotland, amounting to between 1° and 2° Fahr., is also somewhat less than might have been expected.

There is another point which is worthy of consideration. It is often said that about the latitude 45° N. the Gulf Stream thins out and disappears. The course of a warm current is traced farther on the maps, even to the coast of Norway and the North Cape, but this north-easterly extension is called the Gulf Stream drift, and is supposed to be a surface flow caused by the prevailing S.W. anti-trades. There seem to me to be several arguments against this view. The surface of the sea, at all events between 40° and 55° N., has a mean temperature higher than that of the air, and that could scarcely be the case unless there were a constant supply, independent of the wind, of water from a warmer source; and any question is, to my mind, entirely set at rest by our establishment of the mass of warm water moving to the north-eastward, whose curves of excess of temperature, so far as they have as yet been ascertained, correspond entirely with those of the Gulf Stream.

I cannot at present enter at any length into the very fundamental question which has lately given rise to so much discussion, whether the Gulf Stream is actually the agent in conveying heat to the North Atlantic and ameliorating the climate of its north-eastern shores, or whether these results are not rather produced by a "general oceanic circulation."

As, however, I am frequently quoted by my friend and colleague in much scientific work, Dr. Carpenter, as holding an opinion different from his, and as my present remarks place my views beyond doubt, it may be well to give a reason for my want of faith. Dr. Carpenter's view, if I understand him rightly, is that there is a great general convective circulation in the ocean, on the principle of a hot-water heating apparatus, and that the Gulf

Stream is only a modified and partial cause of this general circulation. Now in the first place, as I have already said, it seems to me that the distribution of warm water in the North Atlantic has been traced to its source, and all the general phenomena of the Gulf Stream, its origin, its course, its extension, and its depth at certain points, have been *proved* by the careful observations of many years, which I see no reason whatever to doubt. The constant impulse of the trade wind drives a broad current of equatorial water against the American coast. A great part of this current is observed to turn northwards through the Strait and round the islands, and to pour an eternal flood of hot water in a certain direction, under known laws, into the closed basin of the North Atlantic, and as a natural consequence the temperature is very considerably raised.

We are undoubtedly most deeply indebted to Dr. Carpenter for the forcible way in which he has brought forward the arguments on the other side; and, after carefully considering everything, I am thoroughly willing, with Sir John Herschel, to cede that "there is no refusing to admit that an oceanic circulation of some sort must arise from mere heat, cold, and evaporation as *veræ causæ;*" and that "henceforward the question of ocean currents will have to be studied under a twofold point of view;" but my strong conviction is that if the sagacious philosopher whose loss we now deplore, had been spared so to study it, he would have only been strengthened in his verdict of 1861 as to the Gulf Stream, that there can be no "possible ground for doubting that it owes its origin entirely to the trade-winds." Dr. Carpenter attributes the general oceanic circulation, of which he regards the Gulf Stream as only a modified case, to tropical heat and evaporation, and arctic cold, possibly aided by differences of barometric pressures; or to convection pure and simple, as illustrated in his experiments before the Royal Institution and the Geographical Society. Now what we expect of Dr. Carpenter before we are called upon to accept to the full his magnificent generalisation, is a calculation and demonstration of the amount of the effect of the causes upon which he depends acting under the special circumstances. We must remember that heat is received by the ocean at the surface only, and that owing to cold indraughts all over the globe, so far as we know the temperature falls the deeper we go; that all our observations tend to show that the temperature of the sea is only influenced by direct solar radiation to any amount to the depth of fifty fathoms, so that all currents depending upon difference between equatorial and polar temperatures must be produced and propagated in a film of water about the depth of the height of St. Paul's and 6,000 miles long. The black line bounding that chart represents pretty nearly the depth of the ocean, and even where the whole of the water supposed to be involved in the movement, it would be difficult to imagine a perceptible current to be produced in so thin and wide a sheet by such feeble cause. It would be impossible to indicate by the finest hair line the tenuity of the film which is actually affected by the direct rays of the sun. How differences in barometric pressure can produce constant currents I do not see. Rapid fluctuations in pressure in places within a short distance of one another will doubtless produce readjustment by a wave motion; but constant differences of pressure will simply produce constant differences of level and no currents. Varying pressures at very distant points cannot possibly produce a constant current. I freely admit that I am quite incapable of undertaking the investigations which might lead to the estimation of the relative or actual importance of these causes of currents. I have several times put the question to specialists in such physical inquiries, but they have always said that it was a matter of the greatest difficulty, but that their impression was that the effects would be infinitesimal.

I fear then that, in opposition to the views of my distinguished colleague, I must repeat that I have seen as yet no reason to modify the opinion which I have consistently held, that the remarkable conditions of climate on the coasts of Northern Europe are due in a broad sense solely to the Gulf Stream; that is to say, that while it would be madness to deny that in a great body of water at different temperatures, under varying barometric pressures, and subject to the surface drift of variable winds, currents of all kinds variable and more or less permanent must be set up, yet the influence of the great current which we call the Gulf Stream, the reflux in fact of the great equatorial current, is so paramount as to reduce all other causes to utter insignificance.

WYVILLE THOMSON

## *PHYSIOLOGY*

### The Mouse's Ear as an Organ of Sensation*

DR. SCHOBL, of Prague, who lately published a remarkable paper on the wing of the bat, has made similar researches on the ear of the white mouse, with very interesting and surprising results (in "Schultze's Archiv," vol. vii. p. 260.) The first thing which struck Dr. Schöbl was the immense and "fabulous" richness of the ear in nerves. Even the bat's wing is but poorly supplied in comparison. The outer ear was carefully divided horizontally through the middle of the cartilage into two laminæ, each of which was found to be equally supplied with nerves, and was then examined by removing the epidermis and the Malpighian layer of the skin. In each of these laminæ were discovered three distinct strata of nerves, which are thus described: The first or lowest stratum lies immediately upon the cartilage; it consists of the largest trunks which enter the ear, 5 to 7 in number, and their next branches, varying from ·074 mm. to ·028 mm. in diameter. The mode of division of these trunks is mainly dichotomous, but they are connected by several different kinds of anastomoses; as, for instance, by decussation of two adjacent trunks, by transverse or oblique connecting branches, by plexuses, by loops, &c.; while branches also perforate the cartilage, and bring the nerves of the two halves of the ear into connection. The general distribution agrees with that of the larger blood-vessels. The second stratum lies immediately over the first, and is connected with it by a multitude of small branches, and by a fine marginal plexus at the outer border of the ear, which may be regarded as common to both. The diameter of its nerves is from ·0185 mm. to ·0098 mm.; it lies immediately under the capillary vascular network of the skin, and has a generally reticulated arrangement, forming plexuses of very various shapes. The third stratum of nerves, developed out of the very finest twigs of the second, lies at the level of the capillary network; it is composed of branches ·0098 mm. to ·0037 mm. in thickness, which (like those of the other strata) contain medullated nerve-fibres. It forms an extremely delicate network, like the second layer, but its finest branches may terminate in two ways. Some of them, each containing two to four medullated fibres, run directly to the hair follicles, and form a nervous ring round the shaft of the hair, terminating below the follicle in a nervous knot. Others, again, consisting of not more than two medullated fibres, bend towards the surface where the fibres lose their double outline, and form, immediately under the Malpighian layer of the skin, a fine terminal network of pale fibres, which is the fourth and ultimate stratum of nervous structures. The terminal "knots" or corpuscles, and the nervous rings, are inseparably connected with hairs and their sebaceous glands, so that through the whole of the external ear no hair can be found without this nervous apparatus, and *vice versâ*. The connection of the hair follicle with the nerve termination is as follows:—Under the bulk of the hair in each follicle is a more or less conical prolongation, composed of distinct nucleated cells, which run vertically downwards, and is enclosed within the limiting membrane of the follicle. The nervous twig which, as has been said, runs to each hair follicle from the third stratum of nerves, makes several turns round the shaft of the hair, and from the ring thus formed two to four nerve-fibres run vertically downwards to the prolongation of the follicle, immediately beneath which they form a knot. These knots are almost always spherical, sometimes oval, and about ·015 mm. in diameter. In each square millimetre of the marginal part of the ear there are about 90 such bodies, and near the base perhaps 20, so that the average number may be 30. Calculating from the average size of the ear of a common mouse, it is then found that there are on the average 3,000 nerve terminations on each of its surfaces, making 6,000 on each ear, or 12,000 altogether. The function of this elaborate arrangement would seem to be, like that in the wing of the bat, to supply by means of a very refined sense of touch, the want of vision to these subterranean animals.

## *SCIENTIFIC SERIALS*

PART ii. of the *Zeitschrift für Ethnologie* contains No. 6 of Dr. Hartmann's "Studies of the History of Domestic Animals," on the yak or grunting ox (*Bos grunniens*) living wild at immense altitudes in the mountains of Central Asia north of the Himalaya, and largely used in a domesticated state in Mongolia and

* From the "Quarterly Journal of Microscopical Science" for July.

Siberia. The cross between the yak and common ox has the advantage of thriving in a milder climate than that of the mountainous region of the yak. Dr. Hartmann also continues (No. 2) his summary of the available information as to "Lake Dwellings," here discussing their cultivation and preparation of grain and other vegetables. He reaffirms the usual conclusion that the cultivated plants of the lake dwellers of Central Europe indicate connection with the Mediterranean and even Africa. Perhaps the most remarkable point in the paper is the comparison of their large earthen jars for store corn, and their stone grain rubbers for mealing it, with similar jars and grain rubbers in modern Africa.—Prof. Meinicke's "Remarks on Wallace's Views as to the Population of the Indian Islands" are written in strong opposition to the English naturalist's theory as to the ethnological relations of Malays, Polynesians, and Papuans. With regard to Mr. Wallace's argument from contrast of the Malay character with the Papuan as proving difference of race, Prof. Meinicke argues that the Malay's courtesy and reserve may not be a race-character at all, but an effect of conversion to Mohammedanism; while revenge and bloodthirstiness belong to some Papuans as much as to Malays. In opposition to Mr. Wallace's view of Malays and Papuans being two distinct races, and of the Moluccas being largely populated by their intermixture, Prof. Meinicke claims the natives of the Moluccas as intermediate varieties forming a link of connection between the extreme Malay and Papuan types. As to the relation between Malays and Polynesians, Prof. Meinicke maintains the old and generally received view of an ethnological connection between them.—It is good evidence of the activity with which the science of man is now being pursued that Dr. W. Koner's useful bibliography of Anthropology, Ethnology, and Prehistoric Archæology for 1869-70 extends to twenty pages of the journal.—Dr. Bastian's review of Darwin's "Descent of Man," expressing high admiration for its hypothetically-arranged evidence as a contribution to science, protests against the exaggeration of Darwinism, or rather, the return to Lamarckism prevalent among too impetuous followers of the development theory.

In the July number of the *Geological Magazine* (No. 85) the editor, Mr. Woodward, publishes a most interesting summary of the evidence extant as to the existence of limbs in the Trilobites, with a discussion of the significance of a remarkable specimen of *Asaphus*, lately described by Mr. Billings in the *Quarterly Journal of the Geological Society*. From a personal examination of the specimen, Prof. Dana was led to declare that the objects described by Mr. Billings as legs were merely calcified portions of the ventral integument destined to support branchial appendages. Mr. Woodward shows, and we think satisfactorily, that Prof. Dana is in error here. This valuable paper is illustrated with a plate contrasting the lower surface of Mr. Billings's Trilobite with that of the Norway lobster.—Mr. Hull contributes some observations on the general relations of the drift deposits of Ireland to those of Great Britain, in which the author confirms and extends the views adopted by Prof. Harkness as to the correlation of the Irish drift deposits with those of Britain, and the accordance of the whole with the principles laid down by Mr. Searles V. Wood, jun. A tabular statement of the phenomena of the three stages of the drift period in Britain concludes this paper.—From Mr. G. A. Lebour we have a note on the submergence of Is in western Brittany, in which, after referring to a Breton tradition that a town named Is was submerged in the Bay of Douarneney some fifteen hundred years ago, he adduces certain evidence to show that a gradual depression is taking place along this coast. He notices a submerged forest in the small Bay de la Forêt.—Mr. Mackintosh continues his paper on the drifts of the west and south borders of the Lake district; Mr. A. G. Cameron describes the recently-discovered caverns at Stainton in Furness; and Mr. J. E. Taylor discusses the relation of the Red to the Norwich Crags.

The first part of the fourteenth volume of the *Atti della Società Italiana di Scienze Naturali*, published in April of the present year, contains only three papers, more than one-third of its pages being occupied by the annual report, list of members, &c. The papers are a description of a new species of Dalmatian shell, by MM. A. and G. B. Villa, to which the authors give the name of *Clausilia de Cattaniæ*; a long memoir on rennet and caseification, by M. C. Besana, and a short notice by Dr. C. Marinoni, of some new prehistoric remains collected in Lombardy.

## SOCIETIES AND ACADEMIES

### London

Geologists' Association, July 7.—Prof. Morris, vice-president, in the chair. Mr. J. R. Pattison read a paper "On the Upper Limits of the Devonian System." The author did not wish to reopen the controversy which had taken place between the late Prof. Jukes and the supporters of the classification of the older geologists, but simply to lay before the Association a few facts as a prelude to a more complete paper which he hoped to bring forward during the next session. Mr. Pattison referred at some length to the fauna of the continental Devonian rocks, and strongly opposed the view recently put forward, that the Petherwin series is Lower Devonian and not Upper as generally supposed. He quite agreed with the older geologists in their classification, and concluded by recommending the sections exposed in North Devon to the attention of young geologists.—After some remarks by Prof. Tennant, Mr. Henry Woodward, and Mr Lobley, Prof. Morris described the distribution of the Devonian rocks throughout Europe, and remarked on the absence of vertebrate remains in the Devonian rocks of the South of England, in which corals and brachiopods abound, and the abundance of vertebrate remains in the Devonians or Old Red sandstones of Scotland, in which neither corals nor brachiopods have been detected. In the province of Oranburg, in Russia, however, the Devonian rocks contain both a vertebrate and a molluscan fauna.—A note "On a New Section of the Upper Bed of the London Clay," by Mr. Caleb Evans, drew the attention of the Association to an interesting exposure of a very fossiliferous bed of the London clay at Child's Hill, Hampstead. From an inconsiderable excavation at this place, Mr. Evans had collected in a short time twenty-three species, chiefly gasteropoda, in a fine state of preservation. This bed Mr. Evans considers to be the uppermost bed of the London clay, and immediately underlying the Bagshot sands, which form the summit of Hampstead Heath.

### Mauritius

Meteorological Society, April 28. — The Honorable Colville Barclay, vice-president, in the chair.—The following letters and publications were laid upon the table:—1. A letter from Mr. James Duncan, Government Surveyor, forwarding a copy of observations taken at the Survey Camp, Vacoas, during the month of March last, at about 1,850 feet above the sea-level. 2. A letter from Mr. G. Jenner, Rodrigues, forwarding observations taken there in December, January, February, and March last. 3 From Mr. F. Timperley, Pamplemousses, giving a description of a meteor seen by him on the 22nd March. 4. Queensland Observations for October, November, and December 1870, by Mr. Edmund MacDonnell. 5. Singapore Observations for January 1871, by Dr. H L. Randell.—"On the Converging of the Wind in Cyclones." The Secretary read the following letter addressed to him on the above subject by Captain Douglas Wales, Harbour Master:—"Some remarks of yours respecting the uncertainty of the real position of the centre of a cyclone set me thinking, and I send you a few ideas on the subject, which, as a sailor, I think worthy the serious attention of seamen, and the correctness of which they may put to the test of experience, whenever they have opportunities of doing so. Allow me to premise that I have no intention of dogmatising. I believe our knowledge of the cause of these fearful tempests, of their origin, their progress in this or that direction, their rate of progression, their recurving, the reasons of those recurvings, and their ultimate dispersion, to be still in its infancy. No doubt, the knowledge already acquired has saved many a good ship from becoming entangled in these storms, especially ships approaching them on their equatorial sides; but at the same time it must be admitted that more than one intelligent seaman, who thought himself well up in the subject, has actually run into the very centre of a cyclone, when, by all known rules, he ought to have been certain of avoiding it. There must be some reason for such an error, and it is that reason that I have been seeking for, and which, I trust, I have to some extent discovered. I send you a diagram on a large scale, which will explain my views more clearly than any written description. I assume that within a diameter of 40, 50, 60, 70, or 80 miles, a true circular storm of terrific violence must be found in every so called hurricane, and that to a considerable distance outside and around this central and circular storm winds are to be found gradually decreasing in force from 11, near the outer edge of the central storm, to 7 and 6, at the outer edge of the bad weather, but which, instead of blowing in ever enlarging circles farther and farther out from one common centre, are always converging to that centre, and on all sides gradually increasing, until, at a certain distance from the central calm, they acquire the force of a hurricane (12), and thence inwards blow with great violence in what, in all probability, is as nearly as may be a circle. It is these converging lines of wind that are, I think, likely to lead men into error as to the position of the centre of the storm. In the remarks I make I shall, to prevent confusion, confine myself to cyclones south of the equator, every one acquainted with the cyclonic theory knowing that the inverse of rules for the guidance of seamen in the southern hemisphere will be the rules for their guidance in the northern hemisphere. Let us suppose that a ship bound to Europe arrives at the point marked * in the outer converging curve traced on my diagram, the wind being N.E. with force 7, that is, double reefs and jib—barometer falling, sky overcast, confused swell, and, in short, every appearance of bad weather—lat. 12° S., long. 70° E.—What ought her commander to do? 'Heave to on the port tack,' says one, 'and wait for the weather to clear.' 'Run to the S.W.,' says another, 'and make use of the storm.' Being a pushing fellow, he makes up his mind to run, and, truth to say, there are as many reasons for approving that proceeding as for finding fault with it. If he succeeds in making use of the hurricane, he is considered a smart fellow; if he runs into it, and is dismasted or worse, 'rash,' 'beadstrong,' 'ignorant,' &c., are the best terms he can look for; and yet he might as easily have been wrong in heaving to as in running. The wind being N.E., he infers that the centre bears N.W. He considers that the barometer and weather indicate that he is on the S.E. edge of a cyclone—the N.E. wind upon which he is running forming part of a circular storm, and that necessarily the centre is N.W. of him. Considering, further, that in that lat. and long. the storm is probably travelling W.S.W., he thinks that if he runs S.W. he will be diverging from it, and, that by making use of the storm he will get fine runs perhaps for days to come. But if the N.E. wind be only converging towards the fearful storm raging near the centre, that centre, in the first place, bears W. by N. ½ N., instead of N.W., so that the vessel, by steering S.W. is not diverging from the centre, as was supposed, but is really drawing nearer to it. In due time the weather gets worse from this very cause; the wind veers more to the eastward, the barometer continues to fall, and the captain begins to doubt whether the storm may not after all be progressing more to the southward than he supposed; whether, indeed, it may not, although so far to the eastward, be actually recurving, and he naturally becomes anxious and uncertain what to do. If he decides on running at all risks, he finds the wind still drawing at first more and more easterly, and then more and more southerly, always increasing in fury, and the sea becoming more and more heavy and tumultuous. But run he must now, and he must run dead before it, and being on what I have supposed a line of wind converging to a centre, he finishes by getting into the real hurricane, and loss and disaster are imminent. He may, however, if his ship be tight and staunch and runs well, get round to the N.W. side of the storm, and so get clear, probably with loss of spars and sail; but he has clearly run into what he was running to avoid, because he was under the impression that winds within the influence of a cyclone, although far from its centre, blew in circles round that centre, the wind everywhere clearly indicating the exact, or nearly exact, position of that centre. These opinions I submit with very great diffidence for the consideration of seamen and cyclonists. I am not going to attempt the setting up of any dogmatic theory of my own, but I am inclined to think this theory of converging winds will probably account for the manner in which many vessels have become entangled in hurricanes when seeking to avoid them according to cyclonic rules. Like all other theories on this very important subject, it requires very careful consideration; but there can be no possible risk in deducing from it the rule that vessels on approaching what the barometer, the state of the weather, and the force of the wind, clearly indicate as the dangerous side of a cyclone, should, in seeking to avoid it, keep the wind quite four points on the port quarter. With the wind thus free, a fast ship would run with great rapidity through the water, and, unless the storm were advancing on her in a direct line, would be always increasing her distance from its centre, and getting into finer weather, and, in any case, would have a very good chance of running across its track and thus avoiding it. Ships running into cyclones on their equatorial sides are to a very great extent without excuse. There are, however, some exceptional instances; but they are very rare." The chairman, in thanking Captain Wales for his interesting and valuable communication, expressed the hope that the important suggestions it contained would be taken advantage of by seamen, and prove to be serviceable to them in their attempts to avoid the dangerous parts of cyclones. The diagram prepared by Captain Wales fully explained how it might happen that a vessel, by seeking to keep away from the centre of what was considered a circular storm, would be actually running into it. The secretary was glad that the subject had been taken up by a sailor of long experience and of great practical knowledge and skill, and he had no doubt that Captain Wales's remarks would receive the serious attention they merited. In various papers published during the last fifteen years, he (the secretary) had often called attention to the incurving of the wind in cyclones, and to the losses occasioned by acting upon the supposition that the bearing of the centre was at right angles to the direction of the wind; and he believed that it was now beginning to be admitted that the movement of the air in a cyclone was not at all represented by concentric circles, but by a figure similar to that sketched by Captain Wales. The description given by Captain Wales of the way in which vessels might get involved in a cyclone, whilst acting according to accepted rules, applied to many cases which actually occurred. Captain Wales had framed a practical rule based upon observed facts, and it was for seamen to test its value.

### Paris

Academie des Inscriptions et Belles Lettres, July 14. Two seats of *associés libres*, vacant by the death of MM. Prosper Merimée and Deheque, have been filled at the recent sittings. M. Merimée's seat was given to M. de Robert, and M. Deheque's to M. Thomas Henry Martin, director of the Academy at Rennes. This gentleman has written many valuable volumes on interesting points of history; among others, "On the Physical Opinions of the Greeks and Romans." He was one of the few French savants opposed to M. Chasles' famous letters of Newton, and has written a pamphlet on the subject.

Academie des Sciences, July 17.—M. Faye in the chair. A committee was appointed to discuss the respective merits of several candidates for a free associate membership. The committee was composed of MM. Combes and Bertrand for the section of mathematical sciences, MM. Chevreul and Boussingault for the section of physical sciences, MM. Raulin and Bussy for the free members. The chairman of the committee is *de jure* M. Faye. When a report is to be drawn on the respective merits of ordinary members, the committee is composed from the section to which the late member belonged in his lifetime. In the secret committee held after the public sitting, a discussion was raised as to several candidatures, and it was impossible to come to any definite conclusion.—M. Lacaze Duthiers, a professor at the museum, who claims a seat in the section of zoology, read a paper on a new organ of nervous power which he has discovered in certain gasteropods living in water. This organ is placed behind the œsophagus, and at all events its dimensions are very small indeed. The Academy has appointed a committee to report on the prize Bordin, which is to be awarded this year for the best paper on the function of the stomata in the leaves of plants. —On the 3rd of October, 1870, M. Egger proposed the translation of the four books on Optics by Ptolemy, which were translated from Arabic into Latin, and of which two copies exist amongst the MSS. in the National Library. This suggestion was not lost, as the Royal Academy of Turin passed a resolution to raise the funds required for its publication. Other copies of the same Latin translation are also to be found in the Ambrose Library at Milan, and will be used for the purpose. The translation is very difficult, having been unsuccessfully attempted once in Italian, and once in French.—M. Leverrier presented a report on the observation of falling stars, for August 1869. The phenomenon was observed in twenty-seven different stations, viz. Agde, Barcelona, Bordeaux, Chartres, Chebli (Algiers), Genoa, Grenoble, Le Guerche (Cher), Larenore (Basses Pyrenées), Le Mans, Lyons, Marseilles, Mer (Loire et Cher), Metz, Moncalieri, Montpelier, Nice, Orange, Perpignan, Rochefort, Sainte Honorine (Calvados), Toulon, Toulouse, Tremont, Turin, Valencia. Observations were made by competent observers with correct chronometers, and special maps prepared by the Association Scientifique de

France, of which M. Leverrier is now the chairman. The discussion on the observations is a long work which is not yet finished in consequence of the late war. The observations could not be completed in 1870, but the Association Scientifique de France is resuming its labours, and will be ready to make observations by August 1871 on the former principles.—M. Leverrier sent the description of a bolide observed at 10h. 6m. in the afternoon, 1° 30′ higher than α Andromedæ, and exploding in Pegasus. He asks for some observations from the astronomical public.—The same question is put as to a magnificent falling star seven times larger than Jupiter observed by M. Chapelas 11h. 12m. in the afternoon, on the 18th July, from ξ Pegasus to the horizon in the north-west. It must have been seen in England.—At the last sitting we omitted to mention the presentation of some grains of wheat, &c., burned by electricity in a storm, a few years ago and preserved as a great curiosity.—M. Bert, Professor of Physiology at the Museum, formerly a prefect of Lille during the latter part of the war, sent a most interesting paper on the influence that the diminution of pressure exerts on animal life. Living frogs were placed under the air-pump, and proved to be killed very soon if pressure is diminished quickly to seven or eight inches, but if diminished gradually, they can live in a more perfect vacuum if proper precautions are taken to renew the residual air offered to them for respiration. Certainly the same thing can be said of aëronauts, who cannot reach a high level without inconvenience, except by very gradual ascent.—M. Dumas presented a small pamphlet from M. Janssen, narrating his ascent on December 2 with Volta. Dr. Janssen was himself the aëronaut, and his ascent was the occasion of some interesting observations. He was appointed a commissioner for visiting the meteorological establishments in England, and reporting upon them, and is now on his way to London.—M. Beaugrand, an engineer in the Parisian hydraulic service, presented a report on Roman aqueducts. He has written a very long essay on the matter, which would have been burned by the Communists with his office at the Hotel de Ville, if he had not brought it home on purpose to write out of it a paper for the Academy.

### Vienna

**Imperial Academy of Sciences**, April 13.—Prof. von Reuss reported on the fossil remains of a crab found in the Leithakalk of the Rauchstallbrunn pit near Baden. The fossil most nearly approaches the living genera *Actæon* and *Daira*.—Prof. A. von Waltershofen reported on a new thermopile of great efficacy.—Prof. V. Graber communicated a memoir on the physiology and minute anatomy of insects, especially the Pediculina, in which he treated chiefly of the Malpighian vessels and tracheæ. The former in many cases consist merely of prolongations of the peritoneal membrane.—Prof. V. von Lang presented a memoir containing researches upon the influx of gases, undertaken for the purpose of testing the laws which have been established for the dependency of inflowing gases upon the pressure.—Prof. C. von Ettingshausen presented a first memoir upon the flora of Sagor in Carniolia, in which he described numerous species of fossil plants from the brown coal of that locality. This memoir included the Thallophytes, vascular Cryptogams, Gymnosperms, Monocotyledons, and Apetalæ. The Thallophytes include a *Sphæria* nearly allied to the Greenland species, and a *Laurencia*, which is the only marine plant found in the deposit. Of the Coniferæ *Glyptostrobus europæus* and *Sequoia Couttsiæ* are the most abundant, and of the latter genus three other species occur. A *Cunninghamia*, very like *C. sinensis*, is remarkable as adding a new genus to the Tertiary flora. Grasses are rare, but Naiadæ are abundant and remarkable. A *Pandanus* and a species of palm occur. Among the Apetalæ the author noticed two species of *Casuarina*, one of which is new and allied to *C. quadrivalvis*. The other orders represented are Myricaceæ 3 species, Betulaceæ 6, Cupuliferæ 15, Ulmaceæ 4, Celtideæ 2, Artocarpeæ 2, Salicineæ 2, Nyctagineæ 1, Monimiaceæ 1, Santalaceæ 4, Daphnoideæ 2, Proteaceæ, 21, Moreæ 19, and Laurineæ 18.—Prof. Carl Koritska exhibited and explained a hypsometrical map of the Alban Mountains, with profiles and views. He regarded the district as particularly instructive, from the intimate collocation of the three forms of volcanic craters and their apparent transition one into the other which prevails there.—Dr. E. Klein communicated a contribution to the knowledge of the Malpighian corpuscles in the human kidney, by Dr. Victor Seng; and a contribution to the knowledge of the finer nerves of the buccal mucous membrane, by Dr. E. Elin.—Prof. Ludwig Boltzmann presented a memoir containing several propositions on the equilibrium of heat, and another on the main proposition of the mechanical theory of heat.—Prof. E. Weiss furnished the elements and ephemeris of the comet discovered by Winnecke at Carlsruhe on the 7th April.

April 20.—Prof. C. von Ettingshausen presented a memoir on the leaf-skeleton of the Loranthaceæ.—Prof. Simony noticed some peculiarities of the glaciers of the Dachsteingebirge. The Gosau glacier descends to an elevation of 6030 feet, the Hallstatt glacier to 6115 feet, and the Schladminger Ferner to 6935 feet. The most instructive moraine phenomena are presented by the lower part of the Hallstatt glacier.—Prof. Seeger presented a memoir on the methods at present employed for detecting small quantities of sugar in the urine, which he regards as unsatisfactory.—A paper on the perforations in the vessels of plants, by Dr. Tangl, was communicated by Prof. Ad. Weiss.

April 27.—Prof. Lang communicated some remarks on the abnormal dispersion observed by Christiansen and Kundt in solutions of fuchsine, cyanine, &c. He showed that the appearance is due to the defective achromatism of the human eye.—M. F. Schwackhöfer reported on the occurrence and mode of formation of phosphorite balls in Russian Podolia. He stated that these balls were originally carbonate of lime formed by concretion, and converted into phosphate of lime formed by the lixiviation of the Silurian clay slate in which they occur, which contains phosphoric acid. The analysis of these balls led to the formula $3\ (Ca^3\ P^2\ O^8) + Ca\ Fl^2$, agreeing with that of apatite in the proportion of fluorine.

### Philadelphia

**American Philosophical Society**, April 21.—Dr. Genth described the results of recent investigations by himself into Corundun pseudomorphs of Hersinite, an aluminate of oxide of iron, from specimens from Bengal. He reported finding specimens of Hersinite in N. Carolina Corunduns, and believes the emery of Massachusetts is to be referred to the same mineral. In Chester County, Penna., "Corundun pseudomorphs" occur which are quite soft like talc or scaly talc, which prove to be Margarite. A third pseudomorph very much foliated has not yet been determined.—Prof. Cope presented a paper entitled, "A preliminary report on the Vertebra discovered in the Port Kennedy Cave."—Prof. Cresson stated that the young and tender shoots of the *Symplocarpus fortidus* (skunk cabbage) had forced themselves through a solid asphaltum composition pavement two inches in thickness in many places in "Belmont Glen," Park. The road was used for heavy hauling at the time. —Prof. J. P. Lesley described a discovery which he had made in East Tennessee of a sharp anticlinal axis crossing the coal measures of the Cumberland Mountains at right angles to the dominant system of disturbances, and showed its important bearing on the question of the conversion of the northern anticlinals of the Alleghanies, into the southern system of downthrows. Also its relationship to the latter and to the cross undulations worked out by Joseph Lesley in his instrumental survey of E. Kentucky thirteen years ago; and to the N.W.-S.E. system of faults described by Owen, Hall, and other geologists in the Valley of the Mississippi.

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THURSDAY, AUGUST 3, 1871

## THE ADVANCEMENT OF SCIENCE IN SCHOOLS

WHILE the leaders of Science are in session, and every topic of scientific interest can be brought before them with unusual force and most favourable publicity, we desire to urge the claims of one particular subject as lying at the foundation of all real scientific progress in this country. It is impossible that Science can take root amongst us, that it can inform the national mind or raise the national reputation, while it is excluded from the vast majority of our schools, and while the few schools which have ventured to introduce it are left to struggle unassisted against almost overwhelming difficulties. There are those who congratulate us on the advances made within the last two years, who point with pride to the Eton telescope and the Rugby laboratory, to the Botanical Garden of Clifton and the Scientific Society of Harrow. No doubt the evidence thus cited is most gratifying; no doubt the thanks of the community are due to the men whose individual wisdom and energy have made so admirable a beginning; but if their success is to produce in us only self-complacency, and to hide the enormous deficiencies which it ought to make more glaring and conspicuous, their efforts have been worse than vain.

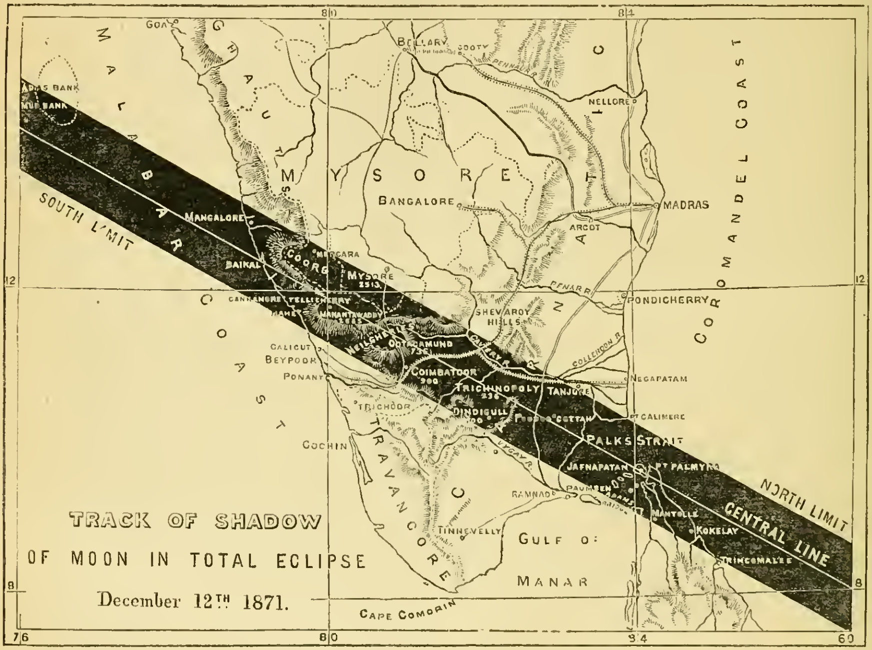
Let us ask the following questions. Of our countless Secondary Schools how many teach or profess to teach Natural Science in any shape whatever? Are there twenty schools in England which teach it systematically on a scale at all extensive, with special master and necessary apparatus? Is there one which accords to it such a place in comparison with other subjects of school teaching as is due to its inherent educational value, its practical use in after life, and the extent to which it is attracting and unfolding the chief intellects of the day? Lastly, are the schools which teach it honestly working on a well-considered plan, agreed amongst themselves as to the economies of methods, subjects, tests; or are their systems contradictory and chaotic, are they ignorant of each others' experience, are their efforts tentative and independent, their results often nugatory, their progress necessarily slow?

There is but one answer to these questions. Science teaching in our schools is as yet potential merely. It rests with those whom we are addressing to make it actual. Observers most conversant with the difficulties which have hitherto kept Science out of schools or paralysed it when nominally admitted, feel most strongly that combined and intelligent action on its behalf, undertaken by men of commanding influence and reputation, is the one thing needful to ensure for it existence, vitality, and permanence. So long as the necessity of teaching it to boys was denied, the action of authority would have been premature. It was necessary that public opinion should be formed, and that experience and argument should work the slow process of conversion. But its claims are now, in theory, established. The most bigoted no longer venture to question its utility; the champions of the old exclusive and one-sided culture are silenced, if not convinced; the general public has pronounced warmly in its favour; the masters and managers of schools are prepared in almost all cases, freely or grudgingly, to admit it. And if this be so; if the principles of opposition are surrendered, and objection rests only upon details; if, further, the deterrent details thus interposing are notorious, and are of a kind which authority, or enlightenment, or guidance, placed in sufficient hands and wielded with sufficient energy, can obviate, surely we may call upon the men whom the suffrage of the scientific world has saluted as its leaders to originate such a plan and to carry out such measures as may supplement the victory of reason over prejudice by assisting willing votaries and kindling half-roused enthusiasm.

There are cases in which the support of external authority is needful for the introduction of Science into schools. Probably few of the readers of NATURE are aware how bitter an opposition is offered to Science teaching by the clergy in many parts of England. The schoolmaster, who, being himself a clergyman, ventures to insist on Science as a necessity in his school curriculum, finds himself the object of a conspiracy as adroit as it is unscrupulous. No matter how able and energetic he may be; no matter how unmistakeably he may care for the moral and religious training of his boys; there is an accursed thing in the midst of him; the word goes forth to ostracise him; the dextrous calumny is dropped in fitting places, his neighbours send their sons elsewhere, and his schemes are broken up. This, which has happened more than once, must happen many times, unless such hapless pioneers of Science can be made to feel that they are backed by men of character, by men whose names are known, to whom they can appeal, who will interfere on their behalf with weight to convince or to overawe their persecutors.

In quite another way again authority is needed. Public competitive examinations, for the universities or elsewhere, must always exercise a paramount influence upon the schools, and must stamp in great measure the value of the subjects taught. It may well be doubted whether in the examinations for India and for Woolwich scientific excellence is appraised sufficiently high. It is quite certain that the influence of the universities both on the higher and lower schools is what it ought not to be in this respect. The local examinations, excellent in many points, vicious in some few, are most vicious in their operation upon Science. The unwise limitation of the subjects taken up, with the certainty that classical and mathematical papers gain many more marks than chemistry or mechanics, prevent the boys in a widely taught school from taking Science in at all, and help to deter masters from a subject which will not count in the examination. And unless they are closely watched, the "matriculation" or "leaving" examinations now contemplated both by Oxford and by Cambridge will be more disastrous still. Between the universities clinging to old subjects as desperately as they distrust the new, and the schoolmasters defeating by nearly ten to one the proposal to give boys the choice between a "linguistic" and a "scientific" matriculation, an obstacle more serious than any which now exists will be built up in the path of Science teaching, if its natural supporters stand aloof from the progress of a mischief which it now lies within their power to avert.

But if School Science lacks authority to help it, it lacks guidance and enlightenment still more. For it may be taken as an established fact that the head masters are, as a body, absolutely helpless. No one can doubt this who will peruse their published utterances on the subject at the Sherborne Meeting in December last. Nor need they be ashamed of the imputation. They owe their position in almost every case to their high classical or mathematical reputation. They are so large minded as to appreciate and to wish to foster in their school studies of whose details they know nothing, and should be allowed to feel that in opening their doors to Science they may fall back with confidence upon supreme and accredited advisers. Think of the difficult points which, without previous experience of any kind, they are called upon to settle. The main subjects of teaching, their relative value, and the order in which they should be taught, the age at which scientific study should commence, the extent to which it may be optional or must be compulsory, the merits and demerits of bifurcation, the text-books to be used, the time to be allowed, the methods of teaching, the frequency of examinations, the mode of obtaining teachers, the necessary apparatus, the arrangement of museums, laboratories, botanic gardens,—on all these points and on more blank and total ignorance holds the minds of many masters, while others are puzzling them out with cruel




MAP OF THE PATH OF THE TOTAL SOLAR ECLIPSE IN DECEMBER NEXT

waste of force, destitute of traditions, ignorant of each others' experience, lacking central guidance.

For such guidance where are they to look, if not to the British Association? It includes men fitted for such a task beyond any others in the country, men individually of commanding reputation, representing severally the great towns, the Universities, the commercial centres. Is it too much to hope that a board of such men as these might assume, at the request and by the appointment of their brethren, the task of counsellers and supporters to the schools in the difficult task which lies before them? They might deliberate on the points which we have noticed, and draw up rules for a scientific course which all schools would adopt. They might send missionaries to schools newly entering upon their task, who should advise upon the many points no published rules could cover. They might suggest and accredit text-books, might bespeak and cheapen apparatus, might secure from Government facilities for obtaining specimens, for stocking gardens, for borrowing or renting instruments. Established more and more securely as the representatives and controllers of scientific education, they would see their power spread from the schools to the Universities, from the Universities throughout the country.

But we forbear. We have stated the difficulties which beset scientific education in our schools, we have hinted at means which may remove them. Our description is only too real, our project may be too chimerical. Be it so. The chimera of one age is often the truism of the next. Let us only call upon our friends at Edinburgh, before they separate for another year, to take this great subject into consideration, and to weigh its claims on their activity. Many a solitary teacher will be cheered, many a half-abandoned scheme will be preserved and furthered, if not by the certainty of their support, yet at any rate by the knowledge of their sympathy.

## *THE APPROACHING TOTAL SOLAR ECLIPSE*

WE regret that we have, as yet, nothing very definite to announce in addition to what has been already stated with reference to the observations of the Total Solar Eclipse of the 12th of December next. We believe that an appeal is about to be made to Government, and if this be so, we may trust that anything that may be asked in the interests of Science will readily be granted by the Government. It is unfortunate that the Astronomer Royal's official position prevents his joining in the request, for his experience in connection with the large expenditure (10,000*l*. has already been voted) incurred by him for the approaching observation of the Transit of Venus, would be valuable in showing the necessity for the sum now required. This amounts only to a few hundreds in excess of the sum saved by the rigid economy practised by the Committee appointed to organise the arrangements connected with the late expedition.

We trust that the proposed arrangements will be brought before the British Association, in order that the influence of that important body may be made to bear upon this matter. We have recently shown the important results obtained by the late observations. It seems clear that the weather prospects for the approaching event are good, while recent calculations made by Mr. Hind show that the totality in Ceylon is much longer than had been at first imagined, amounting to as much as $2^m\ 11^s$ for Trincomalee, and therefore longer in the central line a few miles to the north. The accompanying map shows approximately the shadow path over India, and gives us good ground for congratulating ourselves that there are already in that country such observers as Tennant, Pogson, Herschel, Hennessy, and others, ready to occupy the best stations. The appeal made to Government includes funds for an expedition to Ceylon, under the charge of Mr. Lockyer, who has been requested by the Royal Astronomical Society to undertake spectroscopic observations there, while M. Janssen will probably take up his station in Java. We have already stated that a strong party from Melbourne and Sydney will observe in the north of Australia. All then is in order, provided our scientific leaders will put their shoulders to the wheel.

## *NOTES*

THE American Association for the Advancement of Science, which meets a fortnight later than our own at Indianapolis, is modelled in most respects after the pattern of the parent institution, but presents some features which the managers of our own Association may do well to take into consideration. The arrangements with regard to the opening address, sectional proceedings, &c., are very similar, the following being the officers for the Indianapolis meeting:—President, Prof. Asa Gray, of Cambridge; Vice-president, Prof. George F. Barker, of New Haven; Permanent Secretary, Prof. Joseph Lovering, of Cambridge; General Secretary, Mr. F. W. Putman, of Salem; Treasurer, Mr. Wm. S. Vaux, of Philadelphia. Special convenience will be provided for microscopists in relation to the exhibition and care of any instruments or apparatus, a suite of rooms having been secured in the State House for their special use. It will be remembered that the same thing was attempted at the Liverpool meeting, but in rather a private and unacknowledged manner. Excursions are arranged to Terre Haute, a distance of seventy-three miles, including a visit to the celebrated block coal field and blast furnaces of Clay county, and to New Albany on the Ohio river, where there are a number of interesting manufactories, among them the only finishing plate-glass works in the United States. Special arrangements have been made as to terms for the accommodation of the members of the Association at hotels and boarding-houses, and it is expected that all the railroads will carry the visitors at half fares.

ALTHOUGH the Report of the Science and Art Department in the year 1870 is not yet published, we believe that the following chief results, taken from the *Times*, may be relied upon as accurate. The numbers who during 1870 have attended the schools, museums, and other institutions receiving Parliamentary aid, considerably exceed those of 1869. There is a very large increase in the number of persons receiving instruction in science applicable to industry, which has risen from 24,865 in 1869 to 34,283 in 1870, or upwards of 37 per cent. At the Royal School of Mines there were 17 regular and 124 occasional students, at the Royal College of Chemistry 121 students, at the Royal School of Naval Architecture there were 40, and at the Metallurgical Laboratory 24. The evening lectures at the Royal School of Mines were attended by 2,574 artisans, school teachers, and others; and 243 science teachers attended the special courses of lectures provided for their instruction. At the Royal College of Science, Ireland, there were 17 associate or regular students and 21 occasional students. The various courses of lectures delivered in connection with the department in Dublin were attended by 1,152 persons, and at the Evening Popular Lectures, which were given in the Edinburgh Museum of Science and Art during the session 1869-70, there was an attendance of 1,195. The total number of persons who received direct instruction as students or by means of lectures in connection with the Science and Art Department in 1870 was upwards of 254,000. showing an actual increase as compared with the number in the previous year of 67,000, or nearly 36 per cent., and an increase in the rate of progress of 8 per cent.; the numbers in 1869 having been nearly 28 per cent. higher than in 1868. The museums and collections under the superintendence of the department in London, Dublin, and Edinburgh, have been visited during the past year by 1,847,929 persons, showing an increase of 49,087 on the number in 1869. As we have said before, it is impossible to over-estimate the importance of the work which is being done.

THE correspondence between the Royal Commission on Scientific Instruction and the Advancement of Science and the Science and Art Department on the subject of the transfer of the School of Mines to South Kensington, has been presented to Parliament.

THE assertion made by a contemporary relative to the endowment at University College of a De Morgan professorship of mathematics, has given rise to the statement by Prof. T. Hewitt Key, to the effect that he now withdraws the proposal, not merely because it is said by the family to be at variance with the expressed wishes of the deceased, but more because it has been hinted that he has been unworthily "using Prof. de Morgan's name against such expressed wishes for the emolument of the college." The endowment of a mathematical chair still remains as an object to which his best energies will be applied.

Prof. Key points out that the doctrine is now practically admitted that for all chairs in a college of any pretensions a fixed salary is essential, and that the principle has been recognised in Owens College, Manchester, the Queen's College in Ireland, the Government School of Mines, and the new Indian College for Engineering.

AT an extraordinary General Meeting of the members of University College held on Saturday last, the Right Hon. Lord Belper, LL.D., F.R.S., was unanimously elected President of the College in the place of the late Mr. George Grote. At a session of the council, on the same day, the following appointments were made :—Mr. W. K. Clifford, Fellow of Trinity College, Cambridge, to be Professor of Applied Mathematics and Mechanics; Prof. H. C. Bastian, M.D., F.R.S., to be Physician to University College Hospital; Mr. Berkeley Hill, M.B., Mr. Christopher Heath, and Mr. Marcus Beck, M.S., M.B., to be teachers of Practical Surgery. The Sharpey Scholarship, recently established for the promotion of the study of Biological Science in the college, was conferred upon Mr. E. A. Schäfer.

UNIVERSITY COLLEGE, London, has recently been enriched by several valuable donations and legacies. Mr. Grote left 6,000*l*. for the endowment of a chair of Mental Philosophy, and Mr. James Yates legacies to be similarly applied for the teaching of Geology and Archæology. The treasurer of the College has given an endowment of 200*l*. for five years for the chair of Applied Mathematics, and the late Prof. Graves left a legacy to the College, without a rival of its kind, in the shape of a Mathematical Library, consisting of more than 10,000 volumes, besides some 500 pamphlets.

THE Senate of University College has appointed as its Professor of Hindustani, Kazi Shahabudeen Ibrahim. This gentleman is an accomplished scholar, and held a high position in our service in India. He was afterwards Dewar of the Rajah of Kutch, and is now resident for him in London. He also acts as hon. secretary of the East India Association. Kazi Shahabudeen being a thorough master of our own language, has a great advantage, and we may indeed observe that the progress of English studies in India ought greatly to promote those of the Indian languages in England. We can now get men having literary proficiency in their own languages, and that acquaintance which few but a native can attain, while they have the full power of communicating their knowledge to students in our colleges.

IN an article which will be found elsewhere, we allude to the approaching Total Eclipse of the Sun. On this subject we may refer to a very interesting letter which Mr. Hind has recently addressed to the *Times* on the next Total Solar Eclipse which will be visible in England. Our readers will gather that we shall have some time to wait. Mr. Hind tells us that in the year 1954, June 30, the zone of totality just touches the British Isles, and adds "to discover an eclipse that will be total in England, I have found it necessary to continue the calculations to nearly the close of the same century. Such an eclipse (according to my investigation) will not occur until the 11th of August, 1999, when the circumstances will be nearly as follows :—The central and total eclipse will enter upon the earth's surface in the southern part of the Gulf of Mexico; thence traversing the Atlantic, it meets the English coast at Padstow, in Cornwall, and crossing the south of Devon enters the Channel at Torquay (which will be the most favourable place for observation in this country), and passing over the Eddystone, reaches France about fifteen miles east of Dieppe. It will be central and total, with the sun on the meridian some twenty-five miles south-west of Pesth, and traversing Asia Minor, Persia (at Ispahan), &c., will finally leave the earth's surface in the Bay of Bengal. At Torquay the first contact of limbs, or commencement of the eclipse, occurs at 8.23 A.M. local mean time, and the last contact at 11.20 A.M. Totality begins at 10h. 0m. 43s., with the sun at an altitude of 48°, and continues 2m. 4s. At Plymouth the duration of total eclipse is 1m. 58s., at Weymouth 1m. 55s. The southern part of the Isle of Wight falls within the northern limit of totality according to my calculation." Further, on the subject of the last Total Solar Eclipse visible in London, which occurred on the 3rd of May, 1715, and was successfully observed in the metropolis and at many other English stations, Mr. Hind states, it is "necessary to look further back than the year 1140 for the total solar eclipse in London next preceding that of 1715. I greatly doubt if, excepting the eclipse of August 11, 1999, described above, there can be any total solar eclipse visible in England for two hundred and fifty years from the present time."

THE grounds of the Royal Observatory, Greenwich, are now being rapidly occupied with the temporary observatories and instruments which are to be used for the observations of the Transit of Venus in 1874. We could wish that equal energy were shown in arrangements for other observations which are quite as important as those in question.

THE following are the names of the successful candidates in the competition for the Whitworth Scholarships, 1871, in the Science and Art Department :—Edmund F. Mondy, Rotherhithe; Samuel Anglin, Manchester; George Smith, Birmingham; John Yeo, Portsmouth; Henry H. Greenhill, Portsea; John Armitage, Oldham; William Lee, London; Samuel A. Kirkby, Cambridge; Benjamin A. Raworth, Manchester; George C. V. Holmes, Sydenham.

THE French weekly scientific journal, *Les Mondes*, entered, with its last number, on its 25th volume.

THAT excellent body, the Smithsonian Institution, Washington, has recently issued its report for the year 1869, in addition to which we have, under the same cover, Bertrand's paper on the Life and Works of Kepler, Arago's Eulogy on Thomas Young, Memoirs of Auguste Bravais and von Martius, a paper on the Chemistry of the Earth by Sterry Hunt, another on the Electrical Currents of the Earth by Matteucci, another on the Phenomena of Flight by Marey, and so on,—we really have not space to name all the titles,—and we have already said enough to indicate the extreme value of the volume. Among recent memoirs and papers published by the same Institution, we may mention a paper on the magnetic survey of Pennsylvania by Dr. Bache, on the Gleddon mummy case by Dr. Pickering, and on the phenomena and laws of aurora borealis by Loomis.

WE are glad to see that in the list of Civil Service Pensions just issued, the claims of Science have been recognised by the grant of 100*l*. to Mr. Charles Tilston Beke, in consideration of his geographical researches, and especially of the value of his explorations in Abyssinia; and 150*l*. to Mrs. Emily Coles, widow of Captain Cowper Phipps Coles, in consideration of her husband's services as inventor of the turret ship system.

THE *Revue Scientifique* publishes an account of the chemical investigations and works of the late Prof. Payen, the most important of which are as follows :—In 1824 he made his first investigations on the value of manures; in 1830 he presented to the Society of Agriculture a paper on the means of utilising all the parts of dead animals in the country; in 1836 he read a memoir on the elementary composition of starch in different plants; in 1837 he established the composition of dextrine from its definite combinations with oxide of lead and baryta; and in the same year he read a paper on the distribution of nitrogenous matters in the organs of vegetables; in 1838 he presented a very important memoir on the composition of woody tissue, and pointing out the distinction between cellulose and starch: in 1841 he prepared a memoir, in conjunction with M. Boussingault, on the relative value of different manures; in 1847 a paper on sugar in beetroot; in 1852 two very complete memoirs on caoutchouc and gutta-percha, their chemical composition and different characters; in 1859 another paper on starch and cellulose; in 1861 one on dextrine and glucose; in 1867 a paper on the constitution and structure of woody tissue; besides a large number of others on economical and vegetable chemistry. As separate works, Prof. Payen published a compendium of theoretical and practical agriculture, a compendium of industrial chemistry, a work on the diseases of the potato, beet, corn, and wine, a treatise on the distillation of beet, a work on alimentary substances, and a report on the vegetable and animal substances made to the French Committee of the Jury of the International Exhibition in London. He was appointed Professor of Industrial Chemistry at the Central School in 1830, and at the Conservatoire des Arts et Métiers in 1839, and was elected member of the Institute in 1842.

WE are informed by Dr. Edward L. Moss, R N., that within the last few days he has obtained several specimens of *Appendicularia furcata* and *acrocerca* in the incoming tide off the east coast of Portland. They have in every instance been captured in their "Haus," or have formed it shortly after capture, and have remained in it as long they were left undisturbed. These rare and interesting visitors to our tidal waters were accompanied by oceanic diatoms which Dr. Moss had never before seen near the English coast.

THE sixth annual meeting of the Quekett Microscopical Club was held on Friday evening last at University College. By the annual report of the committee read, it appeared that the number of the members now amounts to 550. The president, Dr. L. S. Beale, F.R.S., gave the usual presidential address. At the election of officers which followed, Dr. L. S. Beale was elected president for the year 1871-72; for vice-presidents, Dr. Robert Braithwaite, F.L.S., Mr. Arthur E. Durham, F.R.C.S., Mr. Charles J. Leaf, F.R.M.S., Mr. Henry Lee, F.L.S.; for four members of committee, Messrs. W. H. Golding, Thomas Greenish, E. Marks, and F. Oxley; for treasurer, Mr. Robert Hardwicke, F.L.S.; hon. secretary for foreign correspondence, Mr. M. C. Cooke, M.A.; hon. secretary, Mr. T. Charters White.

THE Royal Archæological Institute has just held its annual meeting at Cardiff, under the presidency of the Marquis of Bute, who, in his inaugural address, dwelt on the many objects of archæological interest in which South Wales abounds, especially as the locality of some of the best known incidents of the Arthurian romances. The historical section was presided over by Mr. G. A. Freeman, who delivered a very interesting address on the early ethnology of South Wales. A long excursion was undertaken by the members into Monmouthshire, the principal bjects of interest being Caldicot Castle, Caerwent (the Roman Venta Silurium) and Chepstow.

THE annual meeting of the Institution of Mechanical Engineers was held last week at Middlesborough, Mr. John Ramsbottom, of Crewe, being president of the meeting. Papers were read by Mr. William Crossley, of the Askham Ironworks, Lancashire, on the manufacture of hæmatite iron; by Mr. J. Lowthian Bell, upon the preliminary treatment of materials used in the blast furnace; by Mr. Hill, on an improved compound cylinder blowing engines recently erected at the Lackenby Iron Works, Middlesbrough; a description of the geological features of Cleveland by Mr. John Jones, secretary to the iron trade of the district; by Mr John A. Haswell, of Gateshead, describing the break drums and the mode of working at the Ingleby incline on the Rosedale branch of the North Eastern Railway; by Mr. Jeremiah Head, of Middlesborough, on a simple construction of steam-engine governor, having a close approximation to perfect action; and by Mr. Charles Cochrane, of Middlesborough, on steam boilers with small water-space and Roots' tube boiler. The many objects of interest in the neighbourhood were also visited by the members.

## THE BRITISH ASSOCIATION MEETING AT EDINBURGH

EDINBURGH, *Wednesday Morning*

A VERY important point in the peregrination of the British Association lies in the fact that the men of science are now assembled in one of the foci of commercial enterprise, now in an old centre of learning, and now in a locality which, although coming under neither of these heads, yet gives large scope for benefiting the surrounding region. That the Association should meet at Edinburgh at this present juncture is extremely fortunate. In the first place Science was largely taught at Edinburgh by the aid of State-endowed professors before either of our old English Universities thought it worth while to investigate with any earnestness those branches of natural knowledge which are now recognised as not only the necessary accompaniment of a liberal education, but as the foundation of the nation's greatness. In the second place, we learn from the Edinburgh newspapers that the scientific mind of the metropolis of the North has been recently stirred on the subject of the importance of scientific research, and has addressed a memorial to the Royal Commission now sitting, urging that the point shall be strongly taken up.

It may be interesting to mention that this is the third time that the British Association has met at Edinburgh. The first time was in 1834, under the presidency of Sir Thomas Brisbane; the second in 1850, when Sir David Brewster occupied the chair. Already more than 1,300 members have entered their names, a larger number than were present at the last Edinburgh meeting.

The ample accommodation furnished by the Scotch capital is being admirably utilised by the local organisers. The Reception Room is in Parliament House; the sections meet in the University Buildings. In addition to the assemblage of our own *savans*, the following distinguished scientific foreigners are either in Edinburgh or are expected in the course of the meeting:—The Emperor of Brazil; Dr. Janssen, of Paris; Dr. Buys Ballot, of Utrecht; Prof. v. Baumhauer, of Haarlem; Prof. Van Beneden, of Louvain; Dr. D. Bierens de Haan, of Leyden; Dr. Boogaard; Dr. Colding, of Copenhagen; Prof. Delffs, of Heidelberg; Baron Desiderius; Baron Roland Eö oös, of Pesth; Don Asturo de Marcoastin, of Madrid; Prof. Margo, of Pesth; l'Abbé Moigno, of Paris; Prof. Morren, of Liége; Prof. Szabó, of Pesth; Prof. Zenger, of Prague; Dr. Youmans, of New York. Of these Dr. Janssen, Prof. Van Beneden, Dr. Buys Ballot, Profs. Szabó and Zenger, and Dr. Colding, have already arrived. The University of Edinburgh has taken the opportunity of conferring the honorary degree of LL.D. on the following distinguished men of science:- Dr. Gassiot, Prof. Sylvester, Prof. Stokes, Prof. Challis, Dr. Huggins, Dr. Allen Thomson, Dr. Janssen, Prof. Van Beneden, Dr. Colding, Mr. Spottiswoode, Dr. Carpenter, Prof. Andrews of Belfast, and Dr. Paget of Cambridge.

We are enabled, through the courtesy of the officers of the Association, to give in our present number full reports of the president's inaugural address, and of the opening addresses in Sections A, B, and C. In Prof. Geikie's address we have a suitable and altogether to be commended innovation in the shape of an account of the local geology of the neighbourhood, which has been printed separately, and issued with an admirably clear map.

Col. Yule has been appointed president of Section E in the place of the late Dr. Johnston. Geological excursions are projected to East Lothian and the coast of Berwickshire, the latter under the guidance of Prof. Geikie; a botanical excursion to the fertile collecting ground of Ben Ledi, in which Prof. Balfour will take part; a dredging expedition in the Frith of Forth; and visits for antiquarians and the lovers of the picturesque to Melrose, Dryburgh, Abbotsford, and Rosslyn. With this tempting bill of fare, if the weather only proves moderately propitious, the meeting of the British Association in Edinburgh must be an occasion to look back upon with pleasure by all who are fortunate enough to be able to take part in its proceedings.

## Inaugural Address of Sir William Thomson, LL.D., F.R.S., President

For the third time of its forty years' history the British Association is assembled in the metropolis of Scotland. The origin of the Association is connected with Edinburgh in undying memory through the honoured names of Robison, Brewster, Forbes, and Johnston.

In this place, from this chair, twenty-one years ago, Sir David Brewster said:—"On the return of the British Association to the metropolis of Scotland, I am naturally reminded of the small band of pilgrims who carried the seeds of this Institution into the more genial soil of our sister land." . . . . . . "Sir John Robison, Prof. Johnston, and Prof. J. D. Forbes were the earliest friends and promoters of the British Association. They went to York to assist in its establishment, and they found there the very men who were qualified to foster and organise it. The Rev Mr. Vernon Harcourt, whose name cannot be mentioned here without gratitude, had provided laws for its government, and, along with Mr. Phillips, the oldest and most valuable of our office bearers, had made all those arrangements by which its success was ensured. Headed by Sir Roderick Murchison, one of the very earliest and most active advocates of the Association, there assembled at York about 200 of the friends of science."

The statement I have read contains no allusion to the real origin of the British Association. This blank in my predecessor's historical sketch I am able to fill in from words written by himself twenty years earlier. Through the kindness of Prof. Phillips I am enabled to read to you part of a letter to him at York, written by David Brewster from Allerly by Melrose, on the 23rd of February, 1831:—

"Dear Sir,—I have taken the liberty of writing you on a subject of considerable importance. It is proposed to establish a British Association of men of science similar to that which has existed for eight years in Germany, and is now patronised by the most powerful Sovereigns of that part of Europe. The arrangements for the first meeting are in progress; and it is contemplated that it shall be held in York, as the most central city for the three kingdoms. My object in writing you at present is to beg that you would ascertain if York will furnish the accommodation necessary for so large a meeting (which may perhaps consist of above 100 individuals), if the Philosophical Society would enter zealously into the plan, and if the Mayor and influential persons in the town and in the vicinity would be likely to promote its objects. The principal object of the Society would be to make the cultivators of science acquainted with each other, to stimulate one another to new exertions, and to bring the objects of science more before the public eye, and to take measures for advancing its interests and accelerating its progress."

Of the little band of four pilgrims from Scotland to York, not one now survives. Of the seven first associates one more has gone over to the majority since the Association last met. Vernon Harcourt is no longer with us; but his influence remains, a beneficent and surely therefore never dying influence. He was a geologist and chemist, a large-hearted lover of science, and an unwearied worker for its advancement. Brewster was the founder of the British Association; Vernon Harcourt was its lawgiver. His code remains to this day the law of the Association.

On the 11th of May last Sir John Herschel died in the eightieth year of his age. The name of Herschel is a household word throughout Great Britain and Ireland—yes, and through the whole civilised world. We of this generation have, from our lessons of childhood upwards, learned to see in Herschel, father and son, a *præsidium et dulce decus* of the precious treasure of British scientific fame. When geography, astronomy, and the use of the globes were still taught, even to poor children, as a pleasant and profitable sequel to "reading, writing, and arithmetic," which of us did not revere the great telescope of Sir William Herschel (one of the hundred wonders of the world), and learn with delight, directly or indirectly from the charming pages of Sir John Herschel's book, about the sun and his spots, and the fiery tornadoes sweeping over his surface, and about the planets, and Jupiter's belts, and Saturn's rings, and the fixed stars with their proper motions, and the double stars, and coloured stars, and the nebulæ discovered by the great telescope? Of Sir John Herschel it may indeed be said, *nil tetigit quod non ornavit.*

A monument to Faraday and a monument to Herschel, Britain must have. The nation will not be satisfied with any thing, however splendid, done by private subscription. A national monument, the more humble in point of expense the better, is required to satisfy that honourable pride with which a high-spirited nation cherishes the memory of its great men. But for the glory of Faraday or the glory of Herschel, is a monument wanted? No!

> What needs my Shakespeare for his honoured bones
> The labour of an age in piled stones?
> Or that his hallowed relics should be hid
> Under a star-ypointing pyramid?
> Dear son of memory, great heir of fame,
> What need'st thou such weak witness of thy name?
> Thou, in our wonder and astonishment,
> Hast built thyself a live-long monument.
>
> And, so sepulchred, in such pomp dost lie,
> That kings for such a tomb would wish to die.

With regard to Sir John Herschel's scientific work, on the present occasion I can but refer briefly to a few points which seem to me salient in his physical and mathematical writings. First, I remark that he has put forward, most instructively and profitably to his readers, the general theory of periodicity in dynamics, and has urged the practical utilising of it, especially in meteorology, by the harmonic analysis. It is purely by an application of this principle and practical method, that the British Association's Committee on Tides has for the last four years been, and still is, working towards the solution of the grand problem proposed forty-eight years ago by Thomas Young in the following words:—

"There is, indeed, little doubt that if we were provided with a sufficiently correct series of minutely accurate observations on the Tides, made not merely with a view to the times of low and high water only, but rather to the heights at the intermediate times, we might form by degrees, with the assistance of the theory contained in this article* only, almost as perfect a set of tables for the motions of the ocean as we have already obtained for those of the celestial bodies, which are the more immediate objects of the attention of the practical astronomer."

Sir John Herschel's discovery of a right or left-handed asymmetry in the outward form of crystals, such as quartz, which in their inner molecular structure possess the helicoidal rotational property in reference to the plane of polarisation of light, is one of the notable points of meeting between Natural History and Natural Philosophy. His observations on "epipolic dispersion" gave Stokes the clue by which he was led to his great discovery of the change of periodic time experienced by light in falling on certain substances and being dispersively reflected from them. In respect to pure mathematics Sir John Herschel did more, I believe, than any other man to introduce into Britain the powerful methods and the valuable notation of modern analysis. A remarkable mode of symbolism had freshly appeared, I believe, in the works of Laplace, and possibly of other French mathematicians; it certainly appeared in Fourier, but whether before or after Herschel's work I cannot say. With the French writers, however, this was rather a short method of writing formulæ than the analytical engine which it became in the hands of Herschel and British followers, especially Sylvester and Gregory (competitors with Green in the Cambridge Mathematical Tripos struggle of 1837) and Boole and Cayley. This method was greatly advanced by Gregory, who first gave to its working-power a secure and philosophical foundation, and so prepared the way for the marvellous extension it has received from Boole, Sylvester,

* Young's; written in 1823 for the Supplement to the "Encyclopædia Britannica."

and Cayley, according to which symbols of operation become the subjects not merely of algebraic combination, but of differentiations and integrations, as if they were symbols expressing values of varying quantities. An even more marvellous development of this same idea of the separation of symbols (according to which Gregory separated the algebraic signs + and − from other symbols or quantities to be characterised by them, and dealt with them according to the laws of algebraic combinations) received from Hamilton a most astonishing generalisation, by the invention actually of new laws of combination, and led him to his famous "Quaternions," of which he gave his earliest exposition to the Mathematical and Physical Section of this Association, at its meeting in Cambridge in the year 1845. Tait has taken up the subject of quaternions ably and zealously, and has carried it into physical science with a faith, shared by some of the most thoughtful mathematical naturalists of the day, that it is destined to become an engine of perhaps hitherto unimagined power for investigating and expressing results in Natural Philosophy. Of Herschel's gigantic work in astronomical observation I need say nothing. Doubtless a careful account of it will be given in the "Proceedings of the Royal Society of London" for the next anniversary meeting.

In the past year another representative man of British science is gone. Mathematics has had no steadier supporter for half a century than De Morgan. His great book on the differential calculus was, for the mathematical student of thirty years ago, a highly prized repository of all the best things that could be brought together under that title. I do not believe it is less valuable now; and if it is less valued, may this not be because it is too good for examination purposes, and because the modern student, labouring to win marks in the struggle for existence, must not suffer himself to be beguiled from the stern path of duty by any attractive beauties in the subject of his study?

One of the most valuable services to science which the British Association has performed has been the establishment, and the twenty-nine years' maintenance, of its Observatory. The Royal Meteorological Observatory of Kew was built originally for a Sovereign of England who was a zealous amateur of astronomy. George the Third used continually to repair to it when any celestial phenomenon of peculiar interest was to be seen; and a manuscript book still exists filled with observations written into it by his own hand. After the building had been many years unused, it was granted, in the year 1842, by the Commissioners of Her Majesty's Woods and Forests, on application of Sir Edward Sabine, for the purpose of continuing observations (from which he had already deduced important results) regarding the vibration of a pendulum in various gases, and for the purpose of promoting pendulum observations in all parts of the world. The Government granted only the building—no funds for carrying on the work to be done in it. The Royal Society was unable to undertake the maintenance of such an observatory; but, happily for science, the zeal of individual Fellows of the Royal Society and members of the British Association gave the initial impulse, supplied the necessary initial funds, and recommended their new institution successfully to the fostering care of the British Association. The work of the Kew Observatory has, from the commencement, been conducted under the direction of a Committee of the British Association; and annual grants from the funds of the Association have been made towards defraying its expenses up to the present time. To the initial object of pendulum research was added continuous observation of the phenomena of meteorology and terrestrial magnetism, and the construction and verification of thermometers, barometers, and magnetometers designed for accurate measurement. The magnificent services which it has rendered to science are so well known that any statement of them which I could attempt on the present occasion would be superfluous. Their value is due in a great measure to the indefatigable zeal and the great ability of two Scotchmen, both from Edinburgh, who successively held the office of Superintendent of the Observatory of the British Association—Mr. Welsh for nine years, until his death in 1859, and Dr. Balfour Stewart from then until the present time. Fruits of their labours are to be found all through our volumes of Reports for these twenty-one years.

The institution now enters on a new stage of its existence. The noble liberality of a private benefactor, one who has laboured for its welfare with self-sacrificing devotion unintermittingly from within a few years of its creation, has given it a permanent independence, under the general management of a Committee of the Royal Society. Mr. Gassiot's gift of 10,000*l.* secures the continuance at Kew of the regular operation of the self-recording instruments for observing the phenomena of terrestrial magnetism and meteorology, without the necessity for further support from the British Association.

The success of the Kew Magnetic and Meteorological Observatory affords an example of the great gain to be earned for science by the foundation of physical observatories and laboratories for experimental research, to be conducted by qualified persons, whose duties should be, not teaching, but experimenting. Whether we look to the honour of England, as a nation which ought always to be the foremost in promoting physical science, or to those vast economical advantages which must accrue from such establishments, we cannot but feel that experimental research ought to be made with us an object of national concern, and not left, as hitherto, exclusively to the private enterprise of self-sacrificing amateurs, and the necessarily inconsecutive action of our present Governmental Departments and of casual Committees The Council of the Royal Society of Edinburgh has moved for this object in a memorial presented by them to the Royal Commission on Scientific Education and the Advancement of Science. The Continent of Europe is referred to for an example to be followed with advantage in this country, in the following words:—

"On the Continent there exist certain institutions, fitted with instruments, apparatus, chemicals, and other appliances, which are meant to be, and which are made, available to men of science, to enable them, at a moderate cost, to pursue original researches."

This statement is fully corroborated by information, on good authority, which I received from Germany, to the effect that in Prussia "every university, every polytechnical academy, every industrial school (Realschule and Gewerbeschule), most of the grammar-schools, in a word, nearly all the schools superior in rank to the elementary schools of the common people, are supplied with chemical laboratories and a collection of philosophical instruments and apparatus, access to which is most liberally granted by the directors of those schools, or the teachers of the respective disciplines, to any person qualified, for scientific experiments. In consequence, though there exist no particular institutions like those mentioned in the memorial, there will scarcely be found a town exceeding in number 5,000 inhabitants but offers the possibility of scientific explorations at no other cost than reimbursement of the the expense for the materials wasted in the experiments."

Further, with reference to a remark in the Memorial to the effect that, in respect to the promotion of science, the British Government confines its action almost exclusively to scientific instruction, and fatally neglects the advancement of science, my informant tells me that, in Germany, "professors, preceptors, and teachers of secondary schools are engaged on account of their skilfulness in teaching; but professors of universities are never engaged unless they have already proved, by their own investigations, that they are to be relied upon for the advancement of science. Therefore every shilling spent for instruction in universities is at the same time profitable to the advancement of science."

The physical laboratories which have grown up in the Universities of Glasgow and Edinburgh, and in Owens College, Manchester, show the want felt of Colleges of Research; but they go but infinitesimally towards supplying it, being absolutely destitute of means, material or personal, for advancing science except at the expense of volunteers, or securing that volunteers shall be found to continue even such little work as at present is carried on.

The whole of Andrews's splendid work in Queen's College, Belfast, has been done under great difficulties and disadvantages, and at great personal sacrifices; and up to the present time there is not a student's physical laboratory in any one of the Queen's Colleges in Ireland—a want which surely ought not to remain unsupplied Each of these institutions (the four Scotch Universities, the three Queen's Colleges, and Owens College, Manchester) requires two professors of Natural philosophy—one who shall be responsible for the teaching, the other for the advancement of science by experiment. The University of Oxford has already established a physical laboratory. The munificence of its Chancellor is about to supply the University of Cambridge with a splendid laboratory, to be constructed under the eye of Prof. Clerk Maxwell. On this subject I shall say no more at present, but simply read a sentence which was spoken by Lord Milton in the first Presidential Address to the British Association, when it met at York in the year 1831 :—"In addition to other more direct benefits, these meetings [of the British Association]. I hope, will be the means of impressing on the Government the conviction, that the love of scientific pursuits, and the means of pursuing them, are not confined to the metropolis; and I hope that when the Government is fully impressed with the knowledge of the great desire entertained to promote science in every part of the empire, they will see the necessity of affording it due encouragement, and of giving every proper stimulus to its advancement."

Besides abstracts of papers read, and discussions held, before the Sections, the annual Reports of the British Association contain a large mass of valuable matter of another class. It was an early practice of the Association, a practice that might well be further developed, to call occasionally for a special report on some particular branch of science from a man eminently qualified for the task. The reports received in compliance with these invitations have all done good service in their time, and they remain permanently useful as landmarks in the history of science. Some of them have led to vast practical results; others of a more abstract character are valuable to this day as powerful and instructive condensations and expositions of the branches of science to which they relate. I cannot better illustrate the two kinds of efficiency realised in this department of the Association's work than by referring to Cayley's Report on Abstract Dynamics,* and Sabine's Report on Terrestrial Magnetism † (1838).

To the great value of the former, personal experience of benefit received enables me, and gratitude impels me, to testify. In a few pages full of precious matter, the generalised dynamical equations of Lagrange, the great principle evolved from Maupertuis' "least action" by Hamilton, and the later developments and applications of the Hamiltonian principle by other authors are described by Cayley so suggestively that the reading of thousands of quarto pages of papers scattered through the Transactions of the various learned societies of Europe is rendered superfluous for any one who desires only the essence of these investigations, with no more of detail than is necessary for a thorough and practical understanding of the subject.

Sabine's Report of 1838 concludes with the following sentence: "Viewed in itself and its various relations, the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit; and we may feel quite assured that the completion of our knowledge of its distribution on the surface of the earth would be regarded by our contemporaries and by posterity as a fitting enterprise of a maritime people, and a worthy achievement of a nation which has ever sought to rank foremost in every arduous and honourable undertaking." An immediate result of this Report was that the enterprise which it proposed was recommended to the Government by a joint Committee of the British Association and the Royal Society with such success, that Capt. James Ross was sent in command of the *Erebus* and *Terror* to make a magnetic survey of the Antarctic regions, and to plant on his way three Magnetical and Meteorological Observatories, at St. Helena, the Cape, and Van Diemen's Land. A vast mass of precious observations, made chiefly on board ship, were brought home from this expedition. To deduce the desired results from them, it was necessary to eliminate the disturbance produced by the ship's magnetism; and Sabine asked his friend Archibald Smith to work out from Poisson's mathematical theory, then the only available guide, the formulæ required for the purpose. This voluntary task Smith executed skilfully and successfully. It was the beginning of a series of labours carried on with most remarkable practical tact, with thorough analytical skill, and with a rare extreme of disinterestedness, in the intervals of an arduous profession, for the purpose of perfecting and simplifying the correction of the mariner's compass—a problem which had become one of vital importance for navigation, on account of the introduction of iron ships. Edition after edition of the "Admiralty Compass Manual" has been produced by the able superintendent of the Compass Department, Captain Evans, containing chapters of mathematical investigation and formulæ by Smith, on which depend wholly the practical analysis of compass-observations, and rules for the safe use of the compass in navigation. I firmly believe that it is to the thoroughly scientific method thus adopted by the Admiralty, that no iron ship of Her Majesty's Navy has ever been lost through errors of the compass. The "British Admiralty Compass Manual" is adopted as a guide by all the navies of the world. It has been translated into Russian, German, and Portuguese; and it is at present being translated into French. The British Association may be gratified to know that the possibility of navigating ironclad war-ships with safety depends on application of scientific principles given to the world by three mathematicians, Poisson, Airy, and Archibald Smith.

Returning to the science of terrestrial magnetism we find in the Reports of early years of the British Association ample evidence of its diligent cultivation. Many of the chief scientific men of the day from England, Scotland, and Ireland, found a strong attraction to the Association in the facilities which it afforded to them for co-operating in their work on this subject. Lloyd, Phillips, Fox, Ross, and Sabine made magnetic observations all over Great Britain; and their results, collected by Sabine, gave for the first time an accurate and complete survey of terrestrial magnetism over the area of this island. I am informed by Prof. Phillips that, in the beginning of the Association, Herschel, though a "sincere well-wisher," felt doubts as to the general utility and probable success of the plan and purpose proposed; but his zeal for terrestrial magnetism brought him from being merely a sincere well-wisher to join actively and cordially in the work of the Association. "In 1838 he began to give effectual aid in the great question of magnetical observatories, and was indeed foremost among the supporters of that which is really Sabine's great work. At intervals, until about 1858, Herschel continued to give effectual aid." Sabine has carried on his great work without intermission to the present day; thirty years ago he gave to Gauss a large part of the data required for working out the spherical harmonic analysis for the altered state of terrestrial magnetism over the whole earth. A recalculation of the harmonic analysis for the altered state of terrestrial magnetism of the present time has been undertaken by Adams. He writes to me that he has "already begun some of the introductory work, so as to be ready when Sir Edward Sabine's Tables of the Values of the Magnetic Elements deduced from observation are completed, at once to make use of them," and that he intends to take into account terms of at least one order beyond those included by Gauss. The form in which the requisite data are to be presented to him is a magnetic Chart of the whole surface of the globe. Materials from scientific travellers of all nations, from our home magnetic observatories, from the magnetic observatories of St. Helena, the Cape, Van Diemen's Land, and Toronto, and from the scientific observatories of other countries, have been brought together by Sabine. Silently, day after day, night after night, for a quarter of a century he has toiled with one constant assistant always by his side to reduce these observations and prepare for the great work. At this moment, while we are here assembled, I believe that, in their quiet summer retirement in Wales, Sir Edward and Lady Sabine are at work on the Magnetic Chart of the world. If two years of life and health are granted to them, science will be provided with a key which must powerfully conduce to the ultimate opening up of one of the most refractory enigmas of cosmical physics, the cause of terrestrial magnetism.

To give any sketch, however slight, of scientific investigation performed during the past year would, even if I were competent for the task, far exceed the limits within which I am confined on the present occasion. A detailed account of work done and knowledge gained in science Britain ought to have every year. The Journal of the Chemical Society and the Zoological Record do excellent service by giving abstracts of all papers published in their departments. The admirable example afforded by the German "Fortschritte" and "Jahresbericht" is before us; but hitherto, so far as I know, no attempt has been made to follow it in Britain. It is true that several of the annual volumes of the Jahresbericht were translated; but a translation, published necessarily at a considerable interval of time after the original, cannot supply the want. An independent British publication is for many obvious reasons desirable. The two publications, in German and English, would, both by their differences and by their agreements, illustrate the progress of science more correctly and usefully than any single work could do, even if appearing simultaneously in the two languages. It seems to me that to promote the establishment of a British Year Book of Science is an object to which the powerful action of the British Association would be thoroughly appropriate.

* Report on the Recent Progress of Theoretical Dynamics, by A. Cayley, (Report of the British Association 1857, p. 1).

† Report on the Variations of the Magnetic Intensity observed at different points of the Earth's Surface, by Major Sabine, F.R.S. (forming part of the 7th Report of the British Association).

In referring to recent advances in several branches of science, I simply choose some of those which have struck me as most notable.

Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. But nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient long-continued labour in the minute sifting of numerical results. The popular idea of Newton's grandest discovery is that the theory of gravitation flashed into his mind, and so the discovery was made. It was by a long train of mathematical calculation, founded on results accumulated through prodigious toil of practical astronomers, that Newton first demonstrated the forces urging the planets towards the sun, determined the magnitude of those forces, and discovered that a force following the same law of variation with distance urges the moon towards the earth. *Then* first, we may suppose, came to him the idea of the universality of gravitation; but when he attempted to compare the magnitude of the force on the moon with the magnitude of the force of gravitation of a heavy body of equal mass at the earth's surface, he did not find the agreement which the law he was discovering required. Not for years after would he publish his discovery as made. It is recounted that, being present at a meeting of the Royal Society, he heard a paper read, describing geodesic measurement by Picard, which led to a serious correction of the previously accepted estimate of the earth's radius. This was what Newton required. He went home with the result, and commenced his calculations, but felt so much agitated that he handed over the arithmetical work to a friend; then (and not when, sitting in a garden, he saw an apple fall) did he ascertain that gravitation keeps the moon in her orbit.

Faraday's discovery of specific inductive capacity, which inaugurated the new philosophy, tending to discard action at a distance, was the result of minute and accurate measurement of forces.

Joule's discovery of thermo-dynamic law through the regions of electro-chemistry, electro-magnetism, and elasticity of gases was based on a delicacy of thermometry which seemed simply impossible to some of the most distinguished chemists of the day.

Andrews's discovery of the continuity between the gaseous and liquid states was worked out by many years of laborious and minute measurement of phenomena scarcely sensible to the naked eye.

Great service has been done to science by the British Association in promoting accurate measurement in various subjects. The origin of exact science in terrestrial magnetism is traceable to Gauss's invention of methods of finding the magnetic intensity in absolute measure. I have spoken of the great work done by the British Association in carrying out the application of this invention in all parts of the world. Gauss's colleague in the German Magnetic Union, Weber, extended the practice of absolute measurement to electric currents, the resistance of an electric conductor, and the electromotive force of a galvanic element. He showed the relation between electrostatic and electromagnetic units for absolute measurement, and made the beautiful discovery that resistance, in absolute electromagnetic measure, and the reciprocal of resistance, or, as we call it, "conducting power," in electrostatic measure, are each of them a velocity. He made an elaborate and difficult series of experiments to measure the velocity which is equal to the conducting power, in electrostatic measure, and at the same time to the resistance in electromagnetic measure, in one and the same conductor. Maxwell, in making the first advance along a road of which Faraday was the pioneer, discovered that this velocity is physically related to the velocity of light, and that, on a certain hypothesis regarding the elastic medium concerned, it may be exactly equal to the velocity of light. Weber's measurement verifies approximately this equality, and stands in science *monumentum ære perennius*, celebrated as having suggested this most grand theory, and as having afforded the first quantitative test of the recondite properties of matter on which the relations between electricity and light depend. A re-measurement of Weber's critical velocity on a new plan by Maxwell himself, and the important correction of the velocity of light by Foucault's laboratory experiments, verified by astronomical observation, seem to show a still closer agreement. The most accurate possible determination of Weber's critical velocity is just now a primary object of the Association's Committee on Electric Measurement; and it is at present premature to speculate as to the closeness of the agreement between that velocity and the velocity of light. This leads me to remark how much science, even in its most lofty speculations, gains in return for benefits conferred by its application to promote the social and material welfare of man. Those who perilled and lost their money in the original Atlantic Telegraph were impelled and supported by a sense of the grandeur of their enterprise, and of the world-wide benefits which must flow from its success; they were at the same time not unmoved by the beauty of the scientific problem directly presented to them; but they little thought that it was to be immediately, through their work, that the scientific world was to be instructed in a long-neglected and discredited fundamental electric discovery of Faraday's, or that, again, when the assistance of the British Association was invoked to supply their electricians with methods for absolute measurement (which they found necessary to secure the best economical return for their expenditure, and to obviate and detect those faults in their electric material which had led to disaster), they were laying the foundation for accurate electric measurement in every scientific laboratory in the world, and initiating a train of investigation which now sends up branches into the loftiest regions and subtlest ether of natural philosophy. Long may the British Association continue a bond of union, and a medium for the interchange of good offices between science and the world.

The greatest achievement yet made in molecular theory of the properties of Matter is the Kinetic theory of Gases, shadowed forth by Lucretius, definitely stated by Daniel Bernoulli, largely developed by Herapath, made a reality by Joule, and worked out to its present advanced state by Clausius and Maxwell. Joule, from his dynamical equivalent of heat, and his experiments upon the heat produced by the condensation of gas was able to estimate the average velocity of the ultimate molecules or atoms composing it. His estimate for hydrogen was 6,225 feet per second at temperature 60° Fahr., and 6,055 feet per second at the freezing-point. Clausius took fully into account the impacts of molecules on one another, and the kinetic energy of *relative* motions of the matter constituting an individual atom. He investigated the relation between their diameters, the number in a given space, and the mean length of path from impact to impact, and so gave the foundation for estimates of the absolute dimensions of atoms, to which I shall refer later. He explained the slowness of gaseous diffusion by the mutual impacts of the atoms, to which I shall refer later. He explained the slowness of gaseous diffusion by the mutual impacts of the atoms, and laid a secure foundation for a complete theory of the diffusion of fluids, previously a most refractory enigma. The deeply penetrating genius of Maxwell brought in viscosity and thermal conductivity, and thus completed the dynamical explanation of all the known properties of gases, except their electric resistance and brittleness to electric force.

No such comprehensive molecular theory had ever been even imagined before the nineteenth century. Definite and complete in its area as it is, it is but a well-drawn part of a great chart, in which all physical science will be represented with every property of matter shown in dynamical relation to the whole. The prospect we now have of an early completion of this chart is based on the assumption of atoms. But there can be no permanent satisfaction to the mind in explaining heat, light, elasticity, diffusion, electricity, and magnetism, in gases, liquids, and solids, and describing precisely the relations of these different states of matter to one another by statistics of great numbers of atoms, when the properties of the atom itself are simply assumed. When the theory, of which we have the first instalment in Clausius and Maxwell's work, is complete, we are but brought face to face with a superlatively grand question, what is the inner mechanism of the atom?

In the answer to this question we must find the explanation not only of the atomic elasticity, by which the atom is a chronometric vibrator according to Stokes's discovery, but of chemical affinity and of the differences of quality of different chemical elements, at present a mere mystery in science. Helmholtz's exquisite theory of vortex-motion in an incompressible frictionless liquid has been suggested as a finger-post, pointing a way which may possibly lead to a full understanding of the properties of atoms, carrying out the grand conception of Lucretius, who "admits no subtle ethers, no variety of elements with fiery, or watery, or light, or heavy principles; nor supposes light to be one thing, fire another, electricity a fluid, magnetism a vital principle, but treats all phenomena as mere properties or accidents of simple matter." This statement I take from an admirable paper on the atomic theory of Lucretius, which appeared in the *North British Review* for March 1868, containing a most interesting and instructive summary of ancient and modern doctrine regarding atoms. Allow me to read from that article one other short passage finely describing the present aspect of atomic theory:—"The existence of the chemical atom, already quite a complex little world, seems very probable; and the description of the Lucretian atom is wonderfully applicable to it. We are not wholly without hope that the real weight of each such atom may some day be known—not merely the relative weight of the several atoms, but the number in a given volume of any material; that the form and motion of the parts of each atom and the distances by which they are separated may be calculated; that the motions by which they produce heat, electricity, and light may be illustrated by exact geometrical diagrams; and that the fundamental properties of the intermediate and possibly constituent medium may be arrived at. Then the motion of planets and music of the spheres will be neglected for a while in admiration of the maze in which the tiny atoms run."

Even before this was written some of the anticipated results had been partially attained. Loschmidt in Vienna had shown, and not much latter Stoney independently in England showed, how to reduce from Clausius and Maxwell's kinetic theory of gases a superior limit to the number of atoms in a given measurable space. I was unfortunately quite unaware of what Loschmidt and Stoney had done when I made a similar estimate on the same foundation, and communicated to NATURE in an article on "The Size of Atoms." But questions of personal priority, however interesting they may be to the persons concerned, sink into insignificance in the prospect of any gain of deeper insight into the secrets of nature. The triple coincidence of independent reasoning in this case is valuable as confirmation of a conclusion violently contravening ideas and opinions which had been almost universally held regarding the dimensions of the molecular structure of matter. Chemists and other naturalists had been in the habit of evading questions as to the hardness or indivisibility of atoms by virtually assuming them to be infinitely small and infinitely numerous. We must now no longer look upon the atom, with Boscovich, as a mystic point endowed with inertia and the attribute of attracting or repelling other such centres with forces depending upon the intervening distances (a supposition only tolerated with the tacit assumption that the inertia and attraction of each atom is infinitely small and the number of atoms infinitely great), nor can we agree with those who have attributed to the atom occupation of space with infinite hardness and strength (incredible in any finite body); but we must realise it as a piece of matter of measurable dimensions, with shape, motion, and laws of action, intelligible subjects of scientific investigation.

The prismatic analysis of light discovered by Newton was estimated by himself as being "the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."

Had he not been deflected from the subject, he could not have failed to obtain a pure spectrum; but this, with the inevitably consequent discovery of the dark lines, was reserved for the nineteenth century. Our fundamental knowledge of the dark lines is due solely to Fraunhofer. Wollaston saw them, but did not discover them. Brewster laboured long and well to perfect the prismatic analysis of sunlight; and his observations on the dark bands produced by the absorption of interposed gases and vapours laid important foundations for the grand superstructure which he scarcely lived to see. Piazzi Smyth, by spectroscopic observation performed on the Peak of Teneriffe, added greatly to our knowledge of the dark lines produced in the solar spectrum by the absorption of our own atmosphere. The prism became an instrument for chemical qualitative analysis in the hands of Fox Talbot and Herschel, who first showed how, through it, the old "blowpipe test" or generally the estimation of substances from the colours which they give to flames, can be prosecuted with an accuracy and a discriminating power not to be attained when the colour is judged by the unaided eye. But the application of this test to solar and stellar chemistry had never, I believe, been suggested, either directly or indirectly, by any other naturalist, when Stokes taught it to me in Cambridge at some time prior to the summer of 1852. The observational and experimental foundations on which he built were:—

1. The discovery by Fraunhofer of a coincidence between his double dark line D of the solar spectrum and a double bright line which he observed in the spectra of ordinary artificial flames.

2. A very rigorous experimental test of this coincidence by Prof. W. H. Miller, which showed it to be accurate to an astonishing degree of minuteness.

3. The fact that the yellow light given out when salt is thrown on burning spirit consists almost solely of the two nearly identical qualities which constitute that double bright line.

4. Observations made by Stokes himself, which showed the bright line D to be absent in a candle-flame when the wick was snuffed clean, so as not to project into the luminous envelope, and from an alcohol flame when the spirit was burned in a watch-glass. And

5. Foucault's admirable discovery (L'Institut, Feb. 7, 1849) that the Voltaic arc between charcoal points is "a medium which emits the rays D on its own account, and at the same time absorbs them when they come from another quarter."

The conclusions, theoretical and practical, which Stokes taught me, and which I gave regularly afterwards in my public lectures in the University of Glasgow, were:

1. That the double line D, whether bright or dark, is due to vapour of sodium.

2. That the ultimate atom of sodium is susceptible of regular elastic vibrations, like those of a tuning-fork or of stringed musical instruments; that like an instrument with two strings tuned to approximate unison, or an approximately circular elastic disc, it has two fundamental notes or vibrations of approximately equal pitch; and that the periods of these vibrations are precisely the periods of the two slightly different yellow lights constituting the double bright line D.

3. That when vapour of sodium is at a high enough temperature to become itself a source of light, each atom executes these two fundamental vibrations simultaneously; and that therefore the light proceeding from it is of the two qualities constituting the double bright line D.

4. That when vapour of sodium is present in space across which light from another source is propagated, its atoms, according to a well-known general principle of dynamics, are set to vibrate in either or both of those fundamental modes, if some of the incident light is of one or other of their periods, or some of one and some of the other; so that the energy of the waves of those particular qualities of light is converted into thermal vibrations of the medium and dispersed in all directions, while light of all other qualities, even though very nearly agreeing with them, is transmitted with comparatively no loss.

5. That Fraunhofer's double dark line D of solar and stellar spectra is due to the presence of vapour of sodium in atmospheres surrounding the sun and those stars in whose spectra it had been observed.

6. That other vapours than sodium are to be found in the atmospheres of sun and stars by searching for substances producing in the spectra of artificial flames bright lines coinciding with other dark lines of the solar and stellar spectra than the Fraunhofer line D.

The last of these propositions I felt to be confirmed (it was perhaps partly suggested) by a striking and beautiful experiment admirably adapted for lecture illustrations, due to Foucault, which had been shown to me by M. Duboscque Soleil, and the Abbé Moigno, in Paris in the month of October 1850. A prism and lenses were arranged to throw upon a screen an approximately pure spectrum of a vertical electric arc between charcoal poles of a powerful battery, the lower one of which was hollowed like a cup. When pieces of copper and pieces of zinc were separately thrown into the cup, the spectrum exhibited, in perfectly definite positions, magnificent well-marked bands of different colours characteristic of the two metals. When a piece of brass, compounded of copper and zinc, was put into the cup, the spectrum showed all the bands, each precisely in the place in which it had been seen when one metal or the other had been used separately.

It is much to be regretted that this great generalisation was not published to the world twenty years ago. I say this, not because it is to be regretted that Angström should have the credit of having in 1853 published independently the statement that an "incandescent gas emits luminous rays of the same refrangibility as those which it can absorb"; or that Balfour Stewart should have been unassisted by it when, coming to the subject from a very different point of view, he made, in his extension of the "Theory of Exchanges," * the still wider generalisation that the radiating power of every kind of substance is equal to its absorbing power for every kind of ray; or that Kirchhoff also should have in 1859 independently discovered the same proposition, and shown its

* Edin. Transactions, 1858-59.

application to solar and stellar chemistry; but because we might now be in possession of the inconceivable riches of astronomical results which we expect from the next ten years' investigation by spectrum analysis, had Stokes given his theory to the world when it first occurred to him.

To Kirchhoff belongs, I believe, solely the great credit of having first actually sought for and found other metals than sodium in the sun by the method of spectrum analysis. His publication of October 1859 inaugurated the practice of solar and stellar chemistry, and gave spectrum analysis an impulse to which in a great measure is due its splendidly successful cultivation by the labours of many able investigators within the last ten years.

To prodigious and wearing toil of Kirchhoff himself, and of Angström, we owe large-scale maps of the solar spectrum, incomparably superior in minuteness and accuracy of delineation to anything ever attempted previously. These maps now constitute the standards of reference for all workers in the field. Plücker and Hittorf opened ground in advancing the physics of spectrum analysis, and made the important discovery of changes in the spectra of ignited gases produced by changes in the physical condition of the gas. The scientific value of the meetings of the British Association is well illustrated by the fact that it was through conversation with Plücker at the Newcastle meeting that Lockyer was first led into the investigation of the effects of varied pressure on the quality of the light emitted by glowing gas which he and Frankland have prosecuted with such admirable success. Scientific wealth tends to accumulation according to the law of compound interest. Every addition to knowledge of properties of matter supplies the naturalist with new instrumental means for discovering and interpreting phenomena of nature, which in their turn afford foundations for fresh generalisations, bringing gains of permanent value into the great storehouse of philosophy. Thus Frankland, led, from observing the want of brightness of a candle burning in a tent on the summit of Mont Blanc, to scrutinise Davy's theory of flame, discovered that brightness without incandescent solid particles is given to a purely gaseous flame by augmented pressure, and that a dense ignited gas gives a spectrum comparable with that of the light from an incandescent solid or liquid. Lockyer joined him; and the two found that every incandescent substance gives a continuous spectrum—that an incandescent gas under varied pressure gives bright bars across the continuous spectrum, some of which, from the sharp, hard and fast lines observed where the gas is in a state of extreme attenuation, broaden out on each side into nebulous bands as the density is increased, and are ultimately lost in the continuous spectrum when the condensation is pushed on till the gas becomes a fluid no longer to be called gaseous. More recently they have examined the influence of temperature, and have obtained results which seemed to show that a highly attenuated gas, which at a high temperature gives several bright lines, gives a smaller and smaller number of lines, of sufficient brightness to be visible, when the temperature is lowered, the density being kept unchanged. I cannot refrain here from remarking how admirably this beautiful investigation harmonises with Andrews's great discovery of continuity between the gaseous and liquid states. Such things make the life-blood of science. In contemplating them we feel as if led out from narrow waters of scholastic dogma to a refreshing excursion on the broad and deep ocean of truth, where we learn from the wonders we see that there are endlessly more and more glorious wonders still unseen.

Stokes's dynamical theory supplies the key to the philosophy of Frankland and Lockyer's discovery. Any atom of gas when struck and left to itself vibrates with perfect purity its fundamental note or notes. In a highly attenuated gas each atom is very rarely in collision with other atoms, and therefore is nearly at all times in a state of true vibration. Hence the spectrum of a highly attenuated gas consists of one or more perfectly sharp bright lines, with a scarcely perceptible continuous gradation of prismatic colour. In denser gas each atom is frequently in collision, but still is for much more time free, in intervals between collisions, than engaged in collision; so that not only is the atom itself thrown sensibly out of tune during a sensible proportion of its whole time, but the confused jangle of vibrations in every variety of period during the actual collision becomes more considerable in its influence. Hence bright lines in the spectrum broaden out somewhat, and the continuous spectrum becomes less faint. In still denser gas each atom may be almost as much time in collision as free, and the spectrum then consists of broad nebulous bands crossing a continuous spectrum of considerable brightness. When the medium is so dense that each atom is always in collision, that is to say never free from influence of its neighbours, the spectrum will generally be continuous, and may present little or no appearance of bands, or even of maxima of brightness. In this condition the fluid can be no longer regarded as a gas, and we must judge of its relation to the vaporous or liquid states according to the critical conditions discovered by Andrews.

While these great investigations of properties of matter were going on, naturalists were not idle with the newly recognised power of the spectroscope at their service. Chemists soon followed the example of Bunsen in discovering new metals in terrestrial matter by the old blow-pipe and prism test of Fox Talbot and Herschel. Biologists applied spectrum analysis to animal and vegetable chemistry, and to sanitary investigations. But it is in astronomy that spectroscopic research has been carried on with the greatest activity, and been most richly rewarded with results. The chemist and the astronomer have joined their forces. An astronomical observatory has now, appended to it, a stock of reagents such as hitherto was only to be found in the chemical laboratory. A devoted corps of volunteers of all nations, whose motto might well be *Ubique*, have directed their artillery to every region of the universe. The sun, the spots on his surface, the corona and the red and yellow prominences seen round him during total eclipses, the moon, the planets, comets, auroras, nebulæ, white stars, yellow stars, red stars, variable and temporary stars, each tested by the prism, was compelled to show its distinguishing prismatic colours. Rarely before in the history of science has enthusiastic perseverance directed by penetrative genius produced within ten years so brilliant a succession of discoveries. It is not merely the *chemistry* of sun and stars, as first suggested, that is subjected to analysis by the spectroscope. Their whole laws of being are now subjects of direct investigation; and already we have glimpses of their evolutional history through the stupendous power of this most subtle and delicate test. We had only solar and stellar chemistry; we now have solar and stellar physiology.

It is an old idea that the colour of a star may be influenced by its motion relatively to the eye of the spectator, so as to be tinged with red if it moves from the earth, or blue if it moves towards the earth. William Allen Miller, Huggins, and Maxwell showed how, by aid of the spectroscope, this idea may be made the foundation of a method of measuring the relative velocity with which a star approaches to or recedes from the earth. The principle is, first to identify, if possible, one or more of the lines in the spectrum of the star, with a line or lines in the spectrum of sodium, or some other terrestrial substance, and then (by observing the star and the artificial light simultaneously by the same spectroscope) to find the difference, if any, between their refrangibilities. From this difference of refrangibility the ratio of the periods of the two lights is calculated, according to data determined by Fraunhofer from comparisons between the positions of the dark lines in the prismatic spectrum and in his own "interference spectrum" (produced by substituting for the prism a fine grating). A first comparatively rough application of the test by Miller and Huggins to a large number of the principal stars of our skies, including Aldebaran, α Orionis, β Pegasi, Sirius, α Lyræ, Capella, Arcturus, Pollux, Castor (which they had observed rather for the chemical purpose than for this), proved that not one of them had so great a velocity as 315 kilometres per second to or from the earth, which is a *most momentous result in respect of cosmical dynamics.* Afterwards Huggins made special observations of the velocity test, and succeeded in making the measurement in one case, that of Sirius, which he then found to be receding from the earth at the rate of 66 kilometres per second. This, corrected for the velocity of the earth at the time of the observation, gave a velocity of Sirius, relative to the Sun, amounting to 47 kilometres per second. The minuteness of the difference to be measured, and the smllness ofa the amount of light, even when the brightest star is observed, renders the observation extremely difficult. Still, with such great skill as Mr. Huggins has brought to bear on the investigation, it can scarcely be doubted that velocities of many other stars may be measured. What is now wanted is, certainly not greater skill, perhaps not even more powerful instruments, but *more instruments and more observers.* Lockyer's applications of the velocity test to the relative motions of different gases in the Sun's photosphere, spots, chromosphere, and chromospheric prominences, and his observations of the varying spectra presented by the same substance as it moves from one position to another in the Sun's atmosphere, and his interpretation of these observations according to the laboratory results of Frankland and himself, go far towards confirming the conviction that in a few years all the marvels of the Sun will be dynamically explained according to known properties of matter.

During six or eight precious minutes of time, spectroscopes have been applied to the solar atmosphere and to the corona seen round the dark disc of the moon eclipsing the sun. Some of the wonderful results of such observations, made in India on the occasion of the eclipse of August 1868, were described by Prof. Stokes in a previous address. Valuable results have, through the liberal assistance given by the British and American Governments, been obtained also from the total eclipse of last December, notwithstanding a generally unfavourable condition of weather. It seems to have been proved that at least some sensible part of the light of the "corona" is a terrestrial atmospheric halo or dispersive reflection of the light of the glowing hydrogen and "helium"* round the sun I believe I may say on the present occasion when preparation must again be made to utilise a Total Eclipse of the sun, that the British Association confidently trusts to our Government exercising the same wise liberality as heretofore in the interests of science.

The old nebular hypothesis supposes the solar system and other similar systems through the universe which we see at a distance as stars, to have originated in the condensation of fiery nebulous matter. This hypothesis was invented before the discovery of thermo-dynamics, or the nebulæ would not have been supposed to be fiery; and the idea seems never to have occurred to any of its inventors or early supporters that the matter, the condensation of which they supposed to constitute the Sun and stars, could have been other than fiery in the beginning. Mayer first suggested that the heat of the Sun may be due to gravitation; but he supposed meteors falling in to keep always generating the heat which is radiated year by year from the Sun. Helmholtz, on the other hand, adopting the nebular hypothesis, showed in 1854 that it was not necessary to suppose the nebulous matter to have been originally fiery, but that mutual gravitation between its parts may have generated the heat to which the present high temperature of the Sun is due. Further he made the important observations that the potential energy of gravitation in the Sun is even now far from exhausted; but that with further and further shrinking more and more heat is to be generated, and that thus we can conceive the Sun even now to possess a sufficient store of energy to produce heat and light, almost as at present, for several million years of time future. It ought, however, to be added that this condensation can only follow from cooling, and therefore that Helmholtz's gravitational explanation of future Sun-heat amounts really to showing that the Sun's thermal capacity is enormously greater, in virtue of the mutual gravitation between the parts of so enormous a mass, than the sum of the thermal capacities of separate and smaller bodies of the same material and the same total mass. Reasons for adopting this theory, and the consequences which follow from it, are discussed in an article "On the Age of the Sun's Heat," published in *Macmillan's Magazine* for March, 1862.

For a few years Mayer's theory of solar heat had seemed to me probable; but I had been led to regard it as no longer tenable, because I had been in the first place driven, by consideration of the very approximate constancy of the Earth's period of revolution round the Sun for the last 2000 years, to conclude that "The principal source, perhaps the sole appreciably effective "source of Sun-heat, is in bodies circulating round the Sun at present inside "the Earth's orbit"†; and because Le Verrier's researches on the motion of the planet Mercury, though giving evidence of a sensible influence attributable to matter circulating as a great number of small planets within his orbit round the Sun, showed that the amount of matter that could possibly be assumed to circulate at any considerable distance from the Sun must be very small; and therefore "if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the Sun, within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to one-eighth of his radius. All things considered, there seems little probability in the hypothesis that solar radiation is compensated to any appreciable degree, by heat generated by meteors falling in, at present; and, as it can be shown that no chemical theory is tenable,* it must be concluded as most probable that the Sun is at present more an incandescent liquid mass cooling."†

Thus on purely astronomical grounds was I long ago led to abandon as very improbable the hypothesis that the Sun's heat is supplied dynamically from year to year by the influx of meteors. But now spectrum analysis gives proof finally conclusive against it.

Each meteor circulating round the Sun must fall in along a very gradual spiral path, and before reaching the Sun must have been for a long time exposed to an enormous heating effect from his radiation when very near, and must thus have been driven into vapour before actually falling into the Sun. Thus, if Mayer's hypothesis is correct, friction between vortices of meteoric vapours and the Sun's atmosphere must be the immediate cause of solar heat; and the velocity with which these vapours circulate round equatorial parts of the Sun must amount to 435 kilometres per second. The spectrum test of velocity applied by Lockyer showed but a twentieth part of this amount as the greatest observed relative velocity between different vapours in the Sun's atmosphere.

At the first Liverpool meeting of the British Association (1854), in advancing a gravitational theory to account for all the heat, light, and motions of the universe, I urged that the immediately antecedent condition of the matter of which the Sun and Planets were formed, not being fiery, could not have been gaseous; but that it probably was solid, and may have been like the meteoric stones which we still so frequently meet with through space. The discovery of Huggins, that the light of the Nebulæ, so far as hitherto sensible to us, proceeds from incandescent hydrogen and nitrogen gases, and that the heads of comets also give us light of incandescent gas, seems at first sight literally to fulfil that part of the Nebular hypothesis to which I had objected. But a solution, which seems to me in the highest degree probable, has been suggested by Tait. He supposes that it may be by ignited gaseous exhalations proceeding from the collisions of meteoric stones that Nebulæ and the heads of Comets show themselves to us, and he suggested, at a former meeting of the Association, that experiments should be made for the purpose of applying spectrum analysis to the light which has been observed in gunnery trials, such as those at Shoeburyness, when iron strikes against iron at a great velocity, but varied by substituting for the iron various solid materials, metallic or stony. Hitherto this suggestion has not been acted upon; but surely it is one the carrying out of which ought to be promoted by the British Association.

Most important steps have been recently made towards the discovery of the nature of comets; establishing with nothing short of certainty the truth of a hypothesis which had long appeared to me probable,—that they consist of groups of meteoric stones;—accounting satisfactorily for the light of the nucleus; and giving a simple and rational explanation of phenomena presented by the tails of comets which had been regarded by the greatest astronomers as almost preternaturally marvellous. The meteoric hypothesis to which I have referred remained a mere hypothesis (I do not know that it was ever even published) until, in 1866, Schiaparelli calculated, from observations on the August meteors, an orbit for these bodies which he found to agree almost perfectly with the orbit of the great comet of 1862 as calculated by Oppolzer; and so discovered and demonstrated that a comet consists of a group of meteoric stones. Professor Newton, of Yale College, United States, by examining ancient records, ascertained that in periods of about thirty-three years, since the year 902, there have been exceptionally brilliant displays of the November meteors. It had long been believed that these interesting visitants came from a train of small detached planets circulating round the Sun all in nearly the same orbit, and constituting a belt analogous to Saturn's ring, and that the reason for the comparatively large number of meteors which we observe annually about the 14th of November is, that at that time the earth's orbit cuts through the supposed meteoric belt. Professor Newton concluded from his investigation that there is a denser part of the group of meteors which extends over a portion of the orbit so great as to occupy about one-tenth or one-fifteenth of the

* Frankland and Lockyer find the yellow prominences to give a very decided bright line not far from D, but hitherto not identified with any terrestrial flame. It seems to indicate a new substance, which they propose to call Helium.

† On the Mechanical Energies of the Solar System. Transactions of the Royal Society of Edinburgh, 1854; and Phil. Mag. 1854, second half year.

* "Mechanical Energies," &c.

† "Age of the Sun's Heat" (*Macmillan's Magazine*, March 1862).

periodic time in passing any particular point, and gave a choice of five different periods for the revolution of this meteoric stream round the sun, any one of which would satisfy his statistical result. He further concluded that the line of nodes, that is to say, the line in which the plane of the meteoric belt cuts the plane of the Earth's orbit, has a progressive sidereal motion of about 52″·4 per annum. Here, then, was a splendid problem for the physical astronomer; and, happily, one well qualified for the task, took it up. Adams, by the application of a beautiful method invented by Gauss, found that of the five periods allowed by Newton just one permitted the motion of the line of nodes to be explained by the disturbing influence of Jupiter, Saturn, and other planets. The period chosen on these grounds is 33¼ years. The investigation showed further that the form of the orbit is a long ellipse, giving for shortest distance from the Sun 145 million kilometres, and for longest distance 2,895 million kilometres. Adams also worked out the longitude of the perihelion and the inclination of the orbit's plane of the ecliptic. The orbit which he thus found agreed so closely with that of Tempel's Comet I. 1866 that he was able to identify the comet and the meteoric belt.* The same conclusion had been pointed out a few weeks earlier by Schiaparelli, from calculations by himself on data supplied by direct observations on the meteors, and independently by Peters from calculations by Leverrier on the same foundation. It is therefore thoroughly established that Temple's Comet I. 1866 consists of an elliptic train of minute planets, of which a few thousands or millions fall to the earth annually about the 14th of November, when we cross their track. We have probably not yet passed through the very nucleus or densest part; but thirteen times, in Octobers and Novembers, from October 13, A.D. 902 to November 14, 1866 inclusive (this last time having been correctly predicted by Prof. Newton), we have passed through a part of the belt greatly denser than the average. The densest part of the train, when near enough to us, is visible as the head of the comet. This astounding result, taken along with Huggins's spectroscopic observations on the light of the heads and tails of comets, confirms most strikingly Tait's theory of comets, to which I have already referred; according to which the comet, a group of meteoric stones, is self-luminous in its nucleus, on account of collisions among its constituents, while its "tail" is merely a portion of the less dense part of the train illuminated by sunlight, and visible or invisible to us according to circumstances, not only of density, degree of illumination, and nearness, but also of tactic arrangement, as of a flock of birds or the edge of a cloud of tobabco smoke! What prodigious difficulties are to be explained, you may judge from two or three sentences which I shall read from Herschel's Astronomy, and from the fact that even Schiaparelli seems still to believe in the repulsion. "There is, beyond question, some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the etherial or imponderable elements), may enable us ere long to penetrate this mystery, and to declare whether it is really *matter* in the ordinary anticipation of the term which is projected from their heads with such extraordinary velocity, and if not *impelled*, at least *directed*, in its course, by reference to the Sun, as its point of avoidance." †

"In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration than in that of the enormous sweep which it makes round the sun *in perihelio*, in the manner of a straight and rigid rod, *in defiance of the law of gravitation*, nay, even *of the received* laws of motion." *

"The projection of this ray . . . to so enormous a length, in a single day conveys an impression of the intensity of the forces acting to produce such a velocity of material transfer through space such as no other natural phenomenon is capable of exciting. It is clear that *if we have to deal here with matter, such as we conceive it*, viz., *possessing inertia—at all*, it must be under the dominion of forces incomparably more energetic than gravitation, and quite of a different nature." †

Think now of the admirable simplicity with which Tait's beautiful "sea-bird analogy," as it has been called, can explain all these phenomena.

The essence of science, as is well illustrated by astronomy and cosmical physics, consists in inferring antecedent conditions, and anticipating future evolutions, from phenomena which have actually come under observation. In biology, the difficulties of successfully acting up to this ideal are prodigious. The earnest naturalists of the present day are, however, not appalled or paralysed by them, and are struggling boldly and laboriously to pass out of the mere "Natural History stage" of their study, and bring Zoology within the range of Natural Philosophy. A very ancient speculation, still clung to by many naturalists (so much so that I have a choice of modern terms to quote in expressing it) supposes that, under meteorological conditions very different from the present, dead matter may have run together or crystallised or fermented into "germs of life," or "organic cells," or "protoplasm." But science brings a vast mass of inductive evidence against this hypothesis of spontaneous generation, as you have heard from my predecessor in the Presidential chair. Careful enough scrutiny has, in every case up to the present day, discovered life as antecedent to life. Dead matter cannot become living without coming under the influence of matter previously alive. This seems to me as sure a teaching of science as the law of gravitation. I utterly repudiate, as opposed to all philosophical uniformitarianism, the assumption of "different meteorological condition"—that is to say, somewhat different vicissitudes of temperature, pressure, moisture, gaseous atmosphere—to produce or to permit that to take place by force or motion of dead matter alone, which is a direct contravention of what seems to us biological law. I am prepared for the answer, "our code of biological law is an expression of our ignorance as well of our knowledge." And I say yes: search for spontaneous generation out of inorganic materials; let any one not satisfied with the purely negative testimony of which we have now so much against it, throw himself into the inquiry. Such investigations as those of Pasteur, Pouchet, and Bastian are among the most interesting and momentous in the whole range of Natural History, and their results, whether positive or negative, must richly reward the most careful and laborious experimenting. I confess to being deeply impressed by the evidence put before us by Professor Huxley, and I am ready to adopt, as an article of scientific faith, true through all space and through all time, that life proceeds from life, and from nothing but life.

How, then, did life originate on the Earth? Tracing the physical history of the Earth backwards, on strict dynamical principles, we are brought to a red-hot melted globe on which no life could exist. Hence when the Earth was first fit for life, there was no living thing on it. There were rocks solid and disintegrated, water, air all round, warmed and illuminated by a brilliant Sun, ready to become a garden. Did grass and trees and flowers spring into existence, in all the fulness of ripe beauty, by a fiat of Creative power? or did vegetation, growing up from seed sown, spread and multiply over the whole Earth? Science is bound, by the everlasting law of honour, to face fearlessly every problem which can fairly be presented to it. If a probable solution, consistent with the ordinary course of nature, can be found, we must not invoke an abnormal act of Creative Power. When a lava stream flows down the sides of Vesuvius or Etna it quickly cools and becomes solid; and after a few weeks or years it teems with vegetable and animal life, which for it originated by the transport of seed and ova and by the migration of individual living creatures. When a volcanic island springs up from the sea, and after a few years is found clothed with vegetation, we do not hesitate to assume that seed has been wafted to it through the air, or floated to it on rafts. Is it not possible, and if possible, is it not probable, that the beginning of vegetable life on the earth is to be similarly explained?

* Signor Schiaparelli, Director of the Observatory of Milan, who, in a letter dated 31st December, 1866, pointed out that the elements of the orbit of the August Meteors, calculated from the observed position of their radiant point on the supposition of the orbit being a very elongated ellipse agreed very closely with those of the orbit of Comet II 1862, calculated by Dr. Oppolzer. In the same letter Schiaparelli gives elements of the orbit of the November meteors, but these were not sufficiently accurate to enable him to identify the orbit with that of any known comet. On the 21st January, 1867, M. Leverrier gave more accurate elements of the orbit of the November Meteors, and in the "Astronomische Nachrichten" of January 9, Mr. C. F. W. Peters, of Altona, pointed out that these elements closely agreed with those of Tempel's Comet (I. 1866), calculated by Dr. Oppolzer, and on Feb. 2, Schiaparelli having recalculated the elements of the orbit of the meteors himself noticed the same agreement. Adams arrived quite independently at the conclusion that the orbit of 33¼ years period is the one which must be chosen out of the five indicated by Prof. Newton. His calculations were sufficiently advanced before the letters referred to appeared, to show that the other four orbits offered by Newton were inadmissible. But the calculations to be gone through to find the secular motion of the node in such an elongated orbit as that of the meteors, were necessarily very long, so that they were not completed till about March 1867. They were communicated in that month to the Cambridge Philosophical Society, and in the month following to the Astronomical Society.

† Herschel's Astronomy, § 599.

* Herschel's Astronomy, § 599. † *Ibid.*, 10th Edition, § 589.

Every year thousands, probably millions, of fragments of solid matter fall upon the Earth—whence came these fragments? What is the previous history of any one of them? Was it created in the beginning of time an amorphous mass? This idea is so unacceptable that, tacitly or explicitly, all men discard it. It is often assumed that all, and it is certain that some, meteoric stones are fragments which had been broken off from greater masses and launched free into space. It is as sure that collisions must occur between great masses moving through space as it is that ships, steered without intelligence directed to prevent collision, could not cross and recross the Atlantic for thousands of years with immunity from collisions. When two great masses come into collision in space it is certain that a large part of each is melted; but it seems also quite certain that in many cases a large quantity of *débris* must be shot forth in all directions, much of which may have experienced no greater violence than individual pieces of rock experience in a land-slip or in blasting by gunpowder. Should the time when this earth comes into collision with another body, comparable in dimensions to itself, be when it is still clothed as at present with vegetation, many great and small fragments carrying seed and living plants and animals would undoubtedly be scattered through space. Hence and because we all confidently believe that there are at present, and have been from time immemorial, many worlds of life besides our own, we must regard it as probable in the highest degree that there are countless seed-bearing meteoric stones moving about through space. If, at the present instant, no life existed upon this earth, one such stone falling upon it might, by what we blindly call natural causes, lead to its becoming covered with vegetation. I am fully conscious of the many scientific objections which may be urged against this hypothesis, but I believe them to be all answerable. I have already taxed your patience too severely to allow me to think of discussing any of them on the present occasion. The hypothesis that life originated on this earth through moss-grown fragments from the ruins of another world may seem wild and visionary; all I maintain is that it is not unscientific.

From the earth stocked with such vegetation as it could receive meteorically, to the earth teeming with all the endless variety of plants and animals which now inhabit it, the step is prodigious; yet, according to the doctrine of continuity, most ably laid before the Association by a predecessor in this chair (Mr. Grove), all creatures now living on earth have proceeded by orderly evolution from some such origin. Darwin concludes his great work on "The Origin of Species" with the following words:—"It is interesting to contemplate an entangled bank clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent on each other in so complex a manner, have all been produced by laws acting around us." . . . . "There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms, most beautiful and most wonderful, have been and are being evolved." With the feeling expressed in these two sentences I most cordially sympathise. I have omitted two sentences which come between them, describing briefly the hypothesis of "the origin of species by natural selection," because I have always felt that this hypothesis does not contain the true theory of evolution, if evolution there has been in biology. Sir John Herschel, in expressing a favourable judgment on the hypothesis of zoological evolution, with however, some reservation in respect to the origin of man, objected to the doctrine of natural selection, that it was too like the Laputan method of making books, and that it did not sufficiently take into account a continually guiding and controlling intelligence. This seems to me a most valuable and instructive criticism. I feel profoundly convinced that the argument of design has been greatly too much lost sight of in recent zoological speculations. Reaction against the frivolities of teleology, such as are to be found, not rarely, in the notes of the learned Commentators on Paley's "Natural Theology," has I believe had a temporary effect in turning attention from the solid and irrefragable argument so well put forward in that excellent old book. But overwhelmingly strong proofs of intelligent and benevolent design lie all around us, and if ever perplexities, whether metaphysical or scientific, turn us away from them for a time, they come back upon us with irresistible force, showing to us through nature the influence of a free will, and teaching us that all living beings depend on one ever-acting Creator and Ruler.

## SECTION A.

### MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, PROF. P. G. TAIT, M.A.

IN opening the proceedings of this Section my immediate predecessors have exercised their ingenuity in presenting its widely different component subjects from their several points of view, and in endeavouring to coordinate them. What they were obliged to leave unfinished, it would be absurd in me to attempt to complete. It would be impossible, also, in the limits of a brief address, to give a detailed account of the recent progress of physical and mathematical knowledge. Such a work can only be produced by separate instalments, each written by a specialist, such as the admirable "Reports" which form from time to time the most valuable portions of our annual volume.

I shall therefore confine my remarks in the main to those two subjects, one in the mathematical, the other in the purely physical, division of our work, which are comparatively familiar to myself. I wish, if possible, to induce, ere it be too late, native mathematicians to pay much more attention than they have yet paid to Hamilton's magnificent Calculus of Quaternions, and to call the particular notice of physicists to our President's grand Principle of Dissipation of Energy. I think that these are, at this moment, the most important because the most promising parts of our field.

If nothing more could be said for Quaternions than that they enable us to exhibit in a singularly compact and elegant form, whose meaning is obvious at a glance on account of the utter inartificiality of the method, results which in the ordinary Cartesian coordinates are of the utmost complexity, a very powerful argument for their use would be furnished. But it would be unjust to Quaternions to be content with such a statement; for we are fully entitled to say that in *all* cases, even in those to which the Cartesian methods seem specially adapted, they give as simple an expression as any other method; while in the great majority of cases they give a vastly simpler one. In the common methods a judicious choice of coordinates is often of immense importance in simplifying an investigation; in Quaternions there is usually *no choice*, for (except when they degrade to mere scalars) they are in general utterly independent of any particular directions in space, and select of themselves the most natural reference lines for each particular problem. This is easily illustrated by the most elementary instances, such as the following:—The general equation of Cones involves merely the *direction* of the vector of a point, while that of Surfaces of Revolution is a relation between the *lengths* of that vector and of its resolved part parallel to the axis, and Quaternions enable us by a mere mark to separate the ideas of length and direction without introducing the cumbrous and clumsy square roots of sums of squares which are otherwise necessary.

But, as it seems to me that mathematical methods should be specially valued in this Section as regards their fitness for physical applications, what can possibly from that point of view be more important than Hamilton's $\nabla$? Physical analogies have often been invoked to make intelligible various mathematical processes. Witness the case of Statical Electricity, wherein Thomson has by the analogy of Heat-conduction, explained the meaning of various important theorems due to Green, Gauss, and others; and wherein Clerk-Maxwell has employed the properties of an imaginary incompressible liquid (devoid of inertia) to illustrate not merely these theorems, but even Thomson's Electrical Images. [In fact he has gone much further, having applied his analogy to the puzzling combinations presented by Electrodynamics.] There can be little doubt that these comparisons owe their birth to the small intelligibility, *per se*, of what has been called Laplace's Operator, $\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}$, which appears alike in all theories of attraction at a distance, in the steady flow of heat in a conductor, and in the steady motion of incompressible fluids. But when we are taught to understand the operator itself, we are able to dispense with these analogies, which, however, valuable and beautiful, have certainly to be used with extreme caution, as tending very often to confuse and mislead. Now Laplace's operator is merely the negative of the *square* of Hamilton's $\nabla$, which is perfectly intelligible in itself and in all its combinations; and can be defined as giving the vector-rate of most rapid increase of any scalar function to which it is applied—giving, for instance, the vector-force from a potential, the heat-flux from a distribution of temperature, &c. Very simple functions of the same operator give the rate of increase of a quantity in any assigned direction, the condensation and elementary rotation produced by given displacements of the parts of a system, &c. For instance, a very elementary application of $\nabla$ to the theory of attraction enables us to put one of its fundamental principles in the following extremely suggestive form :—If the displacement or velocity of each particle of a medium represent in magnitude and direction the electric force at that particle, the corresponding statical distribution of electricity is proportional everywhere to the condensation produced. Again, Green's celebrated theorem is at once seen to be merely the well-known equation of continuity expressed for a heterogeneous fluid, whose density at every point is proportional to one electric potential, and its displacement or velocity proportional to and in the direction of the electric force due to another potential. But this is not the time to pursue such an inquiry, for it would lead me at once to discussions as to the possible nature of electric phenomena and of gravitation. I believe myself to be fully justified in saying that, were the theory of this operator thoroughly developed and generally known, the whole mathematical treatment of such physical questions as those just mentioned would undergo an immediate and enormous simplification ; and this, in its turn, would be at once followed by a proportionately large extension of our knowledge.*

And this is but *one* of the claims of Quaternions to the attention of physicists. When we come to the important questions of stress and strain in an elastic solid, we find again that all the elaborate and puzzling machinery of coordinates commonly employed can be at once comprehended and kept out of sight in a mere single symbol—a linear and vector function, which is self-conjugate if the strain be pure. This is simply, it appears to me, a proof either that the elaborate machinery ought never to have been introduced, or that its use was an indication of a comparatively savage state of mathematical civilisation. In the motion of a rigid solid about a fixed point, a Quaternion, represented by a single symbol which is a function of the time, gives us the operator which could bring the body by a single rotation from its initial position to its position at any assigned instant. In short, whenever with our usual means a result can be obtained in, or after much labour reduced to, a single form, Quaternions will give it at once in that form ; so that nothing is ever *lost* in point of simplicity. On the other hand, in numberless cases the Quaternion result is immeasurably simpler and more intelligible than any which can be obtained or even *expressed* by the usual methods. And it is not to be supposed that the modern Higher Algebra, which has done so much to simplify and extend the ordinary Cartesian methods, would be ignored by the general employment of Quaternions ; on the contrary, Determinants, Invariants, &c., present themselves in almost every Quaternion solution, and in forms which have received the full benefit of that simplification which Quaternions generally produce. Comparing a Quaternion investigation, no matter in what department, with the equivalent Cartesian one, even when the latter has availed itself to the utmost of the improvements suggested by Higher Algebra, one can hardly help making the remark that they contrast even more strongly than the decimal notation with the binary scale, or with the old Greek Arithmetic—or than the well-ordered subdivisions of the metrical system with the preposterous no-systems of Great Britain, a mere fragment of which (in the form of Table of Weights and Measures) form, perhaps the most effective, if not the most ingenious, of the many instruments of torture employed in our elementary teaching.

It is true that, in the eyes of the pure mathematician, Quaternions have one grand and fatal defect. They cannot be applied to space of $n$ dimensions, they are contented to deal with those poor three dimensions in which mere mortals are doomed to dwell, but which cannot bound the limitless aspirations of a Cayley or a Sylvester. From the physical point of view this, instead of a defect, is to be regarded as the greatest possible recommendation. It shows, in fact, Quaternions to be a special instrument so constructed for application to the *Actual* as to have thrown overboard everything which is not absolutely necessary, without the slightest consideration whether or no it was thereby being rendered useless for applications to the *Inconceivable.*

The late Sir John Herschel was one of the first to perceive the value of Quaternions : and there may be present some who remember him, at a British Association meeting not long after their invention, characterising them as a "Cornucopia from which, turn it how you will, something valuable is sure to fall." Is it not strange, to use no harsher word, that such a harvest has hitherto been left almost entirely to Hamilton himself? If but half a dozen tolerably good mathematicians, such as exist in scores in this country, were seriously to work at it, instead of spending (or rather wasting) their time, as so many who have the requisite leisure now do, in going over again what has been already done, or in working out mere details where a grand theory has been sketched, a very great immediate advance would be certain. From the majority of the papers in our few mathematical journals, one would almost be led to fancy that British mathematicians have too much pride to use a simple method while an unnecessarily complex one can be had. No more telling example of this could be wished for than the insane delusion under which they permit Euclid to be employed in our elementary teaching. They seem voluntarily to weight alike themselves and their pupils for the race ; and a cynic might, perhaps without much injustice, say they do so that they may have mere self-imposed and avoidable difficulties to face instead of the new, real, and dreaded ones (belonging to regions hitherto unpenetrated) with which Quaternions would too soon enable them to come into contact. But this game will certainly end in disaster. As surely as mathematics came to a relative stand-still in this country for nearly a century after Newton, so surely will it do so again if we leave our eager and watchful rivals abroad to take the initiative in developing the grand method of Hamilton. And it is not alone French and Germans whom we have now to dread, Russia, America, regenerated Italy, and other nations, are all fairly entered for the contest.

The flights of the imagination which occur to the pure mathematician are in general so much better described in his formulæ than in words, that it is not remarkable to find the subject treated by outsiders as something essentially cold and uninteresting—while even the most abstruse branches of physics, as yet totally incapable of being popularised, attract the attention of the uninitiated. The reason may perhaps be sought in the fact that, while perhaps the only successful attempt to invest mathematical reasoning with a halo of glory—that made in this section by Prof. Sylvester—is known to a comparative few, several of the highest problems of physics are connected with those observations which are possible to the many. The smell of lightning has been observed for thousands of years, it required the sagacity of Schönbein to trace it to the formation of Ozone. Not to speak of the (probably fabulous) apple of Newton—what enormous consequences did he obtain by passing light through a mere wedge of glass, and by simply laying a lens on a flat plate! The patching of a trumpery model led Watt to his magnificent inventions. As children at the sea-shore playing with a "roaring buckie," or in later life lazily puffing out rings of tobacco-smoke, we are illustrating two of the splendid researches of Helmholtz. And our President, by the bold, because simple, use of reaction, has eclipsed even his former services to the Submarine Telegraph, and given it powers which but a few years ago would have been deemed unattainable.

In Experimental Physics our case is not hopeless, perhaps not as yet even alarming. Still something of the same kind may be said in this as in pure Mathematics. If Thomson's Theory of Dissipation, for instance, be not speedily developed in this country, we shall soon learn its consequences from abroad. The grand test of our science, the proof of its being a reality and not a mere inventing of new terms and squabbling as to what they shall mean, is that it is ever advancing. There is no standing still ; there is no running round and round as in a beaten donkey-track, coming back at the end of a century or so into the old posi-

* The following extracts from letters of Sir W. R. Hamilton have a perfectly general application, so that I do not hesitate to publish them :—"De Morgan was the very *first* person to *notice* the Quaternions *in print*; namely in a paper on Triple Algebra, in the Camb. Phil. Trans. of 18 4 It was, I think, about that time, or not very long afterwards, that he wrote to me, nearly as follows :—'I suspect, Hamilton, that you have *caught the right sow by the ear!*' Between us, dear Mr. Tait, I think *we* shall *begin* the SHEARING of it!" "You might without offence to me, consider that I abused the licence of *hope*, which may be indulged to an inventor, if I were to confess that I expect the Quaternions to supply, hereafter, not merely *mathematical methods*, but also *physical suggestions.* And, in particular, you are quite welcome to smile, if I say that it does not seem extravagant to *me* to suppose that a *full* possession of those *a priori principles* of mine, about the *multiplication of vectors*:—including the Law of the Four Scales, and the conception of the Extra-spatial Unit—which have as yet been not much more than *hinted* to the public—MIGHT have led (I do not at all mean that *in my hands* they ever *would* have done so) to an ANTICIPATION of the great discovery of OERSTED."

"It appears to me that one, and not the least, of the services which Quaternions may be expected to do to mathematical analysis generally, is that their introduction will *compel* those who adopt them,—or even who admit that they *may* be reasonably adopted by other persons—to consider, or to admit, that others may usefully inquire, *what common grounds* can be established, for *conclusions common to Quaternions* and to older branches of mathematics."

"*Could* anything be simpler, or more satisfactory? Don't you *feel*, as well as think, that we are on a *right track*, and shall be *thanked* hereafter? Never mind when."

tions, and fighting the self-same battles under slightly different banners, which is merely another form of stagnation (Kinetic Stability in fact). "A little folding of the hands to sleep" in chuckling satisfaction at what has been achieved of late years by our great experimenters—and we shall be left hopelessly behind. The sad fate of Newton's successors ought ever to be a warning to us. Trusting to what he had done, they allowed mathematical science almost to die out in this country, at least as compared with its immense progress in Germany and France. It required the united exertions of late Sir J. Herschel and many others to render possible in these islands a Boole and a Hamilton. If the successors of Davy and Faraday pause to ponder even on *their* achievements, we shall soon be again in the same state of ignominious inferiority. Who will then step in to save us?

Even as it is, though we have among us many names quite as justly great as any that our rivals can produce, we have also (even in our educated classes) such an immense amount of ignorance and consequent credulity, that it seems matter for surprise that true science is able to exist. Spiritualists, Circle-squarers, Perpetual-motionists, believers that the earth is flat and that the moon has no rotation, swarm about us. They certainly multiply much faster than do genuine men of science. This is characteristic of all inferior races, but it is consolatory to remember that in spite of it these soon become extinct. Your quack has his little day, and disappears except to the antiquary. But in science nothing of value can ever be lost; it is certain to become a stepping-stone on the way to further truth. Still, when our stepping-stones are laid, we should not wait till others employ them. "Gentlemen of the Guard, be kind enough to fire first" is a courtesy entirely out of date; with the weapons of the present day it would be simply suicide.

There is another point which should not be omitted in an address like this. For obvious reasons I must speak of the general question only, not venturing on examples, though I could give many telling ones. Even among our greatest men of science in this country, there is comparatively little knowledge of what has been already achieved, except, of course, in the one or more special departments cultivated by each individual. There can be little doubt that one cause at least of this is to be sought in the extremely meagre interest which our statesmen, as a rule, take in scientific progress. While abroad we find half a dozen professors teaching parts of the same subject in one University—each having therefore reasonable leisure—with us one man has to do the whole, and to endeavour as he best can to make something out of his very few spare moments. Along with this, and in great part due to it, there is often found a proneness to believe that what seems evident to the thinker cannot but have been long known to others. Thus the credit of many valuable discoveries is lost to Britain because her philosophers, having no time to spare, do not know that they are discoveries. The scientific men of other nations are, as a rule, better informed (certainly far better encouraged, and less over-worked), and perhaps likewise are not so much given to self-depreciation. Until something resembling the "Fortschritte der Physik," but in an improved form, and published at smaller intervals and with much less delay, is established in this country, there is little hope of improvement in this respect. Why should science be imperfectly summarised in little haphazard scraps here and there, when mere property has its elaborate series of money articles and exact broker's share lists? Such a work would be very easy of accomplishment; we have only to begin boldly—we do not need to go back, for in every year good work is being done at almost every part of the boundary between, as it were, the cultivated land and the still unpenetrated forest—enough at all events to show with all necessary accuracy whereabouts that boundary lies.

There is no need to enter here on the question of Conservation of Energy. It is thoroughly accepted by scientific men, and has revolutionised the greater part of physics. The facts as to its history also are generally agreed upon, but differences of a formidable kind exist as to the deductions to be drawn from them. These are matters, however, which will be more easily disposed of thirty years hence than now. The Transformation of Energy is also generally accepted, and, in fact, under various unsatisfactory names was almost popularly known before the Conservation of Energy was known in its entirety to more than a very few. But the Dissipation of Energy is by no means well known—and many of the results of its legitimate application have been received with doubt, sometimes even with attempted ridicule. Yet it appears to be at the present moment by far the most promising and fertile portion of Natural Philosophy; having obvious applications of which as yet only a small percentage appear to have been made. Some indeed were made before the enunciation of the principle, and have since been recognised as instances of it. Of such we have good examples in Fourier's great work on Heat-conduction, in the optical theorem that an image can never be brighter than the object, in Gauss's mode of investigating electrical distribution, and in some of Thomson's theorems as to the energy of an electromagnetic field. But its discoverer has, so far as I know, as yet confined himself in its explicit application to questions of Heat-conduction and Restoration of Energy, Geological Time, the Earth's Rotation, and such like. Unfortunately his long-expected Rede Lecture has not yet been published, and its contents (save to those who were fortunate enough to hear it) are still almost entirely unknown.

But there can be little question that the Principle contains implicitly the whole theory of Thermo-electricity, of Chemical Combination, of Allotropy, of Fluorescence, &c., and perhaps even of matters of a higher order than common physics and chemistry. In Astronomy it leads us to the grand question of the *age*, or perhaps more correctly the *phase of life*, of a star or nebula, shows us the material of potential suns, other suns in the process of formation, in vigorous youth, and in every stage of slowly protracted decay. It leads us to look on each planet and satellite as having been at one time a tiny sun, a member of some binary or multiple group, and even now (when almost deprived, at least at its surface, of its original energy) presenting an endless variety of subjects for the application of its methods. It leads us forward in thought to the far-distant time when the materials of the present stellar system shall have lost all but their mutual potential energy, but shall in virtue of it form the materials of future larger suns with their attendant planets. Finally, as it alone is able to lead us, by sure steps of deductive reasoning, to the necessary future of the universe—necessary, that is, if physical laws for ever remain unchanged—so it enables us distinctly to say that the present order of things has *not* been evolved through infinite past time by the agency of laws now at work—but must have had a distinctive beginning, a state beyond which we are totally unable to penetrate, a state in fact which must have been produced by other than the now acting causes.

Thus also, it is possible that in Physiology it may ere long lead to results of a different and much higher order of novelty and interest than those yet obtained, immensely valuable though they certainly are.

It was a grand step in science which showed that just as the consumption of fuel is necessary to the working of a steam-engine, or to the steady light of a candle, so the living engine requires food to supply its expenditure in the forms of muscular work and animal heat. Still grander was Rumford's early anticipation that the animal is a more economic engine than any lifeless one we can construct. Even in the explanation of this there is involved a question of very great interest, still unsolved, though Joule and many other philosophers of the highest order have worked at it. Joule has given a suggestion of great value, viz., that the animal resembles an electromagnetic rather than a heat-engine; but this throws us back again upon our difficulties as to the nature of electricity. Still, even supposing this question fully answered, there remains another—perhaps the highest which the human intellect is capable of directly attacking, for it is simply preposterous to suppose that we shall ever be able to understand scientifically the source of consciousness and volition, not to speak of loftier things—there remains the question of Life. Now it may be startling to some of you, especially if you have not particularly considered the matter, to hear it surmised that possibly we may, by the help of physical principles, especially that of the Dissipation of Energy, some time attain to a notion of what constitutes Life—mere Vitality I repeat, nothing higher. If you think for a moment of the vitality of a plant or a zoophyte, the remark, perhaps, will not appear so strange after all. But do not fancy that the Dissipation of Energy to which I refer is at all that of a watch or such-like piece of mere human mechanism, dissipating the low and common form of energy of a single coiled spring. It must be such that every little part of the living organism has its own store of energy constantly being dissipated, and as constantly replenished from external sources drawn upon by the whole arrangement in their harmonious working together. As an illustration of my meaning, though an extremely inadequate one, suppose Vaucanson's Duck to have been made up of excessively small parts, each microscopically constructed as perfectly as was the comparatively coarse whole, we should have had something barely distinguishable save by want of instincts from the living model. But let no one imagine that, should we ever penetrate this mystery, we shall thereby be enabled to produce, except from life, even the lowest form of life. Our President's splendid suggestion of Vortex-atoms, if it be correct, will enable us thoroughly to understand matter, and mathematically to investigate all its properties. Yet its very basis implies the absolute necessity of an intervention of Creative Power to form or to destroy one atom even of dead matter. The question really stands thus: Is Life physical or no? For if it be in any sense, however slight or restricted, physical, it is to that extent a subject for the Natural Philosopher, and for him alone. It would be entirely out of place for me to discuss such a question as this now and here; I have introduced it merely that I may say a word or two about what has been so often and so persistently croaked against the British Association, viz. that it tends to develope what are called Scientific Heresies. No doubt such charges are brought more usually against other Sections than against this; but Section A has not been held blameless. It seems to me that the proper answer to all such charges will be very simply and easily given, if we merely show that in our reasonings from observation and experiment we invariably confine our physical conclusions strictly to matter and energy (things which we can weigh and measure) in their multiform combinations. Excepting that which is obviously purely mathematical, whatever is certainly neither matter nor energy, nor dependent upon these, *is not a subject to be discussed here*, even by implication. All our reasonings in Physics *must*, so far as we know, be based upon the assumption founded on experience, that in the universe, whatever be the epoch or the locality, under exactly similar circumstances exactly similar results will be obtained. If this be not granted there is an end of Physical Science, or rather, there never could have been such a Science.* To use the word "Heresy" with reference to purely physical reasonings about Geological Time, or matters of that kind, is nowadays a piece of folly from which even Galileo's judges, were they alive, would shrink, as calculated to damage none but themselves and the cause which of old they, according to their lights, very naturally maintained.

There must always be wide limits of uncertainty (unless we choose to look upon Physics as a necessarily finite Science) concerning the exact boundary between the Attainable and the Unattainable. One herd of ignorant people, with the sole *prestige* of rapidly increasing numbers, and with the adhesion of a few fanatical deserters from the ranks of Science, refuse to admit that all the phenomena even of ordinary dead matter are strictly and exclusively in the domain of physical science. On the other hand, there is a numerous group, not in the slightest degree entitled to rank as Physicists—though in general they assume the proud title of Philosophers—who assert that not merely Life, but even Volition and Consciousness are mere physical manifestations. These opposite errors, into neither of which it is possible for a genuine scientific man to fall, so long at least as he retains his reason, are easily seen to be very closely allied. They are both to be attributed to that Credulity which is characteristic alike of Ignorance and of Incapacity. Unfortunately there is no cure—the case is hopeless—for great ignorance almost necessarily presumes incapacity, whether it shows itself in the comparatively harmless folly of the Spiritualist, or in the pernicious nonsense of the Materialist.

Alike condemned and contemned, we leave them to their proper fate—oblivion; but still we have to face the question:—where to draw the line between that which is physical and that which is utterly beyond physics. And again, our answer is —Experience alone can tell us; for experience is our only possible guide. If we attend earnestly and honestly to its teachings, we shall never go far astray. Man has been left to the resources of his intellect for the discovery not merely of physical laws, but of how far he is capable of comprehending them. And our answer to those who denounce our legitimate studies as heretical is simply this:—A revelation of anything which we can discover for ourselves, by studying the ordinary course of nature, would be an absurdity.

A profound lesson may be learned from one of the earliest little papers of our President, published while he was an undergraduate at Cambridge, where he shows that Fourier's magnificent treatment of the Conduction of Heat leads to formulæ for its distribution which are intelligible (and of course capable of being fully verified by experiment) for all time future, but which, except in particular cases, when extended to time past, remain intelligible for a finite period only, and then indicate a state of things which could not have resulted under known laws from any conceivable previous distribution. So far as heat is concerned, modern investigations have shown that a previous distribution of the matter involved may, by its potential energy, be capable of producing such a state of things at the moment of its aggregation; but the example is now adduced, not for its bearing on heat alone, but as a simple illustration of the fact that all portions of our science, and especially that beautiful one the Dissipation of Energy, point unanimously to a beginning, to a state of things incapable of being derived by present laws from any conceivable previous arrangement.

I conclude by quoting some noble words used by Stokes in his Address at Exeter, words which should be stereotyped for every meeting of this Association:—"When from the phenomena of life we pass on to those of mind, we enter a region still more profoundly mysterious. . . . Science can be expected to do but little to aid us here, since the instrument of research is itself the object of investigation. It can but enlighten us as to the depth of our ignorance, and lead us to look to a higher aid for that which most nearly concerns our well-being."

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## SECTION B.

### CHEMICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, DR. ANDREWS, F.R.S.

AMIDST the vicissitudes to which scientific theories are liable, it was scarcely to be expected that the discarded theory of phlogiston should be resuscitated in our day, and connected with one of the most important generalisations of modern science. The phlogistic theory, elaborated nearly two hundred years ago, by Beccher and Stahl, was not, it now appears, wholly founded on error; on the contrary, it was an imperfect anticipation of the great principle of energy, which plays so important a part in physical and chemical changes. The disciple of phlogiston, ignorant of the whole history of chemical combination, connected, it is true, his phlogiston with one only of the combining bodies, instead of recognising that it is eliminated by the union of all. "There can be no doubt," says Dr. Crum Brown, who first suggested this view, "that potential energy is what the chemists of the seventeenth century, meant when they spoke of phlogiston." "Phlogiston and latent heat," playfully remarks Volhard, "which formerly opposed each other in so hot a combat, have entered into a peaceful compact, and to banish all recollection of their former strife, have assumed in common the new name of energy." But as Dr. Odling well remarks, "In interpreting the phlogistic writings by the light of modern doctrine, we are not to attribute to their authors the precise notion of energy which now prevails. It is only contended that the phlogistians had in their time possession of a real truth in nature, which, altogether lost sight of in the intermediate period, has since crystallised but in a definite form."

But whatever may be the true value of the Stahlian views, there can be no doubt that the discoveries which have shed so bright a lustre round the name of Black, mark an epoch in the history of science, and gave a mighty impulse to human progress. A recent attempt to ignore the labours of Black and his great contemporaries, and to attribute the foundation of modern chemistry to Lavoisier alone, has already been amply refuted in an able inaugural address delivered a short time ago from the chair formerly occupied by Black. The statements of Dr. Crum Brown may indeed be confirmed on the authority of Lavoisier himself. Through the kindness of Dr. Black's representatives, I have been permitted to examine his correspondence which has been carefully preserved, and I have been so fortunate as to find in it three original letters from Lavoisier to Dr. Black. They were written in 1789 and 1790, and they appear to comprise the whole of the correspondence on the part of Lavoisier which passed between those distinguished men. Some extracts from these letters were published soon after Dr. Black's death by his friends, Dr. Adam Ferguson and Dr. Robison; but the letters

* It might be possible, and, if so, perhaps interesting, to speculate on the results of secular changes in physical laws, or in particles of matter which are subject to them, but (so far as experience, which is our *only* guide has taught us since the beginning of modern science) there seems no trace of such. Even if there were, as these changes must be of necessity extremely slow (because not yet even suspected) we may reasonably expect from the analogy of the history of such a question as gravitation, especially in the discovery of Neptune, that our work far from becoming impossible, will merely become considerably more difficult as well as more laborious, but on that account, all the more creditable when successfully carried out.

themselves, as far as I know, have never appeared in an entire form. I will crave permission to have them printed as an appendix to this address. Lavoisier, it will be seen, addresses Black as one whom he was accustomed to regard as his master, and whose discoveries had produced important revolutions in science. It may indeed be said with truth tha Lavoisier completed the foundation on which the grand structure of modern chemistry has since arisen; but Black, Priestley, Scheele, and Cavendish were before Lavoisier, and their claims to a share in the great work are not inferior to those of the illustrious French chemist.

Among the questions of general chemistry, few are more interesting, or have of late attracted more attention, than the relations which subsist between the chemical composition, and refractive power of bodies for light. Newton, it will be remembered, pointed out the distinction between the refractive power of a medium and its refractive index, and gave for the former the expression $\frac{\mu^2 - 1}{d}$ where $\mu$ is the refractive index, and $d$ the density of the refracting medium. Sir J. Herschel. anticipating later observations, remarked in 1830 that Newton's function only expresses the intrinsic refractive power on the supposition of matter being infinitely divisible, but that if material bodies consist of a finite number of atoms differing in weight for different substances, the intrinsic refractive power of the atoms of any given medium will be the product of the above function by the atomic weight. The same remark has since been made by Bertholet. Later observations have led to an important modification in the form of Newton's function. Beer showed that the experiments of Biot and Arago, as well as those of Dulong on the refractive power of gases, agree quite as well with a simpler expression as with that given by Newton; and Gladstone and Dale proposed in 1863 the formula $\frac{\mu - 1}{d}$ as expressing more accurately than any other. the results of their experiments on the refractive power of the liquids. The researches of Landolt and Wüllner have fully confirmed the general accuracy of the new formula. An important observation made, about twenty years ago by Delffs, has been the starting point for all subsequent investigations on this subject. Delffs remarked that the refractive indices of the compound ethers increase with the atomic weight, and that isomeric ethers have the same refractive indices. The later researches of Gladstone and of Landolt have, on the whole, confirmed these observations, and have shown that the specific refractive power depends chiefly on the atomic composition of the body, and is little influenced by the mode of grouping of the atoms. These inquiries have gone further, and have led to the discovery of the refraction equivalents of the elements. By comparing the refractive power of compound bodies differing from one another by one or more atoms of the same element, Landolt succeeded in obtaining numbers which express the refraction equivalents of carbon, hydrogen and oxygen, and corresponding numbers have been obtained for other elements by Gladstone and Haagen. The whole subject has been recently discussed and enriched with many new observations in an able memoir by Gladstone. As might be expected in so novel and recondite a subject, some anomalies occur which are difficult to explain. Thus hydrogen appears in different classes of compounds with at least two refraction equivalents—one three times as great as the other, and the refraction equivalents of the aromatic compounds and their derivations as given by observation are, in general, higher than the calculated numbers.

A happy modification of the ice calorimeter has been made by Bunsen. The principle of the method—to use as a measure of heat the change of volume which ice undergoes in melting—had already occurred to Herschel, and, as it now appears, still earlier to Hermann; but their observations had been entirely overlooked by physicists, and had led to no practical result. Bunsen has indeed clearly pointed out that the success of the method depends upon an important condition which is entirely his own. The ice to be melted must be prepared with water free from air, and must surround the source of heat in the form of a solid cylinder frozen artificially *in situ*. Those who have worked on the subject of heat know how difficult it is to measure absolute quantities with certainty, even where relative results of great accuracy may be attained. The ice calorimeter of Bunsen will therefore be welcomed as an important addition to our means of research. Bunsen has applied his method to determine the specific heats of ruthenium, calcium, and indium; and finds that the atomic weight of indium must be increased by one-half in order to bring it into conformity with the law of Dulong and Petit. He has also made a new determination of the density of ice, which he finds to be 0·9167.

In a Report on the Heat of Combination which was made to this Association in 1849, the existence of a group of isothermal bases was pointed out. "As some of the bases—potash, soda, baryta, strontia—" it was remarked, "form what we may perhaps designate an isothermal group, such bases will develop the same, or nearly the same heat in combining with an acid, and no heat will be disengaged during their mutual displacements." The latest experiments of Thomsen have given a remarkable extension to this group of isothermal bases. He finds that the hydrates of lithium, thallium, calcium and magnesium produce, when all corrections are made, the same amount of heat on being neutralised by sulphuric acid, as the four bases before mentioned. The hydrate of tetramethylammonium belongs to the same class of bases. Ethylamin, on the other hand, agrees with ammonia, which, as has long been known, gives out less heat in combining with the acids than potash or soda. An elaborate investigation of the amount of heat evolved in the combustion of coal of different kinds has been made by Scheurer-Kestner and Meusnier, accompanied by analyses of the coal. Coal rich in carbon and hydrogen disengages more heat in burning than coal in which those elements are partially replaced by oxygen. After deducting the cinders, the heat produced by the combustion of 1 gramme of coal varied from 8215 to 9622 units.

Tyndall has given an extended account of his experiments on the action of a beam of strong light on certain vapours He finds that there is a marked difference in the absorbing power of different vapour for the actinic rays. Thus the nitrate of amyl in the state of vapour absorbs rapidly the rays of light competent to decompose it, while iodide of allyl in the same state allows them freely to pass. Morren has continued these experiments in the south of France, and among other results he finds that sulphurous acid is decomposed by the solar beam.

Roscoe has prosecuted the photo-chemical investigations which Bunsen and he began some years ago. For altitudes above 10 degrees the relation between the sun's altitude and the chemical intensity of light is represented by a straight line. Till the sun has reached an altitude of about 20 degrees, the chemical action produced by diffused daylight exceeds that of the direct sunlight. The two actions are then balanced; and at higher elevations the direct sunlight is superior to the diffused light. The supposed inferiority of the chemical action of light under a tropical sun to its action in higher latitudes proves to be a mistake. According to Roscoe and Thorpe, the chemical intensity of light at Para under the equator in the month of April is more than three times greater than at Kew in the month of August.

Hunter has given a great extension to the earlier experiments of Saussure on the absorptive power of charcoal for gases. Cocoa-nut charcoal, according to Hunter's experiments, exceeds all other varieties of wood charcoal in absorptive power, taking up at ordinary pressures 170 volumes of ammonia and 69 of carbonic acid. Methylic alcohol is more largely absorbed than any other vapour at temperatures from 90° to 127°; but at 159°, the absorption of ordinary alcohol exceeds it. Cocoa-nut charcoal absorbs 44 times its volumes of the vapour of water at 127°. The absorptive power is increased by pressure.

Last year two new processes for improving the manufacture of chlorine attracted the attention of the section; one of these has already proved to be a success, and I am glad to be able to state that Mr. Deacon has recently overcome certain difficulties in his method, and has obtained a complete absorption of the chlorine. May we hope to see oxygen prepared by a cheap and continuous process from atmospheric air? With baryta the problem can be solved very perfectly, if not economically. Another process is that of Tessier de Mothay, in which the manganate of potassium is decomposed by a current of superheated steam, and afterwards revived by being heated in a current of air. A company has lately been formed in New York to apply this process to the production of a brilliant house-light. A compound argand burner is used, having a double row of apertures,—the inner row is supplied with oxygen, the outer with coal gas or other combustible. The applications of pure oxygen, if it could be produced cheaply, would be very numerous, and few discoveries would more amply reward the inventor. Among other uses, it might be applied to the production of ozone free from nitric acid by the action of the electrical discharge, and to the introduction of that singular body in an efficient form into the arts as a bleaching and oxidising agent. Tessier de Mothay has also proposed to prepare hydrogen gas on the large scale by heating hydrate of lime with anthracite.

We learn from the history of metallurgy that the valuable alloy which copper forms with zinc was known and applied long before zinc itself was discovered. Nearly the same remark may be made at present with regard to manganese and its alloys. The metal is difficult to obtain, and has not in the pure state been applied to any useful purpose; but its alloys with copper and other metals have been prepared, and some of them are likely to be of great value. The alloy with zinc and copper is used as a substitute for German silver, and possesses some advantages over it. Not less important is the alloy of iron and manganese prepared according to the process of Henderson, by reducing in a Siemen's furnace a mixture of carbonate of manganese and oxide of iron. It contains from 20 to 30 per cent. of manganese, and will doubtless replace, to a large extent, the spiegeleisen now used in the manufacture of Bessemer steel.

The classical researches of Roscoe have made us acquainted, for the first time, with metallic vanadium. Berzelius obtained brilliant scales which he supposed to be the metal, by heating an oxychloride in ammonia, but they have proved to be a nitride. Roscoe prepared the metal by reducing its chloride in a current of hydrogen, as a light gray powder, with a metallic lustre under the microscope. It has a remarkable affinity both for nitrogen and silicon. Like phosphorus, it is a pentad, and the vanadates correspond in composition to the phosphates, but differ in the order of stability at ordinary temperatures, the soluble tribasic salts being less stable than the tetrabasic compounds.

Sainte-Claire Deville, in continuation of his researches on dissociation, has examined the conditions under which the vapour of water is decomposed by metallic iron. The iron maintained at a constant temperature, but varying in different experiments from 150° C. to 1600° C., was exposed to the action of the vapour of water of known tension. It was found that for a given temperature the iron continued to oxidise, till the tension of the hydrogen formed reached an invariable value. In these experiments, as Deville remarks, the iron behaves as if it emitted a vapour (hydrogen) obeying the laws of hygrometry. An interesting set of experiments has been made by Lowthian Bell on the power possessed by spongy metallic iron of splitting up carbonic oxide into carbon and carbonic acid, the former being deposited in the iron. A minute quantity of oxide of iron is always formed in this reaction.

The fine researches of Graham on the colloidal state have received an interesting extension by Reynolds's discovery of a new group of colloid bodies. A solution of mercuric chloride is added to a mixture of acetone and a dilute solution of potassium hydrate, till the precipitate which at first appears is redissolved, and the clear liquid poured upon a dialyser which floated upon water. The composition of the colloid body thus obtained in the anhydrous state was found to be $(CH_3)_2\ (CO)_2\ Hg_3\ O_3$. The hydrate is regarded by Reynolds as a feeble acid even more readily decomposed than alkaline silicates. A solution containing only five per cent. forms a firm jelly when heated to 50° C. Analogous compounds were formed with the higher members of the fatty kenone series. In the same direction are the researches of Marcet on blood, which he finds to be a strictly colloid fluid containing a small proportion of diffusable salts.

In organic chemistry the labours of chemists have been of late largely directed to a group of hydrocarbons which were first discovered among the products of the destructive distillation of coal or oil. The central body round which these researches have chiefly turned is benzol, whose discovery will always be associated with the name of Faraday. With this body naphthaline and anthracene form a series, whose members differ by $C_4\ H_2$, and their boiling points by about 140°. The recent researches of Liebermann have proved, as was before suspected, that chrysene is a fourth member of the same series. I may add that ethylene, which boils at about 70°, corresponds in composition and boiling point to a lower member of the same series. Kekulé propounded some time ago with great clearness the question as to whether the six atoms of hydrogen in benzol are equivalent, or on the contrary play dissimilar parts. According to the first hypothesis, there can be only one modification of the mono- and penta-derivatives of benzol; while three modifications of the bi-, tri-, and tetra-derivatives are possible. On the second hypothesis, two modifications of the mono-derivatives are possible, and in general a much larger number of isomeric compounds than on the first hypothesis. Such is the problem which has of late occupied the attention of some of the ablest chemists of Germany, and has led to a large number of new and important investigations. The aromatic hydrocarbons, toluol, xylol, &c., which differ from one another by $C\ H_2$, have been shown by Fittig to be methyl derivatives of benzol. According to the first of the two hypotheses to which I have referred, only one benzol and one methyl benzol (toluol) are possible, and accordingly no isomeric modifications of these bodies have been discovered. But the three following members of the series ought each to be capable of existing in three distinct isomeric forms. The researches of Fittig had already established the existence of two isomeric compounds having the formula $C_8\ H_{10}$—methyl toluol obtained synthetically from toluol, and isoxylol prepared by the removal of an atom of methyl from the mesytelene of Kane. The same chemist has since obtained the third modification, orthoxylol, by the decomposttion of paraxylylic acid. These three isomeric hydrocarbons may be readily distinguished from one another by the marked difference in the properties of their tri-nitro-compounds, and also by their different behaviour with oxydising agents. Other facts have been adduced in support of the equality or homogeneity of position of the hydrogen atoms in benzol. Thus Hübner and Alsberg have prepared aniline, a mono-derivative from different bi-derivatives, and have always obtained the same body. The latest researches on this subject are those of Richter.

Baeyer has prepared artificially picoline, a base isomeric with aniline, and discovered by Anderson in his very able researches on the pyridine series. Of the two methods described by Baeyer, one is founded on an experiment of Simpson, in which a new base was obtained by heating tribromallyl with an alcoholic solution of ammonia. By pushing further the action of the heat, Baeyer succeeded in expelling the whole of the bromine from Simpson's base in the form of hydrobromic acid, and in obtaining picoline. The same chemist has also prepared artificially collodine, another base of the pyridine series. To this list of remarkable synthetical discoveries, another of the highest interest has lately been added by Schiff—the preparation of artificial coniine. He obtained it by the action of ammonia on butyric aldehyde ($C_4H_8O$). The artificial base has the same composition as coniine prepared from hemlock. It is a liquid of an amber-yellow colour, having the characteristic odour and nearly all the ordinary reactions of ordinary coniine. Its physiological properties, so far as they have been examined, agree with those of coniine from hemlock, but the artificial base has not yet been obtained in large quantity, nor perfectly pure.

Valuable papers on alizarine have been published by Perkin and Schunck. The latter has described a new acid—the anthraflavic—which is formed in the artificial preparation of alizarine. Madder contains another colouring principle, purpurine, which, like alizarine, yields anthracene when acted on by reducing agents, and has also been prepared artificially. These colouring principles may be distinguished from one another, as Stokes has shown, by their absorption bands; and Perkin has lately confirmed by this optical test the interesting observation of Schunck, that finished madder prints contain nothing but pure alizarine in combination with the mordaunt employed.

Hofmann has achieved another triumph in a department of chemistry which he has made peculiarly his own. In 1857 he showed that alcohol bases, analogous to those derived from ammonia, could be obtained by replacement from phosphuretted hydrogen; but he failed in his attempts to prepare the two lower derivatives. These missing links he has now supplied, and has thus established a complete parallelism between the derivatives of ammonia and of phosphuretted hydrogen. The same able chemist has lately described the aromatic cyanates, of which one only, the phenylic cyanate $(CO, C_6H_5, N)$, was previously known, having been discovered about twenty years ago by Hofmann himself. He now prepares this compound by the action of phosphoric anhydride on phenylurethane, and by a similar method he has obtained the tolylic, xylylic and naphthylic cyanates.

Stenhouse had observed many years ago that when aniline is added to furfurol, the mixture becomes rose-red, and communicates a fugitive red stain to the skin, and also to linen and silk. He has lately resumed the investigation of this subject, and has obtained two new bases, furfuraniline and furfurtoluidine, which, like roseaniline, form beautifully coloured salts, although the bases themselves are nearly colourless or of a pale brown colour. The furfuraniline hydrochlorate $(C_{17}\ H_{19}\ O_2\ N_2\ Cl)$ is prepared by adding furfurol to an alcoholic solution of aniline hydrochlorate containing an excess of aniline. We have also from Stenhouse a new contribution to the history of orcin, in continuation of his former masterly researches on that body. He has prepared the trinitroorcin ($C_7H_5(NO_2)_3O_2$), a powerful acid having many points of resemblance to picric acid. In connection with another research of Stenhouse, made many years ago, it is interesting to find his formula for escanthron, which was also that of Erdmann, confirmed by the recent experiments of Baeyer.

The interesting work of Dewar on the oxidation of picoline must not be passed over without notice. By the action of the permanganate of potassium on that body, he has obtained a new acid, which bears the same relation to pyridine that phthalic acid does to benzol. Thorpe and Young have published a preliminary notice of some results of great promise, which they have obtained by exposing paraffin to a high temperature in closed vessels. By this treatment it is almost completely resolved into liquid hydrocarbons, whose boiling points range from 18° C. to 300° C.; those boiling under 100° C. have been examined, and consist chiefly of olefines. In connection with this subject, it may be interesting to recall the experiments of Pelouze and Cahours on the Pennsylvanian oils, which proved to be a mixture of carbohydrogens belonging to the marsh-gas series.

An elaborate exposition of Berthelot's method of transforming an organic compound into a hydrocarbon containing a maximum of hydrogen, has appeared in a connected form. The organic body is heated in a sealed tube, with a large excess of a strong solution of hydriodic acid, to the temperature of 250°. The pressure in these experiments Berthelot estimated at 100 atmospheres, but apparently without having made any direct measurements. He has thus prepared ethyl hydride ($C_2H_6$) from alcohol, aldehyde, &c.; hexyl hydride ($C_6H_{14}$) from benzol. Berthelot has submitted both wood charcoal and coal to the reducing action of hydriodic acid, and, among other interesting results, he claims to have obtained in this way oil of petroleum.

By the action of chloride of zinc upon codeia, Matthiessen and Burnside have obtained apocodeia, which stands to codeia in the same relation as apomorphia to morphia, an atom of water being abstracted in its formation. Apocodeia is more stable than apomorphia, but the action of reagents upon the two bases is very similar. As regards their physiological action, the hydrochlorate of apocodeia is a mild emetic, while that of apomorphia is an emetic of great activity. Other bases have been obtained by Wright by the action of hydrobromic acid on codeia. In two of these bases, bromotetracodeia and chlorotetracodeia, four molecules of codeia are welded together so that they contain no less than 72 atoms of carbon. They have a bitter taste, but little physiological action. The authors of these valuable researches were indebted to Messrs. Macfarlane for the precious material upon which they operated.

We are indebted to Crum Brown and Fraser for an important work on a subject of great practical as well as theoretical interest, the relation between chemical constitution and physiological action. It has long been known that the ferrocyanide of potassium does not act as a poison on the animal system, and Bunsen has shown that the kakodylic acid, an arsenical compound, is also inert. Crum-Brown and Fraser find that the methyl compounds of strychnia, brusia, and thebaia are much less active poisons than the alkaloids themselves, and the character of their physiological action is also different. The hypnotic action of sulphate of methyl-morphium is less than that of morphia. But a reverse result occurs in the case of atropia, whose methyl and ethyl derivatives are much more poisonous than the salts of atropia itself.

Before proceeding to the subject of fermentation, I may refer to Apjohn's chemico-optical method of separating cane sugar, inverted sugar, and grape sugar from one another when present in the same solution, by observing the rotative power of the syrup before and after inversion, and combining the indications of the saccharometer with the results of an analysis of the same syrup after inversion. Heisch's test for sewage in ordinary water is also deserving of notice. It consists in adding a few grains of pure sugar to the water, and exposing it freely to light for some hours, when the liquid will become turbid from the formation of a well-marked fungus, if sewage to the smallest amount be present. Frankland has made the important observation that the development of this fungus depends upon the presence of a phosphate, and that if this condition be secured, the fungus will appear even in the purest water.

The nature of fermentation, and in particular of the alcoholic fermentation, has been lately discussed by Liebig with consummate ability, and his elaborate memoir will well repay a careful perusal. Dr. Williamson has also given a most instructive account of the subject, particularly with reference to the researches of Pasteur, in his recent Cantor lectures. A brief statement of the present position of the question will therefore not be out of place here. It is now 34 years since Cagniard de la Tour and Schwann proved by independent observations that yeast globules are organised bodies capable of reproduction by gemmation; and also inferred as highly probable that the phenomena of fermentation are induced by the development or living action of these globules. These views, after having fallen into abeyance, were revived and extended a few years ago by Pasteur, whose able researches are familiar to every chemist. Pasteur, while acknowledging that he was ignorant of the nature of the chemical act, or of the intimate cause of the splitting up of sugar in the alcoholic fermentation, maintained that all fermentations, properly so called, are co-relative with physiological phenomena. According to Liebig, the developement and multiplication of the yeast plant, or fungus, is dependent upon the presence and absorption of nutriment which becomes part of the living organism, while in the process of fermentation, an external action takes place upon the substance, and causes it to split up into products which cannot be made use of by the plant. The vital process and the chemical action, he asserts, are two phenomena which in the explanation must be kept separate from one another. The action of a ferment upon a fermentable body he compares to the action of heat upon organic molecules, both of which cause a movement in the internal arrangement of the atoms. The phenomena of fermentation Liebig refers now as formerly to a chemico-physical cause, the action, namely, which a substance in a state of molecular movement exercises upon another of highly complex constitution, whose elements are held together by a feeble affinity, and are to some extent in a state of tension or strain. Baeyer, who considers that in the alcoholic and lactic fermentations one part of the compound is reduced and another oxidised, adopts the view of Liebig that the molecules of sugar which undergo fermentation do not serve for the nourishment of the yeast plant, but receive an impulse from it. All are however agreed that fermentation is arrested by the death of the plant, and even a tendency to the acetous fermentation in wine may be checked, as Pasteur has shown, by heating the wine to a temperature a little below boiling point in the vessel in which it is afterwards to be kept.

I regret that the limits of an address like the present forbid me to pursue further this analysis of chemical work. Had they admitted of abridgment I should gladly have described the elaborate experiments of Gore on hydro-fluoric acid and the fluoride of silver. The important researches of Abel on explosive compounds will be explained by himself in a lecture with which he has kindly undertaken to favour the Association. Mr. Tomlinson will also communicate to the section some observations on catharism and nuclei, a difficult subject to which he has of late devoted much attention. And I am also informed that we shall have important papers on recent improvements in chemical manufactures.

No one can be more painfully alive than myself to the serious omissions in the historical review I have now read, more particularly in organic chemistry, where it was wholly impossible to grapple with the large number of valuable works which even a few months produce. I cannot, however, refrain from bearing humble tribute to the great ability and indomitable perseverance which characterise the labourers in the great field of organic chemistry. It would scarcely be possible to conceive any work more intelligently undertaken or more conscientiously performed than theirs, yet much of it, from its abstruse character, receiving little sympathy or encouragement except from the band of devoted men who have made this subject the chief pursuit of their lives. They will, however, find their reward in the consciousness that they have not lived in vain, but have been engaged, and successfully engaged, in the noble enterprise of extending for the benefit of the human family the boundaries of scientific knowledge. Nor is there any real ground for discouragement; Faraday, Graham, Magnus, and Herschel, who have left their impress on this age, were all distinguished chemical as well as physical discoverers; and the relations of the sciences are becoming every day so intimate that the most special research leads often to results of wide and general interest. No one felt this truth more clearly or illustrated it better than our lamented and distinguished friend, Dr. Miller, whose presence used to cheer our meetings, and whose loss we all most sincerely deplore.

## SECTION C.

### GEOLOGICAL SECTION

### Opening Address by the President, Archibald Geikie, F.R.S.

Instead of offering to the Geological Section of the British Association an opening Address on some special aspect, or branch of general Geology, I have thought that it might be more interesting, and perhaps even more useful, if I were to lay before you an outline of the geology of the district in which we are now assembled. Accordingly, in the remarks which I am now about to make, I propose to sketch to you the broader features of the geological structure and history of Edinburgh and its neighbourhood, dwelling more especially on those parts which have more than a mere local interest, as illustrative of the general principles of our science.

It would be as unnecessary, as it would be out of place here, to cite the long array of authors who have each added to our knowledge of the geology of this district; and many of them also, at the same time, to the broad fundamental truths of Geology. And yet it would be strange to speak here of the rocks of Edinburgh without even a passing tribute of gratitude to men like Hutton, Hall, Jamieson, Hay Cunningham, Hibbert, Hugh Miller, Fleming, Milne Home, and our late esteemed and venerable associate, Charles Maclaren—men who have made the rocks of Edinburgh familiar to geologists all over the world. If, therefore, I make no further allusion to these and other names, it is neither that I forget for a moment their claims, nor that I now bring forward any new material of my own, but because I wish to be understood as dealing with facts which, thanks to the labours of our predecessors, have become part of the common stock of geological knowledge.

For the purpose of gaining as clear an idea as may be of the rocks among which Edinburgh lies, and of the way in which they are grouped together, let us imagine ourselves placed on the battlements of the Castle, where, by varying our position, we may obtain a clear view of the country in every direction for many miles round. To the south-east the horizon is bounded by a range of high ground, rising as a long table-land above the lowland of Midlothian. That is a portion of the wide Silurian uplands of the south of Scotland, forming here the chain of heights known as the Lammermuir and Moorfoot Hills. Along most of its boundary line, in this district, the Silurian table-land descends with tolerable rapidity towards the plain, being bounded on its north-west side with a long fault, by which the Carboniferous Rocks are brought down against the hills. These Silurian rocks are the oldest strata of the district; and it is on their contorted and greatly denuded beds that the later formations have been laid down.

Turning now to the south, we see the chain of heights known as the Pentland Hills, striking almost from the very suburbs of Edinburgh south-westward in the direction of the Silurian uplands, which they eventually reach in the county of Lanark. This line of hills rises along an anticlinal axis by which the broad Carboniferous tract of the Lothians is divided into two distinct portions. The Pentlands themselves consist, as I shall afterwards point out, chiefly of rocks of Old Red Sandstone age, but the anticlinal fold along which they rise is prolonged through the Braid Hills, and through the Carboniferous ground by the Castle Rock of Edinburgh, even as far as the opposite shores of Fife. From the Castle we can readily follow with the eye the effects of this great dominant fold of the rocks. To the east, we mark how the strata dip away eastward from the axis of movement, as is shown in the escarpments of Salisbury Crag, Arthur Seat, and Calton Hill's, while, on the opposite or western side, the escarpment of the wooded hill of Corstorphine, facing towards us, points out the westward dip. From the same stand-point we can even detect the passage of the arch into Fife, for the rocks about Aberdour are seen dipping to the west, while eastward, they bend over and dip towards the east, at Kinghorn.

Although the structure of the district is simple when the existence and position of this anticlinal axis is recognised, some little complication is introduced by a long powerful fault which flanks the axis on its south-eastern side. The effect of this fault is to throw out a great part of the lower division of the Carboniferous formations, and to bring the Carboniferous Limestone series in some places close against the Lower Old Red Sandstone and its volcanic rocks. Another result has been the extreme tilting of the strata, whereby the Limestone series along the east side of the fault, has been thrown on end, and even in some parts bent back into a reversed dip. Hence, while on one side of the axis, the Limestone series is sometimes only a few hundred yards distant from the Old Red Sandstone, on the opposite or north-west side, the distance is fully eleven miles, the intervening space being there occupied by endless undulations of the lower divisions of the Carboniferous system. Hence, too, the Millstone Grit and Coal-Measures come in along the centre of the Midlothian basin a short way to the east of the Pentland axis; while, on the west side, they are not met with till we reach the borders of Stirlingshire and Linlithgow.

Another remarkable and readily observable feature is that on the west side of the Pentland ridge, the Carboniferous formations from almost their base up to the top of the Carboniferous Limestone series, abound in contemporaneous volcanic rocks; while, on the east side, beyond Edinburgh and Arthur's Seat, such rocks are absent until we reach the Garlton Hills, to the north of Haddington, where they reappear, but in a very different type from that which they exhibit to the west.

Let us now pass in review the different geological formations which come into the district around us, beginning with the oldest and ascending through the others, till we reach the superficial accumulations, and mark, in conclusion, how far the present surface features are connected with geological structure.

[The author then describes the various geological formations of the district—Silurian, Old Red Sandstone, and Carboniferous—dwelling in particular upon the history of volcanic action in that part of Scotland. On this subject he remarks:—]

#### *Outline of the History of Volcanic Action around Edinburgh*

The oldest volcanoes of this part of Scotland were those which, during the time of the Lower Old Red Sandstone, poured out the great sheets of porphyrite and the showers of tuff which now form the main mass of the range of the Pentland Hills. During the same long geological period, volcanic action was rife, as we have seen, along the whole of the broad midland valley of Scotland, since to that time we must refer the origin of the Sidlaw and the Ochil Hills, part of eastern Berwickshire, and the long line of uplands stretching from the Pentland Hills through Lanarkshire, and across Nithsdale, far into Ayrshire.

Of volcanic action, during the remainder of the Old Red Sandstone period, there is around Edinburgh no trace. But early in the following or Carboniferous period, the volcano of Arthur's Seat and Calton Hill came into existence, and threw out its tiny flows of basalt and porphyrite, and its showers of ashes. From that time onwards, through nearly the whole of the interval occupied by the deposition of the Carboniferous Limestone series, the district to the west of Edinburgh was dotted over with small cones, usually of tuff, but sometimes emitting limited currents of different basalt rocks, more especially in the space between Bathgate and the Forth, where a long bank, chiefly formed of such lava-currents, was piled up over and among the pools and shallows in which the limestones, sandstones, shales, and coal-seams were accumulated. To the north, also, similar volcanic activity was shown in the Fife tracts nearest the Forth; while eastwards between Haddington and Dunbar there lay a distinct volcanic focus, where great showers of red felspathic tuff and wide-spread sheets of porphyrite were ejected to form a bank over which the Carboniferous Limestone series was at length tranquilly deposited.

Volcanic activity seems to have died out here before the close of the Carboniferous Limestone period. It remained quiescent during the deposition of the Millstone Grit and Coal-measures; at least, no trace of any contemporaneous igneous ejection is found in any part of these formations. The intrusive masses of various basalt rocks, which here intersect the older half of the Carboniferous system, are, in all probability, of Lower Carboniferous date, connected with the eruptions of the interbedded volcanic rocks. The next proofs of volcanic action in this neighbourhood are furnished by the upper part of Arthur's Seat. At that locality we discover that after more than 3000 feet of strata had been removed by denudation from the Pentland anticlinal fold so as to lay bare the old Lower Carboniferous volcanic rocks of Edinburgh, a new focus of eruption was formed, from which were ejected the basalts and coarse agglomerates of the summit and shoulders of Arthur's Seat. There is no trustworthy evidence for fixing the geological date of this eruption. Evidently, from the great denudation by which it was preceded, it must belong to a much later period than any of the Carboniferous eruptions. Yet, from the great similarity of the Arthur's Seat agglomerate, both in composition and mode of occurrence, to numerous "necks" which rise through all parts of the Carboniferous system between Nithsdale and Fife, and which I have shown to mark the position of volcanic orifices during Permian times, I am inclined to regard these later igneous rocks of Edinburgh as dating from the Permian period. Arthur's Seat, however, seems to have been the only volcano in action during that period in this neighbourhood.

There still remains for notice one further and final feature of the volcanic history of this part of Scotland. Rising indifferently through any part of the other rocks, whether aqueous or igneous, and marked by a singular uniformity of direction, there is a series of basalt dykes, which deserves attention. They have a general easterly and westerly trend, and even where, as in Linlithgowshire, they traverse tracts of basalt-rocks, they preserve their independence, and continue as readily separable as when they are found intersecting sandstones and shales. These dykes belong to that extensive series which, running across a great part of Scotland, the north of England, and the north-east of Ireland, passes into, and is intimately connected with, the wide basaltic plateaux of Antrim and the Inner Hebrides. They date, in fact, from Miocene times, and, from their numbers, their extent, and the distance to which they can be traced from the volcanic centre of the north-west, they remain as a striking memorial of the vigour of volcanic action during the last period of its manifestation in this country.

*Glacial Phenomena*

To an eye accustomed to note the characteristic impress of ice-action upon a land-surface – the neighbourhood of Edinburgh presents many features of interest. It was upon Corstorphine Hill, on the western outskirts of the city, that Sir James Hall first called attention to striated rock-surfaces which, though erroneously attributed to the abrasion produced by torrents of water, were even then recognised as trustworthy evidence of the last great geological changes that had passed over the surface of the country. Even before we come to look at the surface in detail, and note the striation of its rocks, we cannot fail to recognise the distinctively ice-worn aspect of the hills round Edinburgh. Each of them is, in fact, a great *roche moutonnée*, left in the path of the vast ice-sheet which passed across the land. That this ice was of sufficient depth and mass to override even the highest hills, is proved not merely by the general ice-worn surface of the landscape, but by the occurrence of characteristic striæ on the summits of the Pentland Hills, 1,600 feet above the sea; that it came from the Highlands, is indicated by the pebbles of granite, gneiss, schist, and quartz rock, occurring in the older boulder-clays which it produced; and that, deflected by the mass of the southern uplands, the ice in the valley of the Lothians was forced to move seawards, in a direction a little north of east, is shown by the trend of the striæ graven on the rocks, as at Corstorphine, Granton, Arthur's Seat, and Pentland Hills.

*Connection of the present form of the surface with Geological Structure*

In concluding these outlines, let me direct the attention of the Section to the bearing which the geological structure of the district wherein we are now assembled has upon the broad and much canvassed question of the origin of land-surfaces. In the first place, we cannot fail to be struck with the evidence of enormous denudation which the rocks of the district have undergone. Every formation, from the oldest to the latest, has suffered, and the process of waste has been going on apparently from the earliest times. We see that the Lower Silurian rocks were upheaved and denuded before the time of the Lower Old Red Sandstone; that the latter formation had undergone enormous erosion before the beginning of the Carboniferous period; that of the Carboniferous rocks, a thickness more than 3 000 feet had been worn away from the site of Arthur's Seat before the last eruptions of that hill, which are possibly as old as the Permian period; that still further and vaster denudation took place before the setting in of the Ice-age; and finally, that the deposits of that age have since been to a large extent removed. With the proofs, therefore, of such continued destruction, it would be vain to look for any aboriginal outline of the surface, or hope to find any of the later but still early features of the landscape remaining permanent amid the surrounding waste.

In the second place, we note, that in the midst of this greatly denuded area, it is the harder rocks which form the hills and crags. Those masses which in the long process of waste presented most resistance to the powers of destruction, are just those which, as we might expect, rise into eminences, while those whose resistance was least sink into plains and valleys. All the craggy heights which form so conspicuous a feature of Edinburgh and its neighbourhood, are composed of hard igneous rocks, the undulating lowlands lie upon soft aqueous rocks.

In the third place, the coincidence of the position of hills and crags with the existence of ancient igneous rocks, cannot be misinterpreted by inscribing the presence and form of the hills to the outlines assumed by the igneous material ejected to the surface from below. The hills are not due to igneous upheaval at all, but can be shown to have been buried deep under subsequent accumulations, to have been bent and broken with all the bendings and breaks these later formations underwent, and to have been finally brought to light again only after a long cycle of denudation had removed the mass of rock under which they had been concealed. What is true of the hills of Edinburgh, is true also of all the older volcanic districts of Britain. Even where the hills consist of volcanic rocks, their existence, as hills, can be proved to be one of the results not of upheaval, but of denudation.

In the fourth place, this district furnishes an instructive illustration of the influence of faults upon the external contour of a country. The faults here do not form valleys. On the contrary, the valleys have been cut across them in innumerable instances. In the Dalkeith coal-field, for example, the valleys and ravines of the river Esk traverse faults of 190 to nearly 500 feet, yet there is no inequality at the surface, the whole ground having been planed down by denudation to one common level. When, however, a fault brings together rocks which differ much in their relative powers of resistance to waste, the side of the dislocation occupied by the harder rocks will tend to form an eminence, while the opposite side, consisting of softer rocks, will be worn down into a hollow or plain. Conspicuous examples are furnished by the faults which, along the flanks of the Pentland Hills, have brought down the comparatively destructible sandstones and shales of the Carboniferous series, against the much less easily destroyed porphyrites and conglomerates of the Old Red Sandstone.

In fine, we learn here as elsewhere in our country, and here more strikingly than often elsewhere, on account of the varied geological structure of the district, the present landscape has resulted from a long course of sculpturing, and that how much soever that process may have been accelerated or retarded by underground movements, it is to the slow but irresistible action of rain and frost, springs, ice, and the sea, that out of the various geological formations among which Edinburgh lies, her picturesque outline of hill and valley, crag and ravine, has, step by step, been carved.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The New Psychic Force

A YEAR ago Mr. Crookes, in a paper published in the *Quarterly Journal of Science*, announced his intention of scientifically investigating a certain class of phenomena, then known as "spiritual," which he complained had been strangely and unwarrantably neglected by those whose duty it was to investigate them. The results of some of these investigations have at last been published, in the same journal, under the title of "An experimental investigation of a new force."

Owing no doubt to the scientific reputation of Mr. Crookes, and the somewhat sensational title of the paper, it has attracted considerable attention. Whilst in quarters not purely scientific, much has been written about it, no attempt has been made, as far as I am aware, to subject the details of the experiment there described to a critical examination. It is the duty of every scientific man to be very anxious that nothing worthy of the name of Science, or calculated to be of permanent injury to Science, should ever obtain general credence. Whilst far from saying that this will be the result of Mr Crookes' paper, still I must confess that it appears to me that, carried away by enthusiastic impulses, he has trusted to experiments which in matters more purely scientific than his investigations really were, he would never have relied upon without further and more searching examination.

In the first place, then, scientific men will not, cannot admit the validity of a "new force" (of the nature of that which Mr. Crookes calls "psychic") which rests merely on the results of two experiments made in the presence of three or four persons, of whom only two are men of known scientific attainments, the others being but scientific amateurs. Even in the details of these experiments, we find that Dr. Huggins feels himself obliged to confess that the most startling phenomenon of all was *not* witnessed by him, although, of course, he has no doubt that it was seen as described by Mr. Crookes.

The experiments are but two in number; but in them, such is the peculiar nature of this "psychic" force as it manifests itself through the agency of Mr. Home (of spiritual and medi-umistic reputation), that there is hardly any known law in Physical and Biological Science which it does not tend to overthrow. Fortunately, however, in the interest of Science and the true bearing of modern scientific education, this force but rarely manifests itself, and it is particularly disobliging when many scientific sceptics wish to investigate it.* Work is done without apparently any force, mental or physical, being used up, so that we have here the direct creation of force—bodies ordinarily susceptible to the action of gravity are seen freely suspended in the air—a musical instrument (a wind instrument ordinarily played by keys), is suddenly imbued with so great a love and accurate knowledge of music that whilst the keys are visibly not touched, it plays "a well-known sad and plaintive melody, and, moreover, executes it perfectly in a very beautiful manner." All these and other phenomena, so varied, so thrilling, so "psychic," we are solemnly informed took place one evening in a room of which the temperature varied from 68° to 70° F.!

To most scientific men I am afraid there will appear something in the above so absurd and ludicrous, something so allied to the performances of professed jugglers and spiritual mediums, that it would not be worth any serious consideration, did not the scientific reputation of Mr. Crookes and Dr. Huggins demand that the experiments which gave the above results should be at once disproved or confirmed. If we proceed to examine these experiments carefully, and rigidly investigate them, we find, however, a complete want of attention to minute but by no means unimportant details—a complete absence of any attempt to ascertain whether it was not possible to produce these results without any "psychic force," and a firm confidence and belief in the ingenuousness (I had almost unwittingly written "ingenuity") of Mr. Home.

Let us now examine the experiments in detail. Firstly, with regard to the accordion, we are not told why the cage was constructed at all, and why, moreover, when constructed it was placed *under a dining-room table* of all places in the world. Does Mr. Crookes wish us to believe that it is only inside such wooden cages and in such peculiar positions that this "psychic force" manifests itself? If that is not the case, why was not the cage placed openly in the room, so that Dr. Huggins might not have had to confess that *he did not see* the accordion freely suspended in air, which Mr. Crookes and the others by dint of straining under the table did see. Then again, the accordion was confessedly placed in Mr Home's hands before it was placed in the cage under the table—this was certainly unnecessary and is very unsatisfactory. Then it is obvious that to play the accordion the keys must in turn have been depressed. Yet Mr. Crookes does not volunteer a single word to show that he noticed whether the keys were successively pressed down or not, in fact, he rather leads us to infer that they were not. Again, it is clearly a physical impossibility for the accordion to have gone round and round the cage if Mr. Home's hand was quite still, for if he held the accordion at all, his hands must have followed its movements, and what is there to show that the accordion moved his hand or his hand the accordion? Then again, as to the instrument chosen, would a concertina act in the same manner or not? for, from the frequency with which an accordion has been appealed to by "spiritual mediums," it has acquired anything but a good reputation. It is a pity we are not informed whether Mr. Home could in the moments when he is free from "psychic influence" play on the accordion or not, and also as to what were the names of "the simple air" and the "sweet and plaintive melody" which it so obligingly played. We are also not told either how long the experiment lasted, or how long the accordion was playing, or, what is much more to the point, how long it contravened all the laws of gravity and of the acoustics of wind instruments. Surely this is an important question, quite as important as that the temperature varied from 68° to 70° Fahr.

Such are some of the questions which arise with respect to the first experiment, and which must be answered before any reliance can be placed on the results attained.

* *Vide* Mr. Home's St. Petersburg experiments.

There still remains the second experiment, which was of an entirely different kind: the one with the spring-balance. Mr. Crookes here says, "Mr. Home's fingers were never more than one-and a-half inches from the extreme end, and the wooden foot being only one-and-a-half inches wide, and resting flat on the table, it is evident that no amount of pressure exerted in that space could produce any action on the balance;" and in this I quite agree; but did Mr. Crookes notice if the table itself *was moved at all?* From a very slight consideration of the peculiar apparatus employed, it is obvious that were the table to tip up in any so small a manner, the index of the balance must descend; and if the table was to tip up and down successively, the very same effect would be produced on the index of the balance as that which Mr. Crookes ascribes to "successive waves of psychic force." I do not say that the table was tipped up—that would have been trickery—but we have to account for certain results, and I do say that the tipping of the table would produce those very results, and that, moreover, there is nothing said about the table being immovable, or even heavy, or in any way fastened to the ground, as it most assuredly ought to have been. It does not appear so difficult to imagine that the "psychic force," which could produce such a strange effect upon an accordion could also so agitate the table that *it* also should show a tendency to move—and, if this were the case, the whole apparatus was so placed that the very slighest movements of the table would be magnified by the index of the balance.

On account of these and many other objections, I am forced to the conclusion previously stated, that these experiments were inaccurately performed—the details were not sufficiently examined, nor obvious errors apparently avoided, so that until they are repeated in the presence of other scientific men, they are not worthy of scientific consideration. We have read of the same phenomenon over and over again described as due to spiritual manifestations—many of them, as is well known, performed through the same agency—a medium—as those in this case. The British Association is about to meet. Let Mr. Crookes but repeat any one of the experiments at one of the evening *soirées*, and, if he can do this, he will make the Edinburgh Meeting for ever memorable, and will have earned for himself the undying reputation of having been the first to discover that in the midst of apparent humbug true science really and truly did exist.

J. P. Earwaker

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Prof. Balfour Stewart, in Nature for July 27, does but scant justice to Mr. Crookes's investigations. "Allowing," he says, "that things of an extraordinary nature are frequently witnessed on such occasions" (he, no doubt, means to refer to the so-called Spiritualistic *séances*) "yet we are by no means sure that these constitute external realities." And he then goes on to suggest that the phenomena may occur rather in the imagination of the spectators than in the outside world; or that the mediums (though he won't give them that name) may be under some mental influence of an "electro-biological" nature. By the way it is a pity that any man of science should help in giving currency to such a quack-scientific word; if this unknown influence must have a name, Mesmerism is the most appropriate; that does not pretend to explain the cause of the phenomena, but only to commemorate their discoverer. Now in the experiments upon which Prof. Stewart comments (I presume he refers to those described in the current number of the *Quarterly Journal of Science*) there does not seem to have been much room for the exercise of the imagination of the spectators, nor for any "electro-biological" influence to act through the medium. Setting aside the accordion performances, which perhaps left a little scope for eye deception, the results of the trial with the spring-balance were quite opposed to the known laws of mechanics. And certainly this trial took place under conditions which should have rendered deception impossible. The evidence of two such careful observers as Mr. Crookes and Dr. Huggins is not readily set down as a phantasm of their imagination; they are men accustomed to weigh the evidence of their senses with the utmost caution, for the slightest error therein would cause grave disturbance in their calculations. When such men testify that some mysterious force acted upon a lever in a way that no known force acts, and produced before their eyes results quite new to their experience, we should be as ready to believe them as if Dr. Huggins announced a new planet or Mr. Crookes a new metal; their testimony is as valuable in the one case as in the other. It is true that here they can only bear witness to the unknown, but the very existence of this unknown has hitherto been questioned. That when it is known this force shall be acknowledged to be a spiritual one is repugnant to all philosophy, and Serjeant Cox's haste to name it "psychic" is neither wise nor politic. Things spiritual have been materialised in the grossest manner by so-called spiritualists until the word has lost its meaning, and come to signify merely a cause unknown of phenomena sensual to the last degree. So it will be with "psychic" unless some one in authority stop this misuse of it at the very beginning. GEORGE FRASER

### Height of Auroras

I SAW the aurora of Sept. 3, 1870, described by H. C. Key on p. 121, and I observed it from 10 to 11 P.M., but here it never reached quite to the zenith, and at 11.2 P.M. was no where high. Its brightest feature was then a distinct arch, the apex of whose central line was 12° in altitude. If Mr. Key's description of the clear space of 7° or 8° below the aurora in the S.S.E., applies to that time, it would seem that the part of the aurora bordering the clear space cannot have been more than 25 miles above the earth, and was more likely only 17 or 18 miles.

It would be well if the heights of auroras were better known than they are; and I think if systematic observations were made simultaneously at different stations, our knowledge on the subject would be largely increased. I am willing to be one of the observers in such an investigation, and Mr. G. J. Symons, the editor of the *Meteorological Magazine*, has expressed his readiness to aid. T. W. BACKHOUSE

Sunderland, July 22

### Daylight Auroras

ON Sunday, the 23rd July, at 7 40 P.M., there was visible from Blackpool a phenomenon which might readily be mistaken for a daylight manifestation of the Aurora. The phenomenon in question consisted of a number of parallel streamers of light rising vertically and situated from the observer in a north-westerly direction. That portion of the sky occupied by these streamers would be about twenty-five degrees square, its lowest portion being about fifteen degrees above the horizon. At the time of the appearance the sun was obscured by a small but very dense cloud. Large masses of nimbus clouds occupied almost the whole of the north-western, northern, and north-eastern portion of the sky, whilst a few cumulus and cirro-cumulus clouds were visible in the eastern and southern parts of the heavens; one-twentieth part perhaps of the whole sky being apparently free from cloud. The streamers, which, like those of the Aurora, were intermittent in intensity, contrasted greatly in direction with any proximate beams of the sun. The whole thing, however, I am strongly of opinion, was nothing more than a meteorological phenomenon of a very different nature from the Aurora; in short, I believe it was an unusual appearance attendant on a distant and somewhat singularly circumstanced rainfall. Immediately above the uppermost boundary of the space occupied by the streamers there was a large nimbus cloud entirely obscured from the sun's direct rays, whilst that part of the sky occupied by the streamers themselves was in a strong sunshine. The whole phenomenon lasted about half an hour, and my opinion that it was but an unusual aspect of a distant rainfall was strengthened by the fact of a heavy shower of rain descending immediately after the disappearance of the streamers, the upper-current of the air being from the west by north. The rain-fall lasted about a quarter of an hour. and was accompanied by a double rainbow. When it had ceased that portion of the sky previously occupied by the streamers and almost half the remainder contained no visible trace of cloud.

This is the first instance in which I have seen what might be mistaken for a daylight manifestation of the Aurora. The streamers were so like those of the northern light, and the nature of the appearance nevertheless so obviously connected with a transient condition of the atmosphere, that I am very much tempted to doubt the visibility at any time of genuine Aurora by daylight. D. WINSTANLEY

Manchester, July 24

### Spectrum of the Aurora

HAVING noticed that Prof. Zollner observed a *red line* in the spectrum of the aurora on October 25, 1870, and as it appears this was the first time the *red line* had been observed in Europe, I am induced to send you the following extract from a short paper read by me April 12, 1870, before the Royal Society of Victoria, on the great aurora of April 5, 1870:—

"The spectrum of the aurora was obtained with one of Mr. Browning's micro-spectroscopes. When the spectroscope was directed to the red streamers, a red line, more refrangible than C (hydrogen line), a greenish line about the position of the green calcium lines, and an indistinct band more refrangible still, which appeared as if resolvable into lines, were observed. When the spectroscope was directed to the green auroral arch, the red line disappeared, and only green ones remained; the rapid disappearance of the red line as the slit passed across the boundary between the base of the streamers and the green arch, was remarkable."

In this aurora there was the usual auroral cloud-like bank on the horizon (sea-horizon) surmounted by an arch of bright greenish light to an altitude of nearly 20°, terminating with a very defined margin, from which the red streamers sprung upwards as if from behind a screen, which shed enough light at midnight to read a newspaper by. This aurora was ushered in by great magnetic disturbances for days previously, which culminated about the time of the brightest display. ROBERT J. ELLERY

Melbourne Observatory, May 19

### Sparrow Cages

A PARAGRAPH in NATURE speaks of the export of sparrows to America. Such long low cages as are described at page 245, *covered with canvas*, may be seen at Leadenhall Market, in which very many thousands of Egyptian quails are brought to London alive and, I am told, disposed of as larks; of course not for the voice. A. H.

July 27

## BOOKS RECEIVED

ENGLISH.—Taine on Intelligence, part II.; translated by T. D. Haye (L. Reeve and Co.).—A Treatise on Terrestrial Magnetism (Blackwood).—Text-books of Science: Elements of Geometry: J. Watson (Longmans).—Domestic Botany: John Smith (L. Reeve and Co.).—Lighthouse Illumination: T. Stevenson, second edition: (A. and C. Black.)

## PAMPHLETS RECEIVED.

ENGLISH.—On the Dermal and Visceral Structures of the Kagu, Sun-bittern, and Boatbill: Dr. Murie.—Researches on the Anatomy of the Pinnipedia, part I.: Dr. Murie. Poisoning and Pilfering, Wholesale and Retail. —Journal of the Anthropological Institute, part I. March and April, 1871.—Transactions of the Manchester Geological Society, vols. 9—10—Report of the Meteorological Committee of the Royal Society.—Journal of the Statistical Society, June.

AMERICAN.—On the Secular Perturbations of the Planets: A. Hall.—On the Application of Photography to the Determination of Astronomical Data A. Hall.—Equatorial Observations made at the U. S. National Observatory, Washington: A. Hall.—The School Laboratory of Physical Science, part II.: Prof. G. Hinrichs.

## CONTENTS

THURSDAY, AUGUST 10, 1871

## THE ORGANISATION OF LOCAL SCIENTIFIC EFFORT

AMONG the many topics of national importance which have been discussed at the recent meeting of the British Association, there is none which promises to bear more fruit, or which we more gladly bring before the notice of our readers, than a scheme already suggested in these columns, which has been discussed and adopted at a full and influential meeting of representatives of all branches of Science, the President of the Association, Sir William Thomson, being in the chair.

This scheme is essentially as follows:—It is proposed in the first instance, to make an attempt to extend and improve the present system of giving scientific lectures to the people, and by this means to awaken an interest in science and scientific progress in places where otherwise there would be little probability of such good work being done.

There is little need that we should expatiate on the extreme importance of this object, and on the value of the results which are certain to follow from an energetic carrying out of the proposal. With the example of Manchester and other large towns before us, it is not too much to hope that as soon as the scheme is properly developed, the beneficial effects already experienced in these places wlll become general throughout the country. In Manchester, to take one instance, we find that each Science Lecture has, on an average, been attended by upwards of one thousand persons, and that the interest excited by the lectures has not been a mere temporary amusement is evidenced by the fact that the lectures when reprinted have sold by tens of thousands. In Belfast, also, Science Lectures to working-men have been most successfully given for more than ten years. In this way it is clear that not merely the auditors, but a very large outside public, have benefited by this method of bringing science and its teachings home to everyone. A project, which has been so successful over limited areas, and which must be as successful if tried on a larger scale, is well deserving of being adopted and extended by so important a body as the British Association.

There is another consideration which renders the adoption of this scheme by the British Association doubly valuable. The danger attending the delivery of popular lectures has always been that true scientific method may be lost sight of in the desire of the lecturer to merely please the eye, or to keep up interest in the auditory by mere sensational display. It is to be hoped that we shall now have a guarantee at any rate against this evil. It is not possible always to make science amusing, but we now possess ample experience which goes to show that a scientific lecture delivered by a competent man, fully impressed himself with the dignity of what he is doing, is able to awake the interest and rivet the attention of those classes for whom the lectures are specially intended.

This, however, after all, is only one side of the project. We do not for one moment wish to undervalue the extreme importance of science lectures, but we must not forget that they will have missed their mark if they have not engendered the desire for something more durable (because more useful) in the way of scientific instruction, which can be obtained in a variety of ways, as, for instance, in Mechanics' Institutions, in the science classes of the Science and Art Department, or in other organisations which may be subsequently developed.

It is not, however, merely a question of scientific instruction. Throughout the country we find societies, field clubs, local museums, &c., all of which are more or less actively engaged in the pursuit of knowledge, local inquiries, or exploration, and all of which are working, more or less, at a disadvantage, in consequence of the chaotic state of our scientific arrangements, and from their lack of that power which springs from unity.

Now is it too much to expect that under the best possible conditions such engines of scientific advancement would be more useful than they are at present, or that there would be more of them? We have only to look at what has been done in some of the higher schools even, to satisfy ourselves upon this point. At Rugby, Clifton, Marlborough, not to mention other schools, we have museums and natural history societies existing side by side with the work of the school, and the masters testify in the most definite manner to the extreme importance of the culture obtained by such means. Now, if this is important for a limited number of schoolboys, how much more important must it be throughout the length and breadth of the land; where at present we find teaching going on without museums, museums existing in localities where there is no one to look after them, field clubs examining every inch of the ground, while a much richer region elsewhere is entirely unexplored, each worker, as it were, away from his support, and the workers few. It is as if an army were moving through a hostile country without commander, without plan, without any power of combination, and without either vanguard, Uhlan, or second line.

Here then we have plainly before us the ground to be viewed by the Committee to which we have referred, a Committee which we doubt not will be appointed by the British Association with full power to report upon, and, if necessary, to carry out at once, any measures which it may be desirable to take in the directions we have indicated. When once such a body is established, and its existence generally known, its work will soon take the most concrete form, a more concrete one than we have ventured to assign to it in this article; but it is clear that if limited in its functions in the first instance to the lecture arrangements to which we have referred, and to inquiries into the actual geographical position of and condition of our local societies, museums, field clubs, and the like, so that the committee should become the head-quarters of information on these subjects to those who wish to establish similar institutions in new districts, or to expand an existing one, the greatest possible good to science will follow. But it is not too much to hope that such a body would in time become the centre of influence as well as of information, would be able to mould actual and potential institutions into the best form for effective work, and would be able to economise their resources, and to increase the utility of each of them.

## *KINGSLEY'S "AT LAST"*

*At Last: a Christmas in the West Indies.* By Charles Kingsley. With Illustrations. In two volumes. (Macmillan and Co., 1871.)

A BOOK on the West Indies by an ordinary tourist would be hardly bearable. Mr. Trollope was amusingly brilliant as well as philosophical, and we read him with pleasure; but the author of "Westward Ho!" possesses a wealth of knowledge both in history and in natural science wherewith to illustrate his journey, which, even without his charming style and world-wide popularity, would render his book attractive to many a thoughtful reader. To him the air of the West Indies is "full of ghosts" of gallant soldiers and sailors, whose deeds of daring have made almost every bay and roadstead famous, and who, he thinks, might well ask us to render an account of our stewardship of those beautiful islands, which they won for us with precious blood, and which we, too ignorant and helpless to govern them properly, have misused and neglected. Passing by Dominica recalls one of those deeds, the record of which must thrill the heart of every Englishman: "here Rodney, on the glorious 12th of April broke Count de Grasse's line (teaching thereby Nelson to do the same in like case), took and destroyed seven French ships of the line, and scattered the rest, preventing the French fleet from joining the Spaniards at Hispaniola, thus saving Jamaica and the whole West Indies, and brought about by that single tremendous blow the honourable peace of 1783. On what a scene of crippled and sinking, shattered and triumphant ships, in what a sea, must the conquerors have looked round from the *Formidable's* poop, with De Grasse at luncheon with Rodney in the cabin below, and not, as he had boastfully promised, on board his own *Ville de Paris!*"

A little farther he comes in sight of "an isolated rock, of the shape, but double the size, of one of the great Pyramids, which was once the British sloop of war, *Diamond Rock*," and tells us the interesting tale, not of any magical transformation or nautical legend, but of one of those inspirations of genius which converted an almost inaccessible rock into a fortress, which was manned by 120 men and boys, and for a year and a half swept the seas, being "borne on the books of the Admiralty as Her Majesty's ship *Diamond Rock*."

More suited, however, to our present purpose is the reminiscence of the eruption of the volcano of St. Vincent in 1812, which lasted three days and nights, covering most of the island with ashes, and utterly ruining whole estates. In Barbadoes, eighty miles to windward, the dust fell so thick that total darkness continued till near midday, and strange to say, with the darkness was unusual silence, for the trade wind had fallen dead, and the everlasting roar of the surf was gone. As the dust-cloud drifted away and the sun again appeared, the trade wind blew suddenly once more out of the east, and the surf roared again along the shore. The authority for this fact Mr. Kingsley considers to be sufficient, but its explanation is by no means easy.

Arriving at Trinidad, our author fairly revels in the delights of tropical life, scenery, and vegetation. The flowers and forest trees, the creepers and climbers, and the noble palms, fill his soul with delight; and he is never tired of painting the scenes around him in his own picturesque and glowing language. The force and vigour of vegetable growth, the hum and glitter of insects, the strange birds and the howling monkeys, all have the more charm for him that he already knows so much about them, and that they satisfy an intelligent and highly-cultivated curiosity. Here is a little bit out of his picture of the "High Woods," as the virgin forests are called in Trinidad:—

"In Europe a forest is usually made up of one dominant plant—of firs or of pines, of oaks or of beeches, of birch or of heather. Here no two plants seem alike. There are more species on an acre here than in all the New Forest, Savernake, or Sherwood. Stems rough, smooth, prickly, round, fluted, stilted, upright, sloping, branched, arched, jointed, opposite-leaved, alternate-leaved, leafless, or covered with leaves of every conceivable pattern, are jumbled together, till the eye and brain are tired of continually asking 'What next?' The stems are of every colour—copper, pink, grey, green, brown, black as if burnt, marbled with lichens, many of them silvery white, gleaming afar in the bush, furred with mosses and delicate creeping film-ferns, or laced with the air-roots of some parasite aloft. Up this stem scrambles a climbing Seguine (*Philodendron*) with entire leaves; up the next another quite different with deeply cut leaves; up the next the Ceriman (*Monstera pertusa*) spreads its huge leaves, latticed and forked again and again. So fast do they grow, that they have not time to fill up the spaces between their nerves, and are consequently full of oval holes; and so fast does its spadix of flowers expand, that an actual genial heat and fire of passion, which may be tested by the thermometer, or even by the hand, is given off during fructification. Look on at the next stem. Up it and down again a climbing fern, which is often seen in hothouses, has tangled its finely-cut fronds. Up the next a quite different fern is crawling, by pressing tightly to the rough bark its creeping root-stalks, furred like a hare's leg. Up the next the prim little griffe-chatte plant has walked by numberless clusters of small cat's-claws which lay hold of the bark. . . . ."

Again—"Look here at a fresh wonder. Away, in front of us, a smooth grey pillar glistens on high. You can see neither the top nor the bottom of it. But its colour and its perfectly cylindrical shape tell you what it is—a glorious palmiste, one of those queens of the forest which you saw standing in the fields, with its capital buried in the green cloud, and its base buried in that bank of green velvet plumes, which you must skirt carefully round, for they are a dwarf prickly palm, called here Black Roseau. Close to it rises another pillar, as straight and smooth, but one-fourth of the diameter, a giant's walking cane. Its head, too, is in the green cloud. But near are two or three younger ones, only forty or fifty feet high, and you see their delicate feather heads, and are told that they are Manacques (*Euterpe oleracea*), the slender nymphs which attend upon the forest queen, as beautiful, though not as grand, as she."

The wonderful flowers, the strange creepers and fantastic jungle ropes, the buttress trees, the orchids, and a hundred other characteristic tropical forms, are described in equally picturesque language. A giant Hura tree, forty-four feet in girth, and 192 feet high, is the occasion for some remarks on Darwinism. For this is a euphorbiaceous tree, and allied, therefore, to our humble spurges, as well as to the manioc, the castor-oil plant, the crotons, the scarlet poinsettia, and many other distinct forms.

"But what if all these forms are the descendants of one original form? Would that be one whit more wonderful, more inexplicable, than the theory that they were each and all, with their minute and often imaginary shades of difference, created separately and at once? But if it be—which I cannot allow—what can the theologian say save that God's works are even more wonderful than we always believed them to be? As for the theory being impossible, who are we that we should limit the power of God? If it be said that natural selection is too simple a cause to produce such fantastic variety, we always knew that God works by very simple or seemingly simple means; that the universe, as far as we could discern it, was one organisation of the most simple means."

CHINESE MAN AND WOMAN

The beauty of many of the clearings in the forests must have made every traveller in the tropics think what scenes of surpassing beauty might be created by judicious clearing and planting, by helping Nature in a country and climate where, even unassisted, she can do so much, and where such a profusion of beautiful materials exists to work with. Mr. Kingsley remarks that "the plants most capable of beautifying any given spot do not always grow therein, simply because they have not yet arrived there, as may be seen by comparing any wood planted with rhododendrons and azaleas with the neighbouring wood in its native state. Thus may be obtained somewhat of that variety and richness which is wanting everywhere, more or less, in the vegetation of our northern zone, only just recovering slowly from the destructive catastrophe of the glacial epoch, a richness which, small as it is, vanishes as we travel northward, till the drear landscape is sheeted more and more with monotonous multitudes of heather,

COOLIE AND NEGRO

grass, fir, or other social plants. But even in the tropics the virgin forest, beautiful as it is, is without doubt much less beautiful, both in form and colour, than it might be made. Without doubt also, a mere clearing, after a few years, is a more beautiful place than the forest, because by it distance is given, and you are enabled to see the sky, and the forest itself beside; because new plants, and some of them very handsome ones, are introduced by cultivation, or spring up in the rastrago; and lastly, but not least, because the forest on the edge of the clearing is able to feather down to the ground, and change what is at first a bare tangle of stems and boughs into a softly rounded bank of verdure and flowers. When in some future civilisation, the art which has produced, not merely a Dropmore or a Chatsworth, but an average English shrubbery or park, is brought to bear on tropic vegetation, then Nature, always willing to obey when conquered by fair means, will produce such effects of form and colour around tropic estates and cities as we cannot fancy for ourselves."

Much information is given as to the races that now people the West Indies, Negroes, Coolies, and Chinese. The Coolies are very well spoken of, and the system of immigration is said to work well and to be beneficial to all concerned. The contrast between the different races in manners, character, and appearance appears to have struck our author very much, and many clever sketches illustrate his descriptions. In the cuts which we here reproduce, the three widely different races, Negroes, Coolies, and Chinese are very characteristically represented. There are also some excellent illustrations of tropical scenery and productions, that representing "A Tropic Beach" being one of the best, and the cut of the "Little Ant-eater" being also excellent.

We must point out one fault in the book, a fault which nature-loving travellers often fall into, too free use of the local names of natural objects, which, though made familiar to themselves by daily repetition, are a great annoyance to the reader, who cannot possibly learn their meaning during the perusal of the book. Towards the end of the second volume, for example, we find these lines:—"Below were Mamure, Roseau, Timit, Aroumas, and Talumas (*Canna*), mixed with Myrtles and Melastoms, then the copper Bois Mulatre among the Cocorite and Jagua palms." All these names, with a hundred others, have been carefully referred to their respective species in foot-notes in earlier portions of the volumes, but that does not help either the botanist or the general reader to remember such a string of new and uncouth words. Local names should, we think, be used only for a very few of the most abundant and characteristic species, whose mention will be so frequent as to impress them upon the reader's memory. For the others, English equivalents should be used where they exist; and for the majority, the family, generic, or specific names, which will convey some distinct impression to the naturalist, and will enable even the general reader to obtain information by consulting a dictionary of natural history or an encyclopædia.

To conclude, the book is beautifully got up; it conveys much information on the society, politics, and natural history of one of the most luxuriant and interesting of the West Indian Islands, and cannot fail to be read with both pleasure and profit by every lover of nature.

A. R. W.

## OUR BOOK SHELF

*Notes of a Course of Nine Lectures on Light.* By John Tyndall, LL.D., F.R.S. (London: Longmans and Co., 1871.)

THE contents of this little volume fully justify the author in his prefatory remarks, and the intelligent student or teacher will find very great benefit by a perusal of these "Notes." Every statement is extremely clear, and the experiments hinted at are all extremely good. Such a publication is exceedingly well adapted to a certain class of minds, of which the latent powers are better brought out by hinting at solutions than by detailed explanations. The skeleton is brought before them, and they are called upon to clothe it for themselves. In fact, if physical science is to be used in order to educate and train as well as to inform the mind, we cannot dispense with a set of notes of this description. The author has dealt very fully with his subject, and he has not been deterred, when the occasion required, from stepping beyond the physical region into the physiological. Thus we have some very good remarks upon brightness, as well as upon the eye and its peculiarities with respect to light. On the other hand, he has not permitted himself to enter largely on the subject of dark rays, but has confined himself to those which affect the eye. A perusal of these Notes will benefit all who wish to become acquainted with the laws of light, and even if they sat down to such a task, having a previous acquaintance with every statement, they will rise with benefit; for a branch of knowledge, like a landscape, is never fully understood until it is regarded under different atmospheres and from different points of view.

B. S.

*Transactions of the Newcastle-upon-Tyne Chemical Society.* Vol I. (1868-1871.)

THE Newcastle-upon-Tyne Chemical Society has been established for nearly three years; during this period the Society has been very prosperous, both as regards the number of its members and the importance of the papers read at its monthly meetings. The members were fortunate enough to secure the services of Mr. Lowthian Bell as their first president, and of several experienced gentlemen as members of the committee, a fact which must have contributed materially to their success. The papers which have been read before the Society since its commencement, relate, as might be expected, principally to technical chemistry and analysis. Amongst them we find Mond "On the Recovery of Sulphur from Alkali Waste," followed by an interesting discussion. Dr. Lunge has contributed several valuable papers to the volume; they are chiefly abstracts of the more important analytical methods published on the Continent. The papers on the analysis of technical products constitute the principal part of the book, the number of those on original subjects being very small. The inaugural address by Mr. Bell contains an interesting historical sketch of the various chemical manufactures on the banks of the Tyne, showing how rapidly they have grown, until they have now reached an enormous magnitude. There is also a paper by Mr. Clapham on the commencement of the manufacture of soda on the Tyne, which contains a sketch of the difficulties that had to be overcome by the founders of this industry. Among the other papers may be mentioned several by Dr. Wright, and one by Mr. Swan, describing an improved form of anemometer.

A. P.

*Transactions of the Woolhope Naturalists' Club for* 1870. (Hereford, 1871.)

THIS volume is equal in interest and value to its predecessors, and still more varied in the nature of its contents. All branches of natural history are pursued with ardour by the Woolhope Naturalists, and good scientific work is done in the various sections. Zoology furnishes papers

"On the Habits of *Platypus cylindrus*," and on that vexed question "The Life History of *Rhipiphorus paradoxus*," by Dr. Chapman, "On Rare Birds," by Mr. James W. Lloyd, and "On Herefordshire Lepidoptera," by several contributors. In botany, we have papers "On the Reproduction and Growth of the Mistletoe," by the Rev. R. Blight, "On some Curious Algæ only apparent in times of Drought," by E. Lees, a number of contributions on edible fungi and other mycological subjects, by Dr. Bull and other ardent Herefordshire fungophagists, and a continuation of the notes on "Remarkable Trees of Herefordshire." Geology contributes papers "On the Coralline Formations of the Oolite Rocks, by Dr. Wright; "On the Remains of a Giant Isopod, *Prœarcturus gigas*," and "On *Eurypterus Brodiei*," by H. Woodward and others. Meteorology is represented by useful papers by Mr. H. Southall and Mr. E. J. Isbell. The illustrations are unusually abundant, including several of the fossils described, and photographs of remarkable trees, including one of a new mistletoe oak, which Dr. Bull has had the good fortune to find.

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Science Teaching in Schools—An Offer to the London School Board

MR. JONES's letter on the above subject in NATURE, July 27, has much surprised me, his results being so utterly at variance with those obtained during my own experience, which dates from 1848, and has extended over a considerable area, including Edinburgh, Birmingham, and London.

I have no doubt whatever that Mr. Morris's system may be carried out successfully, provided suitable teachers are selected. There may at first be some difficulty in doing this, the worst rock ahead, that upon which I suspect Mr. Jones's experiment has split, being pedantry. Ignorance is curable, but the pedant only progresses from bad to worse, and the atmosphere of schools and colleges is especially favourable to the propagation of the virulent moral pestilence under which he is suffering.

As a set-off against the discouraging results of Mr. Jones's experiments, I may state that between 1848 and 1854 an experiment of teaching physical, economical, and moral science to children of the poorer classes of all ages between five and sixteen, was carried out in Edinburgh, under the direct supervision of the late Mr. George Combe and Mr. James Simpson. Experimental physics, chemistry, general physiology, and economic science, were taught by myself, while the subjects of the advanced special physiology of the brain and moral philosophy were taught by Mr. Combe. Mr. Combe's class included only the senior pupils of ten years and upwards; my own classes embraced the whole school, and the fundamental principle of the instruction was that of *teaching the same subjects to all the children from the youngest upwards*, by adapting the mode of instruction to their respective ages and capacities.

Of all the numerous subjects thus taught to these children, the one which I found the most difficult and unsatisfactory was that of English orthography, while the easiest were those branches of physical science which I was able to teach with the aid of direct objective illustrations. For example, we had a very good articulated human skeleton, which was an object of great interest to all the children—a sort of pet toy, in fact. I found it much easier to teach to young children between four and five years of age the names of all the bones in that skeleton, than to teach them the names of the letters of the alphabet. The alphabet was a work of many weeks, the skeleton of only a few days. Thus as regards mere names and the recognition of objects, in the first step of intellectual training, viz, the exercise of the senses, science was easier than the first of the "three Rs."

In the next step, viz. the action or uses of the bones and the letters, the advantage of the skeleton over the alphabet was found to be ridiculously great. A very respectable amount of knowledge of animal mechanics was attainable in less time and with less effort than was necessary to enable the children to say with any degree of certainty what *ough* spells, when presented in combination with other letters.

A dissertation on the mode of teaching the elements of the sciences to such young children would be out of place in this letter; I can only summarise the result of my experience by saying that any and every subject that is intelligible to a man of fifty years of age, may be taught to a child of five years of age—taught, of course in its beginnings, and with suitable illustrations.

The sceptical reader will perhaps better understand me when I remind him that simple addition and simple subtraction are the beginnings of the same mathematics as those by which the Senior Wrangler gains his worthily esteemed honours, and that the highest and most difficult problems of pure algebra are but addition and subtraction sums of a more complex character. Thus when a teacher throws six marbles on the floor and tells the children to count them, then shows four more in his hand, and after these have been counted throws them down with the others, and instructs the children to count the sum, and thus proceeds with further exercises upon picking up various numbers, and counting the remainders, he is teaching mathematics as truly as though he were demonstrating the most difficult problems of the differential and integral calculus. It is in this sense that I speak of science teaching to such young children, and in such a manner any and every branch of science may be taught simultaneously with the alphabet.

Many very sincere friends of education, resident in Edinburgh at the time above stated, were unconvinced of the possibility of thus communicating sound scientific knowledge to children, and in the course of an address on education delivered by Mr. George Combe, he made the following offer, viz.:—That the audience then present, consisting chiefly of artisans, should send to me on the following day some of their children, between ten and twelve years of age, that I should take the first twelve who presented themselves, and at once commence a course of ten or twelve lessons on physiology, at the end of which course the children should be publicly examined on the subject of the teaching.

The experiment was carried out successfully, a large audience assembled at the examination, and many were much surprised at the result, though there was really no good reason for astonishment, the attainments of the children being merely a natural and necessary result of plain unpretentious teaching of the simple and fundamental elements of a subject in which every human being is interested.

If the London School Board think it desirable, I shall have much pleasure in repeating the experiment. About twelve children, of nearly equal ages, taken at random, street Arabs if they please, may form the class. The materials I shall require are a skeleton and a set of Marshall's Physiological Diagrams. After ten or a dozen lessons of about one hour each, I will orally examine the children in any building or before any audience they may select, large or small.

To test the possibility of teaching another class of subjects, that of physiology might be followed by a similar number of lessons on that part of economic science which includes the natural laws upon which the relations between capital and labour, and some other fundamental elements of our social structure, depend. The examinations would be so conducted as to afford to all who attend them the means of judging whether the children had been crammed or truly taught, whether they would be likely to remember or forget the subject of their lessons, and how far this preliminary glimpse of the wonderful work they are themselves able to perform, might stimulate their intellectual appetite and awaken a slumbering sense of their own human dignity and responsibility.

W. MATTIEU WILLIAMS

The Green, Woodside, near Croydon

### Cramming for Examinations

I ENCLOSE one or two *bonâ fide* extracts from *elementary* examination papers which have during the past few years come under the notice of candidates officially. I do not wish thereby to reflect so much on the regulations drawn up by senates and committees, as upon the way in which those regulations are carried out by examiners. Though cramming is officially denounced, yet there is scarcely anything which is in greater demand; and, so long as this is the case, candidates will of course insist, in spite of their teachers, upon undergoing the operation.

There are few of the matriculation papers of the London University but proclaim cramming to be the order of the day. The papers in Chemistry cannot certainly be called very difficult;

this, however, is not surprising, since, for many years, the University has had for examiner one of our ablest chemists and a most eloquent teacher. Nevertheless, observe the following questions, in which the italics are mine :—

"*Describe by equations* as many processes as you know for the preparation of oxygen gas." (1870.)

"*Explain by an equation* the process of making ammonia, &c." (1870.)

"*Give the names and formulæ* of the oxides of nitrogen, &c." (1870.)

These few are the *worst* detected after careful search; but columns of NATURE could be filled at once with the most unnatural questions in all the other subjects. The following, taken at random, will serve as brilliant specimens; to me they are more heart-rending than the answers given by "Examiner," because even the worst of candidates are corrigible, while examiners do not appear to be so.

"Name the Sovereigns who were reigning in England at the close of each century from the ninth to the eighteenth successively." (1870.)

"Give some estimate of the population of England at the death of Charles II., &c." (1870.)

"Show how the present Royal Family is connected with the House of Tudor, tracing the pedigree to the end of the seventeenth century." (1869.)

"State the principal rules of English syntax." (1869.)

Moreover, candidates are positively compelled to cram their Latin and Greek translation; the one Greek and the one Latin subject are selected "one year and a half previously," which makes competition of talent against talent so far practically an impossibility; it is a mere trial of cramming against cramming. And is this portion of the examination of any practical value as proving the efficiency of a candidate? Let the university answer for itself: "Special stress is laid on accuracy in the answers to the questions in Greek and Latin grammar." Comment is useless.

A much lower standard for Latin translation, and no selection one year and a half previously, would ensure a finer and more useful knowledge of a noble language; besides this, a little rational conduct on the part of examiners, and a far more vigorous and effective supervision of the papers by the Committee of the Senate, would enable education to go hand in hand with instruction, and learning to part company with cramming.

Most of us know what school training should be; it should be such as would enlarge the mind, make it capable of comprehending the great and good, and open up a vista of happiness in early years. Teachers know what school training must be; it must be such as will satisfy inexorable examiners, many of whom appear to be totally unmindful, not only of what should, but of what can, be taught during an ordinary boy's school life.

To one who regards education as the only means of placing man "a little lower than the angels," the questions given at elementary examinations are more than painful.

TEM. AUG. ORME

University College School

### Volcano near Celebes

THE following note may be perhaps of interest for the readers of NATURE. March 2.—The Volcano Roeang, near Tagoelanda, the most southern of the Sangi Islands in the North of Celebes, began to make noises. March 5th.—In the evening, at seven o'clock, a frightful eruption took place; three minutes afterwards a large sea-wave reached the shore of Tagoelanda, about one mile distant from Roeang, and destroyed three villages with 416 men. The mountain worked till March 14, with a heavier final eruption. March 30.—I was at the place and ascended the volcano, which is, according to my measurement, about 2,100 feet high. To proceed into the crater was impossible in consequence of the thick damps of sulphur.

The temperature of the soil at the bottom of the mountain near the sea-shore some inches deep was 45° Réaumur. I brought home a large collection of stones, &c.; the masses thrown out were principally sulphur, ashes, sand, and mud, besides small and large stones, and even rocks. All details are contained in my diary. I then made a tour round the Sangi Islands, and am about at this moment to visit the isles of Bangka and Limbe in the north and east of North Celebes.

ADOLF BERNHARD MEYER

Manado, Celebes, April

## NOTES

VICE-ADMIRAL E. OMMANNEY, C.B., F.R.S., proceeds to Antwerp to represent the Royal Geographical Society of London at the Congress of Geographical Science, which will be held in that city between the 14th and 22nd of this month.

MR. W. CARRUTHERS has just issued his official report for 1870 of the Botanical Department of the British Museum. Several of the Natural Orders and European and British representatives of other orders have been completely rearranged. The most important additions which have been incorporated into the herbarium during the year are: from Formosa, collected by the late Mr. Oldham; from the Levant, 2,625 species by Prof. Haussknecht: from Martinique, by M. Hahn; and from various districts of Europe. A large number of fungi have been added from Europe, and from North and South America and Cuba, and among other palæozoic additions, an important series of Devonian plants from Canada, presented by Principal Dawson, of Montreal, illustrating his published memoirs.

THE Monthly and Annual Reports have reached us of the Department of Agriculture of the United States of America for 1868 and 1869. The amount of information which is thus afforded by the Government to the citizens of the United States, may well astonish us in this country. They comprise Reports from practical men on a vast variety of subjects of the utmost importance to the cultivators of the soil: the cultivation of fruit; the manures best adapted for different soils; report of recent progress in steam culture; meteorological statistics; the physiology of *Trichina spiralis;* abstract of laws relating to fences and wild stock; tests for the authenticity of seeds; agricultural statistics; report of progress of beet-sugar manufacture in Europe, *et multa alia*.

THE discussion which took place before Parliament relating to the adoption of the metric system in England, is considered by French *savans* as highly discreditable to that body, and the result has occasioned much surprise there.

M. BRETON, one of the great Hachette firm, was returned a member of the Municipal Council of Paris in the Conservative interest. His majority was one vote, which was declared *nil*, as a man had voted without any right, and in spite of the exertions of the chairman of his voting section. Being older than M. Hérisson, his competitor, he was elected merely by the privilege of seniority. Two other publishers who had been candidates were unsuccessful, M. Gernier Baillière and M. Victor Masson.

NO stamp duty is to be imposed on newspapers in France, but a duty will be established on every description of printing-paper. For books it will 8*s*. per cwt. and for newspapers 16*s*. per cwt. Newspaper paper is of an inferior description, and will be charged twice as much as the superior kind. This absurdity is owing to the objection raised to the income-tax by several politicians, amongst them M. Thiers himself. But it is supposed he will very shortly give up his old prejudices.

MR. CARRUTHERS, the keeper of the herbarium at the British Museum, has been appointed consulting botanist to the Royal Agricultural Society.

ONE of the Whitworth Scholarships has recently been awarded to John Armitage, an artizan student at the Oldham School of Science and Art; last year he gained the Department Silver Medal for Practical Geometry, and in 1869 the silver medal for Machine Drawing. Last year James Taylor, another artizan student from this school, also gained a Whitworth Scholarship, as well as the Department Gold Medal for Mathematics and the silver medal for Theoretical Mechanics.

A LAUDABLE attempt to encourage floriculture in London is an exhibition which was held yesterday in the churchyard of St.

Botolph, Bishopsgate, under the patronage of the rector, Rev. W. Rogers, in which prizes were offered for the best collection of flowers grown in the City.

AN exhibition of the Royal Cornwall Polytechnic Society will be held at Falmouth from the 11th to the 19th inst.

THE *Revue Scientifique* for August 5 contains a report of a very interesting lecture delivered before the Collège de France by M. Claude Bernard on the Influence of Heat on Animals, accompanied by a series of very careful experiments.

WE have received letters from a number of correspondents on the various subjects opened out by Mr. Howorth's "New View of Darwinism," and the replies to it; but the great pressure on our space compels us to close the discussion.

THE most recently received parts of the Bulletin de l' Académie Impériale des Sciences de St. Petersbourg, viz., vol. xv. part 3-5, and vol. xvi. part. 1, contain among others the following important papers:—Note on the Approximate Rectification of Certain Curves, by J. Somoff; On Tremblings of the Earth, by F. Argelander; Observations on the Planets at St. Petersburg, by A. Sawitsch; On the Physical Properties and Calorific Power of certain Petroleums of the Russian Empire, by M. Sainte-Claire Deville; On the Nervous System of Star-Fish, by Ph. Owsiannikof; Studies of Ozone, Oxygenated Water, and Ammonium Nitrite, by H. Struve; the Nervous System of *Lepas anatifera*, by Dr. E. Brandt; On Polydactylism, by Dr. Gruber; Short Diagnosis of New Plants from Japan and Mantchuria, by C. J. Maximowicz; On the Young of *Idothea entomon*, by E. Brandt; On the Gulf Stream to the East of the North Cape, by A. Middendorff; On the Osteology of the Hand and the Foot, and other Anatomical Papers, by W. Gruber; Rotation of the Plane of Polarisation by the Effect of Electro-Magnets, by Jegorof; On the Organisation of Gregarinidæ, by A. Stuart; Histological Studies on the Nervous System of Mollusca, by Ph. Owsiannikof; On Cerium, by D. Mendéleyf; On the Influence of the Displacements of the Axis of Rotation in the Interior of the Earth or the Level of the Sea, by Dr. H. Gylden.

A SECOND edition is just published of Prof. Corfield's Digest of Facts relating to the Treatment and Utilisation of Sewage. It has been revised throughout, and is issued entirely on the author's own responsibility, and not under the auspices of the British Association Committee. Considerable and important additions are made to the matter contained in the first edition.

THE second volume of the "Flora of Tropical Africa," containing the orders Leguminosæ to Ficoideæ, has just been issued. The work has been divided as follows among the botanists whose names are attached to the respective orders: Leguminosæ (Cæsalpinieæ and Mimoseæ), Rosaceæ, Saxifrageæ, and other small orders, Prof. Oliver; Cucurbitaceæ, Begoniaceæ, and Melastomaceæ, Dr. Hooker; Leguminosæ (Papilionaceæ), Mr. J. G. Baker; Passifloreæ and Samydeæ, Dr. M. T. Masters; Combretaceæ and Myrtaceæ, Prof. M. A. Lawson; Crassulaceæ, Mr. James Britten; Lythraceæ, Mr. W. P. Hiern. The proportion of new species described is very large.

THE Coal Commission appointed on June 28, 1866, "to inquire into the several matters relating to coal in the United Kingdom," have unanimously agreed to their report. The whole work of the Commission will be published as soon as possible in three volumes, with maps, sections, &c., which are all far advanced towards completion.

M. PANCERI, in a memoir recently presented to the Association of Naturalists and Physicians at Turin, claims to have established that the phosphorescent substance in fishes, in whatever part of the body it may be situated, is always fat, and that the phenomenon is due to its slow oxidation in contact with air. The skin of fishes is permeable to gases, and the oxidation of the sub-cutaneous fat proceeds without difficulty. Phosphorescence shows itself, as a rule, some time after death, and continues until putrefaction commences; as soon as a true decomposition sets in, accompanied by the disengagement of ammonia, phosphorescence ceases. Phosphorescence is prevented by the presence of fresh water, alcohol, or carbonic acid; oxygen, on the other hand, strengthens the phenomenon.

THE charge against Mr. Hampden of libelling Mr. Wallace was tried on Thursday week in the Secondary's Court. Our readers will remember that a wager of 500*l*. having been made between Mr. Hampden, who affirmed that the world was flat and not round, and Mr. Wallace, it was decided against the former, who thereupon abused Mr. Wallace as a liar and a swindler. The action now tried was for damages for these and other similar libels, and Mr. Hampden was condemned to pay 600*l*. damages.

MR. HELIODORO RUIZ, of Opin, in Colombia, New Granada, informs the Government ot that country that he has been successful in treating snake bites by cauterisation. The province abounds with snakes of a deadly character, and he has treated seventy cases of bites. He simply drops melted sealing-wax on all the fang marks, and he considers the result is due less to cautery than to the complete exclusion of the air by the adhesion of the wax. At first he administered internally a few drops, but he has discontinued it, not finding it necessary.

SENOR PRIMO LOZANO, of Quibdo, in Colombia, reports to the Tiempo of Bogota that he has discovered a new way between the Atrabo and the Pacific superior to that by Napipi and Truando. The Napipi route has been repeated again by the U.S. explorers.

ON the 25th May a waterspout passed over the hill stations of Ootacamund in Southern India.

ON the 23rd May an earthquake was felt at Nynee Tal in the Himalayas. At that English hill station there is a beautiful lake, and it is now to be noted that since the earthquake it has emitted a strong sulphurous smell.

AN earthquake was felt on two days in May at Gilghit, above Cashmere, on the 22nd and 23rd.

ON the night of the 7th of June Calcutta was visited by one of the severest thunderstorms known for many years. Several houses were struck by lightning in the southern division of the city, but there was no loss of life.

ON the 19th June a strong earthquake was felt at Brooklyn and in the neighbourhood of New York. The shock was vertical.

THE largest tamarind tree in India, in the Khosru Gardens at Allahabad, fell down on the Queen's birthday. The stem was quite hollow. It was an ancient and well-known sight.

IRON telegraph poles have been introduced with great success in Switzerland, and their use is now being extended daily. It is considered that in a short time these iron posts will altogether replace wooden poles throughout Germany. We understand, also, that they are being largely adopted in connection with the Indian telegraph service.

A SPECIES of fish-crow (*Corvus caurinus*) is very abundant in the Oregon and Washington territories, where it is very troublesome to the Indians, stealing their dried fish and other provisions. It is never killed by them, from superstitious feelings, but is driven away by children set to watch for that purpose. In winter it subsists principally upon the refuse food and offal thrown out by the natives from their lodges, and is an attentive hanger-on at the residences of the white settlers. It is cunning, but very tame and impudent, allowing a very near approach, and when closely pursued retiring but a short distance. Like some species of gull, this bird is in the habit of carrying clams high in the air and then dropping them, in order to break the shell. Dr. Studley says: "In watching one thus employed I was very much amused at the unsuccessful endeavours he made to break the shell of a clam by letting it drop upon soft ground. He continued for a long time carrying and recarrying the same clam high aloft and fruitlessly dropping it on the prairie sod. He nevertheless persisted in his efforts until I became tired of watching him. What the result was I am unable to state."

## *THE BRITISH ASSOCIATION MEETING AT EDINBURGH*

EDINBURGH, *Wednesday Morning*

THE proceedings of this year's meeting are now rapidly drawing to a close, so near, in fact, is the end, and so apparently far back in time is the beginning, that already it is easy to sum up the results, and to get a general view of the meeting in its various aspects.

The meeting has certainly been in every respect a most successful one; the weather has done all in its power to conduce to the enjoyment of the members, and to belie the southern notions as to climate. First, as to the attendance: The numbers attending the former meetings here have been almost, if not quite, doubled, and, in fact, we may assume that they have been much larger than was expected, otherwise another, though in some respects a less convenient arrangement for the sectional work would have been adopted. As it is, we have had all the sections massed in the University in the various classrooms—an arrangement which reduces the necessary locomotion to a minimum, and gives the greatest facility to those who choose to visit all the sections, affording a striking contrast to the great waste of time and other inconveniences which resulted from the disconnected positions of some of the sections at Liverpool. The drawback is, that the numbers being so large, the small classrooms have quite broken down in the matter of accommodation, and ingress and egress have been almost impossible.

But in British Association Meetings, as in other things, numbers alone must not be too much considered; and this leads us to the wonderful galaxy of physicists who apparently have come to Edinburgh to do honour to the President. The brilliancy of the gathering, both of British and Foreign men of science in the mathematical and physical section, has been the subject of general remark, and we refer to it, not at the expense of the other sections, but as an indication of what has happened there also, though not to such an extraordinary degree. Joule and Colding, Cayley and Sylvester, Thomson and Tait, Janssen and Huggins, Clifford and Spottiswoode, are combinations not to be seen every day, and the extreme interest of the discussions carried on under such conditions may be easily imagined, much more easily imagined, indeed, than described. Prof. Zenger, of Bohemia; Dr. Paul Güssenfeldt, of the University of Bonn; Prof. Van Beneden, of Louvain; E. L. Youmans, of New York; Rev. J. R. Loomis, LL.D., President of University of Lewisburg, U. S.; Prof. Dr. E. H. von Baumhauer, Secretary of the Dutch Society of Science, Haarlem; Dr. C. H. D. Buys Ballot, of Utrecht; C. Gilbert Wheeler, Professor of Chemistry at the University of Chicago; Dr. Baron R. Eötvös, Professor of Mathematics and Physics at the University of Pesth; Dr. D. Bierens de Haan, Professor of Mathematics, Leiden; are among the foreigners who have attended the meetings in addition to those alluded to last week; not to mention the names of many distinguished English and Scotch *savans*, who attend as representatives of various scientific bodies in different parts of the country.

Then as to the number of papers presented. With the exception of the Mechanical Section, presided over by Prof. Jenkin, the supply of papers has been superabundant, with a quality above the average. So numerous have been the papers in some sections, that divisions have been formed to enable them to be get through.

As to the local conditions of success, we need only say that the meeting is in one of the most beautiful cities of the world, the society of which takes its tone from a wide diffusion of intellectual culture, and where hospitality takes no refusal, and just escapes killing by kindness.

This meeting may be said to have really commenced on the day before the meeting of the General Committee, and of the delivery of the President's address, in consequence of the attendance of so many men of science at the graduation ceremonial of the University, to which we referred last week, when we gave the names of those who had the honorary degree of LL.D. conferred on them. We may here add that the recipients were introduced by Prof. Macpherson, the Dean of the Faculty of Law, in a way which greatly enhanced the value of the honour.

This ceremony was followed by the "capping" of ninety-six gentlemen who had just completed their studies. One of the secrets of Edinburgh's great success as a medical school appears to us to lie in the mode of bringing out originality among her students, which we would gladly see adopted in the science teaching of our English Universities. When a student takes the degree of M.D., he is required to write a thesis on some subject belonging to the sciences related to medicine. Gold medals are awarded for such theses as contain an amount of original work which is deemed worthy of the honour. The consequence of this is, that every year two or three, or even more, really good original memoirs are produced. These are, in very many instances, the nuclei of still greater things in after-life. Powers of research, which might otherwise have lain dormant, are brought out; and so at an early period of life, men get into the habit of doing original work. We are assured by Edinburgh men that the system is, as one would have expected, fraught with excellent results. Our wealthy English universities would do well to take a lesson from their poorer sister; and, instead of rewarding so highly mere grinding in science, they would do well to do something more to develop doers of original scientific work. On this occasion the gentleman who obtained the highest honours is Dr. Urban Pritchard.

This ceremony, after all, however, was merely the prelude. Prof. Bennett's graduation address was the first sensation of the meeting. Indeed, those who had come north with more acquaintance with Dean Ramsay's stories than with the present tone of thought, were simply astonished at the boldness with which Mr. Bennett handled subjects on which, it was imagined, any expression of opinions such as his would not be tolerated. Here, for instance, is a specimen:—

"At the congress of naturalists and medical men held at Innsbruck in 1869, Helmholtz claimed for Germany the principal agency in the progress of modern science. She owes this superiority, he said, to the boldness of her *savans* in propagating truth, whilst, he asserted, that in England and France they dare not do so openly, for fear of compromising their social interests. But I trust the time is past, even in Scotland, when scientific truth has anything to fear from superstitious bigotry or clerical intolerance. It is true that we are constantly hearing that there is a tendency to place new scientific doctrines in opposition to religious beliefs. But I would suggest that the cause of this is not that scientific men are irreligious, so much as that religious men are unscientific. It is utterly impossible, in these days, to oppose the most obvious facts, or persecute the great discoverers of the day, because the writers of the Old and New Testament, 1,800 or 3,000 years ago, knew little of astronomy, chemistry, and physics. Such, however, has been the unfortunate policy of the Church for many centuries. I need not remind you that the great Galileo died a prisoner of the Inquisition, and that Servetus was publicly burnt in Geneva, by the authority of Calvin. The true cause, unquestionably, of the present chasm in thought which divides the literary and religious from scientific men is, that the former have been bred up in ignorance of physiology, that is, of all that relates to their own bodily structure, functions, and requirements. Unfortunately, their education causes in them a want of appreciation and an incapacity of comprehending scientific truths. . . . . Clergymen and most religious teachers are totally insensible to the errors and discrepancies of language they use in the pulpit; so that, when the scientific man takes his place in church, he is surprised at the manifest ignorance of established truths constantly preached to the people."

The main object of the lecture was to insist upon the fact that physiology in some form or other should constitute a part of the education of every one. A Committee of the British Association for the Advancement of Science strongly recommended it in 1868; and wherever it has been tried it has been attended with marked success, especially in girls' schools, and to illustrate this point Mr. Bennett showed how, adding that "Perhaps women in all classes and degrees of society have more to do with the preservation and duration of human life even than men; and in all ranks of society should have physiology taught them. It should be an essential subject in their primary, secondary, and higher schools. So strong are my convictions on this subject, that I esteem it a special duty to lecture on physiology to women, and whenever I have done so, have found them most attentive and interested in the subject, possessing indeed a peculiar aptitude for the study, and an instinctive feeling—whether as servants or mistresses, wives or mothers—that that science contains for them, more than any other, the elements of real and useful knowledge. In advocating the propriety, therefore, of introducing physiology as an essential part of education to all classes of society, I would observe in the last place, that when you enter upon the duties of your profession, you will find too frequently that your best efforts are frustrated by parents, nurses, or attendants on the sick, who, not comprehending, are therefore incapable of carrying out your instructions. I have myself seen, only too frequently, the most melancholy deaths produced in families, and extreme wretchedness occasioned, from carelessness or ignorance of what ought to be done—arising entirely from an unacquaintance with the most common rules requisite for the preservation of life."

It is a strange rider to this to add, that the University here has just by its vote rendered the higher education of women in these subjects impossible for the present so far as Edinburgh is concerned, though it is fair to remark that the majority was so narrow that it is not too much to hope that ere long this decision, which is eminently to be regretted, will be reversed.

At the meeting of the General Committee on Wednesday, the reports of the Council, in which they gave an account of their stewardships for the past year, and the report of the Kew Committee, were read. It is not necessary to give either of these documents *in extenso*, but the following references to them may be useful. The connection between the Association and Kew Observatory is to cease, and the Government is to be informed of the Association's desire to see its direction and maintenance transferred to the Royal Society, who will administer the means placed at the disposal of science by the munificence of Mr. Gassiot. Dr. Hirst has resigned his office as joint general secretary, and Mr. Douglas Galton, C.B., F.R.S., has been elected to succeed him. Those who know Mr. Galton will heartily congratulate the Association on his willingness to undertake the duty. Prof. Van Beneden, Dr. Crafts, Dr. Anton Dohrn, Governor Gilpin, of Colorado, H.H. the Rajah of Kolapore, M. Plateau, and Prof. Tchebichef have been added to the list of corresponding members. The consideration of some revised regulations drawn up by the Council for regulating the proceedings of the several sections was postponed for a future meeting.

An important recommendation has been urged by the committees of the Biological and of the Geological sections, which is likely—if accepted by the Council—to increase much the scientific value and interest of the meetings of the Association. It has been recommended that, in addition to the various rooms provided for the meetings of the sections, sale of tickets, &c., a room be annually provided for the purposes of a temporary museum. It cannot be doubted that such a museum would be a great success. In the meetings of the British Medical Association and the Archæological Association similar museums are very important features of the proceedings. A good-sized room, provided with a number of glass-cases arranged on tables, such as are always to be hired in large towns, would constitute the machinery of the museum. One or two reliable members of the Association would have the management of it, and exclude undesirable or worthless objects, whilst whipping in all of special interest; members would bring new and rare geological specimens, zoological specimens, human crania, flint-weapons, physiological apparatus, chemical apparatus, and microscopes, which would all be arranged judiciously and ticketed. We have no hesitation in saying that such a museum, when once brought into working order, would be the greatest attraction of the meeting. The proposal was originated by Mr. Ray Lankester.

Thanks to the exertions of Dr. King, who urged strongly the formation of a separate section for Ethnology, the meeting of the Committee was not altogether dull, and this gentleman, who is a born Irishman, if not an Irishman born, fairly convulsed the Committee by his method of appeal. First he urged that there should be a separate section, because the Queen and Prince Consort "had come in their yacht to visit all the sections" at the Southampton Meeting. Next he complained that at Exeter the ethnologists "were put into a room which would not hold them," but the appeal was unavailing, Prof. Huxley's *quietus* came in due time, and the matter—and Dr. King—dropped.

The definite acknowledgment of Anthropology as a department of the Biological Section of the British Association, has led to the admission of a wide range of subjects in that department. "What is man?" is a question which cannot be answered by comparative anatomy alone. Dr. Tristram proposed in committee that Psychology be recognised as a distinct branch of Anthropology. This proposal was overruled by the declaration of the president, that man as a compound being could not be discussed apart from the psychological aspects of the question.

The Anthropological Department has, consequently, been flooded by papers of the most controversial tone on this side of the investigation of humanity. The most provocative papers on the subject were those of Mr. Staniland Wake, on Man and the Ape, and of Mr. Kaines, on the Anthropology of Comte. Both these papers are vigorously attacked on the Psychological side; the opponents of Positivism taking their stand on the contemptuous rejection of metaphysics by the writers. But the Positivist papers necessarily invoked the theological element, as they assumed at the outset that the whole metaphysical side of the question must be expunged, as being a question of which physicists were incompetent to judge. This led to as universal an affirmation of the tripartite nature of man, by various speakers, led by Mr. Boyd Dawkins, and the impossibility of admitting the premises of the writers on his origin until the origin of his spirit had been demonstrated to be material.

Among the topics of general conversation during the first part of the meeting, have been the proposed dredging exploration, which it is understood will be undertaken by the Government, following the example set by the American, Swedish, and other nations, and the proposed Eclipse Expedition to Ceylon next December. The former announcement has been hailed with the liveliest satisfaction; and the Government is on all hands congratulated on its appreciation of the importance of this work. The feeling touching the Eclipse Expedition is of an entirely opposite character, as it has leaked out that this year, as last, affairs have been delayed and badly managed. After Messrs. Lockyer's and Janssen's papers on Friday, Sir William Thomson said he joined very warmly in what Mr. Lockyer and M. Janssen had urged. M. Janssen had asked that Britain should join France and Germany in this friendly struggle, and it would be a disgrace to England if it did not accept that challenge, and do its very best to beat both France and Germany in the struggle, adding that all the efforts of all the nations would not be too much for the importance of the work. The *Scotsman*, in a leading article on this subject, after urging an appeal to Government on the part of the British Association, writes as follows:-

"The Chancellor of the Exchequer, in fact, who is *de facto* the keeper of the nation's purse, is *de jure*, so far as science is concerned, the keeper of the nation's honour; and may the time be long distant when the honour of England shall be tarnished by her relinquishing those expeditions and scientific explorations to the precursors of which we all look back with so much pride. Surely, from this point of view, it should be a subject of regret to the leaders of science now among us that the progress of the nation's best interests should be liable to be thwarted by the jealousies and self-seeking of individuals, and we are glad to learn that the action of the British Association, which we are informed becomes necessary in consequence of some such cause as this, is likely to be carried forward with such vigour that Her Majesty's Government will willingly yield to the demands of science, while at the same time a salutary lesson will be read to those who attempt to make the progress of science—the national importance of which is thoroughly acknowledged here—subservient to their own selfish interests. We have been the more anxious to make these remarks, because we think the time has arrived when the general interests of science and truth demand that any effort, by whomsoever made, to retard the progress of knowledge, should be publicly met without respect of persons and without hesitation; and we may express a hope that the Parliament of Science, now assembled in this city, will counteract the efforts of an oligarchy in the same bold manner as the Parliament of the nation has recently done." In these remarks we cordially concur.

We may dismiss this subject by stating that an application for aid is to be sent off to the Government to-night.

The President's address, delivered in the evening in the Music Hall, was received with enthusiasm. The Emperor of Brazil, who seems to have come over to this country to show how easily our own rulers might further the progress of science if they chose, occupied a seat on the platform, which was as crowded by the general committee as the body of the Hall was by the ordinary members. Prof. Huxley, in resigning the presidential chair to Sir William Thomson, reminded his auditors of the achievements of the new president, which in this age of cultivation of science and in the pressing rivalry of able and accomplished men in all directions, entitled him to the appellation of an "intellectual giant," adding, as the poet says of Lancelot,—

Gentler knight
There never broke a lance.

On the morrow the sectional work began in real earnest, and has continued with but small interruptions ever since—the interruptions consisting in excursions on the Saturday, by which the geological, chemical, and botanical sections protested against that rule of the Council which attempts to discountenance such blandishments during the Association, forgetting, as it seems to us, the extreme value of local inquiries which it is impossible to carry out otherwise, as every moment is so fully occupied. Our notice of the sectional work may here be very brief, as we shall give in their proper places notices of all papers of importance or interest.

After the reading of the addresses, in Section A Dr. Carpenter made an interesting communication with reference to oceanic currents. Sir W Thomson and Prof. Stokes joining in the discussion, which was followed by a paper by M. Janssen on his balloon experiences. Among the papers submitted to the Chemical Section the most popular was perhaps one relating to the working of hæmatite ore. The Geological Section had some papers of local interest, as also a report on Scotch earthquakes. Of the zoological papers, a report from the Close-Time Committee, and a paper on the rarer raptorial birds of Scotland, gave rise to a discussion on the extirpation of indigenous animals. This was followed by an important paper on co-operation among natural history societies. The Anthropologists discussed such subjects as longevity, and the degeneration of race in Britain; the Geographers received notes of researches in various parts of the world; and among the subjects taken up in the Economic Science Section was that of the Merchant Company's schools.

On Friday the proceedings in Section A were opened by papers by Mr. Lockyer and M. Janssen on the recent and coming eclipses. The Chemical Section had, among other papers, a report on recent progress in chemistry in the United States. The geologists received a report on the exploration of Kent's Cavern, besides papers detailing the results of researches in various departments of the science. The Anthropologists discussed, among other subjects, that of ancient hieroglyphic structures. In the Biological Department, spontaneous generation formed the subject of a small discussion between Dr. Calvert and Dr. Bastian, and an important paper was communicated by Prof. Thistleton Dyer on mimicry in plants. The geographical programme included papers on the geography of Moab and the famous Moabite stone. In the Economic Section a lively discussion took place on the Merchant Company's Education Scheme, introduced by Mr. Boyd's paper of the preceding day.

On Saturday, Monday, and yesterday, the flow of papers still continued, the Anthropological Section soon becoming notorious for actual or probable rows, though nothing very serious took place. The questions of state aid to science, and obstacles to science teaching in schools, were discussed yesterday in Section A, and here our notice must stop.

To-day we have the final General Meeting, and as many of the recommendations which have been made during the meeting will be discussed there, it will be well to delay our notice of them till next week, merely remarking here that we never knew a larger number of valuable recommendations made for action or money grants. In the meeting of the General Committee on Monday, Bradford was fixed upon as the next place of meeting after Brighton, with Belfast in reserve for the year after. The appointment of Dr. Carpenter as next president was moved in a highly eulogistic speech by Prof. Huxley; the officers of the Association were re-elected with the exception of Dr. Hirst, who, as before stated, is succeeded by Mr. Douglas Galton; and the following Council was appointed for the ensuing year: Messrs. Bateman, Beddoe, Debus, Fitch, G. C. Foster, M. Foster, F. Galton, Gassiot, R.A.C., Godwin-Austen, Huggins, Gwyn-Jeffreys, Lockyer, Merrifield, Ramsay, Simon, Tyndall, Wallace, Williamson, Sir Stafford Northcote, Sir Charles Wheatstone, Colonel Strange, Colonel Sykes, and General Strachey.

The lectures and conversaziones have been great successes, the former we hope to be able to give at some length next week. We must not conclude this letter, written from Edinburgh—the den of the great "Red Lion" Forbes—without adding that the Red Lions dined together on Monday, Lion King Rankine occupying the chair.

The following papers were contributed to this section by unknown authors:—

TO THE CHIEF MUSICIAN ON NUBIA

A TYNDALLIC ODE. *Tune:* "THE BROOK"

I COME from fields of fractured ice,
Whose wounds are cured by squeezing,
They melt and cool, but in a trice
Grow warm again by freezing;
Here in the frosty air the sprays,
With fern-like hoar frost bristle,
Their liquid stars, their watery rays,
Shoot through the solid crystal.

I come from empyrean fires,
From microscopic spaces,
Where molecules with fierce desires
Shiver in hot embraces;
The atoms clash, the spectra flash,
Projected on the screen,
The double D, Magnesian b,
And Thallium's living green.

This crystal tube the electric ray
Shows optically clean,
No dust or cloud appear—but stay:
All has not yet been seen;
What gleams are these of heavenly blue,
What wondrous forms appearing?
What fish of cloud can this be, through
The vacuous spaces steering?

I light this sympathetic flame,
My slightest wish to answer,
I sing, it sweetly sings the same,
It dances with the dancer;
I whistle, shout, and clap my hands,
I hammer on the platform,
The flame bows down to my commands
In this form and in that form.

THE BRITISH ASS

(*Sung by a Cub at the Red Lions' Feed, Edinburgh, August* 7, 1871)

*Air:* "THE BRITISH GRENADIERS"

SOME men go in for Science,
And some go in for Shams,
Some roar like hungry Lions,
And others bleat like Lambs;
But there's a Beast that at this Feast
Demands a special glass,
So let us bray, that long we may
Admire the British Ass!
With a tow, row, row, &c, &c.

On England's fragrant clover
This Beast delights to browse,
But sometimes he's a rover
To Scotland's broomy knowes;
For there he finds above all kinds
The Plant that doth surpass
The Thistle rude—the sweetest food
That feeds the British Ass!

We've read in ancient story
How a great Assyrian swell
Came down from all his glory
With hornèd beasts to dwell.
If you would know how it happened so,
That a King should feed on grass,
In Section D, Department B,
He had joined the British Ass!

On Grecian senses charming
Fell the music of the spheres,
But voices more alarming
Salute our longer ears.
A swell profound doth now propound
How life did come to pass,
From world to world the seeds were hurled,
Whence sprung the British Ass!

In our wandering through Creation
We meet these burning stones,
That bring for propagation
The germs of flesh and bones.
And is it not a thrilling thought
That a huge misguided mass
Will come some day to sweep away
Our dear old British Ass!

The child who knows his father
Has aye been reckoned wise,
But some of us would rather
Be saved that sweet surprise,
If it be true that when we view,
A comely lad or lass,
We find the trace of the monkey's face
In the gaze of the British Ass!

SECTION A.

THURSDAY, *Aug.* 3.—*Speculations on the Continuity of the Fluid State of Matter*, by Prof. James Thomson, of Belfast. The author proceeding from the researches of Dr. Andrews on the Continuity of the Liquid and Gaseous States of Matter, in which it has been discovered that there is gradual transition between the ordinary liquid and the ordinary gaseous states of the same matter by courses passing through temperatures and pressures above those at which boiling can take place, showed that there is probably also a theoretical continuity having a real and true significance directly across temperatures and pressures of boiling points. This he showed by supposing there to be conditions partly stable and practically attainable, and partly unstable, corresponding to curved reflex junctions of the curves shown by Dr. Andrews for the gaseous and liquid states,* where they are interrupted at the boiling breach of continuity. As these new views of Prof. Thomson form the subject of a paper submitted to the Royal Society and intended to appear in an early number of the Proceedings, we hope to give a fuller account of them in a future issue.—Prof. Thomson also drew the attention of the Section to the existence for each of the various substances, (water, or carbonic acid, for instance,) of a remarkable point of pressure and temperature, at which alone the substance can exist in three states, *solid*, *liquid*, and *gaseous*, together in contact with one another. This point of pressure and temperature he designates as *the triple point*; and he shows how this point belongs to three important curves, as being their intersection. On this subject also we propose soon to give a fuller exposition of Prof. Thomson's views.

SECTION B.

ON Thursday, after the address of the President, Dr. Andrews, which has already appeared in our columns, Mr. Dewar presented his *Report on Thermal Equivalents of the Oxides of Chlorine*. The results were merely preliminary, and exhibited in a remarkable manner the difficulties attending this class of investigations. Dr. Gladstone followed with a paper, which he had prepared in conjunction with Mr. Alfred Tribe, *On Some Experiments on Chemical Dynamics*. He commenced by referring to a paper recently communicated to the Royal Society, in which it was shown that in various decompositions of metallic solutions the chemical change, in a given time, is not in proportion to the amount of salt present, but that twice the quantity gives three times the chemical action, and also that while silver is deposited in copper, in the decomposition of nitrate of silver by copper an actual passage of the nitric element towards the copper plate occurs.

In the present paper, the authors exhibited this latter phenomenon in a dissected form, with other observations. A copper plate was immersed in copper nitrate, and a silver plate in silver nitrate; while the two metals were connected by a wire, and the liquids by a porous cell. Silver deposited upon the silver plate, and the copper plate dissolved; the sp. gr. of the copper nitrate increased from 1·015 to 1·047, and only a trace of this salt passed into the cell which originally contained silver nitrate. The passage of $SO_4$ ($SO_4 H_2$?) was also found to take place by an analogous experiment.

Similar experiments were made in which the nitrate of silver was kept constant, but the nitrate of copper was increased in equivalent multiples. It was found that the silver deposited increased with the increase in copper salt, being about double when the copper salt was seven times as strong, and that the effect of successive additions gradually diminished. This is in strict accordance with other experiments showing that when the copper plate is immersed in a mixture of the nitrate of copper and silver, the amount of silver deposited is increased, though in a diminishing ratio, by successive additions of copper salt. That this acceleration is not produced by a copper salt only was proved by repeating the experiments with various other nitrates. The tabulated results show that the increased effect does not de-

* The reader will find these curves engraved in NATURE for August 4, 1870, p. 279.

pend simply upon the nitric element, but likewise on the nature of the salt.

In the discussion which followed, some curious facts were elicited with respect to the action of sugar on metallic iron. It is well known that hitherto it has not been possible, on account of this action, to convey sugar in iron ships; but Dr. Calvert stated that he had discovered a very simple method, which entirely prevented the action, and he had no doubt that henceforward sugar would be as safely carried in iron ships as in wooden bottoms.

Mr. Thos. Ainsworth then read a paper *On Facts Developed by the working of Hæmatite Ores* in the Ulverstone and Whitehaven districts from 1844-1871. The communication was exceedingly well illustrated by diagrams and specimens; but the conclusions arrived at by Mr. Ainsworth were pretty generally combated.

On Friday the proceedings commenced with a paper by Prof. Wheeler, of Chicago, *On the Recent Progress of Chemistry in the United States.* Mr. Henry Deacon gave an account of his *Chlorine Process as applied to the Manufacture of Bleaching Powder on the larger Scale.* A note *On Regianic Acid*, a product derived from walnuts, was then communicated by Dr. Phipson. It was followed by a paper by Dr. Calvert *On the Estimation of Sulphur in Coal and Coke.* The sulphur found in coal or coke often exists in two states, partly as sulphuric acid combined with lime, and partly as sulphur combined with iron; it is only the latter combination which lessens the commercial value of the fuel. By boiling the powdered coal with a solution of carbonate of soda, the lime composed is decomposed, and by washing the sulphuric acid may be removed; in the residue is contained the sulphur, combined with iron, which is estimated by any of the methods familiar to chemists. Mr. E. C. C. Stanford next gave the results of *Some Preliminary Experiments on the Retention of Organic Nitrogen by Charcoal;* these he intends to prosecute still further, and to communicate his observations to the next meeting at Brighton. Mr. I. Smyth gave an account of *Some Improvements in Chlorimetry.* In his opinion the use of the milky solution of bleaching powder as employed in the usual methods of chlorimetry is unsatisfactory, and he accordingly recommends that the chloride of lime be decomposed by a solution of carbonate of soda and filtered from the precipitated carbonate of lime when the amount of available chlorine may be determined in the filtrate by any of the usual methods. Professor Delffs, of Hiedelberg, exhibited some splendid *Crystals of Sorbin.* This body was discovered nearly twenty years ago by Pelouze, but hitherto nobody has succeeded in preparing it from the source indicated by the distinguished French chemist. Dr. Delffs attributed the want of success to the fact that it was usual to combine the preparation of malic acid with that of sorbin, and he showed that it is only when the production of the former substance is dispensed with that sorbin is obtained. By strictly following the method given by Pelouze, Dr. Delffs obtained a large quantity of fine crystals of Sorbin, but on searching for malic acid in the residue, he found that not a trace was present. He attributes its absence to its combination with the radical of alcohol (the malic acid being contained in the alcoholic extract of the berries of *Sorbus Aucuparia*, the source of the body), whereby malate of ethyl is formed, while by assimulating two atoms of water is converted into sorbin. It would appear therefore that no sorbin is contained ready formed in the fruit of *Sorbus Aucuparia.*

Dr. Emerson Reynolds gave an account of his experiments *On the Action of Aldehyde on Sulpho- and Oxygen Ureas*, and exhibited a variety of preparations of these compounds.

Mr. W. Chandler Roberts, chemist of the Mint, read a short paper *On the Molecular Arrangement of the Alloy employed for the British Silver Coinage.* The paper proved that the homogeneous character of the alloy of silver and copper is destroyed by the cooling of the molten mass, the silver being concentrated in the centre.

Dr. Moffatt read a paper on *Ozonometry*, in which he stated that ozone test papers do not become permanently coloured in the neighbourhood of cesspools, and that the brown coloration when found is removed by the products of putrefaction. He also stated that light, the humidity of the atmosphere, and the direction of the wind, influence the colouring of the test paper, moisture with heat accelerating chemical action, while strong wind causes a great quantity of ozone to impinge upon the test paper in a given time. To counteract the effects of these, he recommended the test paper to be kept in a box. He next described a tube ozonometer which he had had in use, and gave results obtained by an aspirator ozonometer, and concluded by stating that the results obtained by the aspirator ozonometer were not satisfactory.

## SECTION C.

*On the Progress of the Geological Survey in Scotland*, by Prof. Geikie.

When the British Association last met in Scotland, I had the honour of bringing before this Section a report upon the progress of the Geological Survey, from the time of its commencement here in 1854 by Professor Ramsay, under the direction of the late Sir Henry De la Beche, up to the year 1867, under the supervision of the present Director, Sir Roderick Murchison. During the four years which have since elapsed, considerable advance has been made in the survey of the southern half of Scotland, and I propose now, with the sanction of Sir Roderick, to present to you a brief outline of what has been done, and of the present state of the Survey.

At the time of my previous report rather more than 3,000 square miles had been surveyed. Since then we have completed 2,700 square miles additional, making a total area of nearly 6,000 square miles. Of this area 3,175 square miles have been published on the one-inch scale, and three sheets, representing in all 632 square miles, are now in course of being engraved. The whole country is surveyed upon the Ordnance Maps on the scale of six inches to a mile, and from these field-maps the work is reduced to the one-inch scale, which is the scale adopted for the general Geological Map of the country. In addition to that general map, however, maps on the larger or six-inch are published of all mineral tracts. In this way five sheets of the six-inch maps have now been published, embracing the whole of the coal-fields of Fife, Haddingtonshire, and Edinburghshire, with a large portion of the coal-fields of Lanarkshire, Renfrewshire, Ayrshire, and Dumfriesshire.

The area over which the field-work of the Survey has extended lies between the mouths of the Firths of Tay, Forth, Clyde, and Solway, eastwards to the borders of Roxburghshire and the mouth of the Tweed. It includes the counties of Fife, Kinross, the Lothians, Lanark, Renfrew, Peebles, Ayr, Wigton, Kirkcudbright, Dumfries, and Selkirk, with parts of Stirling, Dumbarton, and Perth.

Of the geological formations examined, the Lower Silurian rocks of the southern uplands cover a considerable space upon the published maps. Until three years ago the mapping of these rocks continued to be most unsatisfactory, owing to the want of any continuous recognisable section from which the order of succession among the strata could be ascertained, and to the great scarcity of organic remains. Our more recent work among the Leadhills, however, has at last given us the means of unravelling, as we hope, the physical structure and stratigraphical relations of the uplands of the south of Scotland. The rocks there are capable of division into several well-marked groups of strata, characterised by distinct assemblages of fossils. We have a lower or Llandeilo series with a suite of graptolites, and forming probably an upper part of the Moffat group, and a higher or Caradoc set of beds, with a considerable assemblage of distinctive fossils. This higher group we believe to be on the same general horizon as the limestones of Wrae and Kilbucho in Peeblesshire.

The Lower Old Red Sandstone has now been mapped completely over the whole of its extent between Edinburgh and the south of Ayrshire. Fossils have only been met with at one locality in the latter county, where *Cephalaspis* occurs. The most characteristic feature of the formation is the enormous development of its interbedded volcanic rocks. Between Edinburgh and Lanarkshire, also, there occurs in this formation a local but violent unconformability, connected probably with some phase of the contemporaneous volcanic activity of the region.

Most of the detailed work of the Survey has lain upon Carboniferous rocks. In the lowest formations of this system, known as the Calciferous Sandstones, the Survey has now been able to trace a twofold division completely across the country, from sea to sea, viz. a lower group of red sandstones, and a higher group of white sandstones, green, grey, and dark shales, cement-stones, limestones, and occasional coal-seams. All these strata lie beneath the true Carboniferous Limestone. They are becoming daily more important from their containing in some places highly bituminous shales, from which paraffin oil can be made. The Carboniferous Limestone series, with its valuable coals and ironstones, has been mapped, and in great part published, for the eastern and south-western coal-fields, and this is also the case with the Coal-measures. Much additional information has been obtained regarding the development of volcanic action in central Scotland during the Carboniferous period.

The Permian basins of Ayrshire and Thornhill have been surveyed and in great part published. Much fresh light has in the course of this Survey been thrown on the interesting Permian volcanoes of the south-west of Scotland.

Attention has been continuously given to the superficial accumulations. These are now mapped in as great detail as the rocks underneath, and plans are being prepared with the view to an issue of maps of the surface geology.

By a recent order of the Director-General, each one-inch map is now accompanied at the time of its publication, or as soon thereafter as possible, with an explanatory pamphlet, in which the form of the ground, geological formations, fossils, rocks, faults, and economic minerals, are briefly described, and such further information given as seems necessary for the proper elucidation of the map. These pamphlets are sold at an uniform price of 3*d*. Detailed vertical sections are published for each coal-field. For the construction of these sections, records of boring operations are procured and recorded in the register-books of the Survey. Since 1867 more than 312,200 feet of such borings have in this way been entered in our books. Sheets of horizontal sections on a large scale are likewise issued to form, with the maps and explanations, a compendium of the geological structure of each large district.

Another feature of the work of the Survey is the collection of specimens of the rocks and fossils of each tract of country as it is surveyed. Since my previous report to this Section of the British Association, we have collected 1,011 specimens of rocks, and 7,500 fossils. These are named and exhibited, as far as the present accommodation will permit, in the Museum of Science and Art at Edinburgh.

The work of the Geological Survey is carried on, as I have said, under the guidance of its Director-General, Sir Roderick Murchison, a name which has long been a household word at the meetings of the British Association, and one to which I am sure you will permit me to make on this occasion more than a passing reference. While the Survey advances, as I have shown, steadily over the face of the country, unravelling piece by piece the complicated details of its geological structure, to Sir Roderick belongs the rare merit of having himself led the way, by sketching for us, boldly and clearly, the relations of the older rocks over more than half of the kingdom. Much must undoubtedly remain for future investigation, but his outline of the grand essential features of Highland geology will ever remain as a monument of his powers of close yet rapid observation and sagacious inference. At one time I had hoped that the Chair of this Section might be filled by him, and that we should be permitted to listen anew to his expositions of the rocks of his native country. There is no one among us who does not regret the absence of the familiar face and voice of the veteran of Siluria. We meet once more on Scottish ground, and for the first time we have not here with us the man who has laid a deeper, broader impress on Scottish geology than any other geologist either of past generations or of this. There is, however, on the present occasion, a special cause for regret. Only within the last few months he founded a Chair of Geology in the University within whose walls we are now assembled—the first and only chair of the kind in Scotland. It would have been a fitting and grateful duty on the part of the University to welcome one of its most distinguished benefactors. I am well aware, indeed, that this Section-room is no place for the obtrusion of personal sentiments; yet I would fain be allowed to add in conclusion an expression of my own deep regret at the recent illness and consequent absence of one to whom, over and above the admiration which we all feel for his life-long labours and his personal character, many years of friendly intercourse have bound me by the closest ties of affection.

## SECTION D.

### BIOLOGY

### Opening Address by the President, Professor Allen Thomson

In now opening the meetings of the Biological Section, it is my first duty to express my deep sense of the honour which has been conferred on me in appointing me to preside over its deliberations. I trust that my grateful acceptance of the office will not appear to be an assumption on my part of more than a partial connection with the very wide field of science included under the term Biology.

I would gladly have embraced the opportunity now afforded me of conforming to a custom which has of late become almost the rule with presidents of sections—viz., that of bringing under your review the more valuable discoveries with which our science has been enriched in recent times, were it not that the subjects which I might have been disposed to select would require an amount of detail in each which would necessarily limit greatly their number, and that any attempt to overtake the whole range of this wide-spread department of science would be equally presumptuous and futile on the part of one whose attention has been restricted mainly to one of its divisions. I am further embarrassed in the choice of topics for general remark by the circumstance that many of those upon which I might have ventured to address you have been most ably treated of by my predecessors, as for example, in the sectional addresses of Dr. Acland, Dr. Sharpey, Mr. Berkeley, Dr. Humphry, and Dr. Rolleston, as well as in the presidential addresses of Dr. Hooker and Prof. Huxley. I must content myself therefore with endeavouring to convey to you some of the ideas which arise in my mind in looking back from the present upon the state of Biological science at the time when, forty years since, the meetings of the British Association commenced—a period which I am tempted to particularise from its happening to coincide very nearly with that at which I began my career as a public teacher in one of the departments of biology in this city. In the few remarks which I shall make, it will be my object to show the prodigious advance which has taken place, not only in the knowledge of our subject as a whole, but also in the ascertained relation of its parts to each other, and in the place which biological knowledge has gained in the estimation of the educated part of the community, and the consequent increase in the freedom with which the search after truth is now asserted in this as in other departments of science. And first, in connection with the distribution of the various subjects which are included under this section, I may remark that the general title under which the whole Section D has met since 1866, viz., Biology, seems to be advantageous both from its convenience, and as tending to promote the great consolidation of our science, and a juster appreciation of the relation of its several parts. It may be that, looking merely to the derivation of the term, it is strictly more nearly synonymous with Physiology in the sense in which that word has been for a long time employed, and therefore designating the science of life, rather than the description of the living beings in which it is manifested. But until a better or more comprehensive term be found, we may accept that of Biology under the general definition of "the science of life and of living beings," or as comprehending the history of the whole range of organic nature—vegetable as well as animal. The propriety of the adoption of such a general term is further shown by a glance at the changes which the titles and distribution of the subordinate departments of this section have undergone during the period of the existence of the Association.

*History of the Section*

During the first four years of this period the Section met under the combined designation of Zoology and Botany, Physiology and Anatomy—words sufficiently clearly indicating the scope of its subjects of investigation. In the next ten years a connection with Medicine was recognised by the establishment of a sub-section or department of Medical Science, in which, however, scientific anatomy and physiology formed the most prominent topics, though not to the exclusion of more strictly medical and surgical, or professional, subjects. During the next decade, or from the year 1845 to 1854, we find along with Zoology and Botany a sub-section of Physiology, and in several years of the same time along with the latter a separate department of Ethnology. In the eleven years which extended from 1855 to 1865, the branch of Ethnology was associated with Geography in Section E. More recently, or since the arrangement which was commenced in 1866, the section Biology has included, with some slight variation, the whole of its subjects in three departments. Under one of these are brought all investigations in Anatomy and Physiology of a general kind, thus embracing the whole range of these sciences when without special application. A second of these departments has been occupied with the extensive subjects of Botany and Zoology; while the third has been devoted to the subject of Anthropology, in which all researches having a special reference to the structure and functions or life-history of man have been received and discussed. Such I understand to be the arrangement under which we shall meet on this occasion. At the conclusion of my remarks, therefore, the department of Anatomy and Physiology will remain with me in this room; while that of Zoology and Botany, on the one hand, and of Anthropology on the other, will adjourn to the apartments which have been provided for them respectively.

### *Anthropology*

With regard to the position of Anthropology, as including Ethnology, and comprehending the whole natural history of man, there may be still some differences of opinion, according to the point of view from which its phenomena are regarded: as by some they may be viewed chiefly in relation to the bodily structure and functions of individuals or numbers of men; or as by others they may be considered more directly with reference to their national character and history, and the affinities of languages and customs; or by a third set of inquirers, as bearing more immediately upon the origin of man and his relation to animals. As the first and third of these sets of topics entirely belong to Biology, and as those parts of the second set which do not properly fall under that branch may with propriety find a place under Geography or Statistics, I feel inclined to adhere to the distinct recognition of a department of Anthropology, in its present form; and I think that the suitableness of this arrangement is apparent, from the nature and number of the appropriate reports and communications which have been received under the last distribution of the subjects.

### *Condition of Biological Research*

The beneficial influence of the British Association in promoting biological research is shown by the fact that the number of the communications to the sections received annually has been nearly doubled in the course of the last twenty years, and this influence has doubtless been materially assisted by the contributions in money made by the Association in aid of various biological investigations; for it appears that out of the whole sum of nearly 34,500*l*. contributed by the Association to the promotion of scientific research, about 2,800*l*. has been devoted to biological purposes, to which it would be fair to add a part at least of the grants for Palæontological researches, many of which must be acknowledged to stand in close relation to Biology.

The enormous extent of knowledge and research in the various departments of Biology has become a serious impediment to its more complete study, and leads to the danger of confined views on the part of those whose attention, from necessity or taste, is too exclusively directed to the details of one department, or even, as often happens, to a subdivision of it. It would seem, indeed, as if our predecessors in the last generation possessed this superior advantage in the then existing narrow boundaries of knowledge, that it was possible for them to overtake the contemplation of a wider field, and to follow out researches in a greater number of the sciences. To such combinations of varied knowledge, united with their transcendent powers of sound generalisation and accurate observation, must be ascribed the wide-spread and enduring influence of the works of such men as Haller, Linnæus, and Cuvier, Von Baer, and Joannes Müller. There are doubtless brilliant instances in our own time of men endowed with similar powers; but the difficulty of bringing these powers into effectual operation in a wide range is now so great, that, while the amount of research in special biological subjects is enormous, it must be reserved for comparatively few to be the authors of great systems, or of enduring broad and general views which embrace the whole range of biological science. It is incumbent, therefore, on all those who are desirous of promoting the advance of biological knowledge, to combat the confined views which are apt to be engendered by the too great restriction of study to one department. However much subdivision of labour may now be necessary in the original investigation and elaboration of new facts in our science (and the necessity for such subdivision will necessarily increase as knowledge extends), there must be secured at first, by a wider study of the general principles and some of the details of collateral branches of knowledge, that power of justly comparing and correlating facts which will mature the judgment and exclude partial views. To refer only to one bright example; I may say that it can scarcely be doubted that it is the unequalled variety and extent of knowledge, combined with the faculty of bringing the most varied facts together in new combinations, which has enabled Mr. Darwin (whatever may be thought otherwise of his system) to give the greatest impulse which has been felt in our own times to the progress of biological views and thought; and it is most satisfactory to observe the effect which this influence is already producing on the scientific mind of this country, in opposing the tendency perceptible in recent times to the too restricted study of special departments of natural history. I need scarcely remind you that for the proper investigation and judgment of problems in physiology, a full knowledge of anatomy in general, and much of comparative anatomy, of histology and embryology, of organic chemistry and of physics, is indispensable as a preliminary to all successful physiological observation and experiment. The anatomist, again, who would profess to describe rationally and correctly the structure of the human body, must have acquired a knowledge of the principles of morphology derived from the study of comparative anatomy and development, and he must have mastered the intricacies of histological research. The comparative anatomist must be an accomplished embryologist in the whole range of the animal kingdom, or in any single division of it which he professes to cultivate. The zoologist and the botanist must equally found their descriptions and systematic distinctions on morphological, histological, and embryological data. And thus the whole of these departments of biological science are so interwoven and united that the scientific investigation of no one can now be regarded as altogether separate from that of the others. It has been the work of the last forty years to bring that intimate connection of the biological sciences more and more fully into prominent view, and to infuse its spirit into all scientific investigation. But while in all the departments of Biology prodigious advance has been made, there are two more especially which merit particular mention, as having almost taken their origin within the period I now refer to, as having made the most rapid progress in themselves, and as having influenced most powerfully and widely the progress of discovery, and the views of biologists in other departments — I mean histology and embryology.

### *Histology*

I need scarcely remind those present that it was only within a few years before the foundation of the British Association that the suggestions of Lister in regard to the construction of achromatic lenses brought the compound microscope into such a state of improvement as caused it to be restored, as I might say, to the place which the more imperfect instrument had lost in the previous century. The result of this restoration became apparent in the foundation of a new era in the knowledge of the minute characters of textural structure, under the joint guidance of Robert Brown and Ehrenberg, with contributions from many other observers, so as at last to have entitled this branch of inquiry to its designation, by Prof. Huxley, of the "exhaustive investigation of structural elements." All who hear me are fully aware of the influence which, from 1839 onwards, the researches of Schwann and Schleiden exerted on the progress of Histology and the views of anatomists and physiologists as to the structure and development of the textures both of plants and animals, and the prodigious increase which followed in varied microscopic observations. It is not for me here even to allude to the steps of that rapid progress by which a new branch of anatomical science has been created; nor can I venture to enter upon any of the interesting questions presented by this department of microscopic anatomy; nor attempt to discuss any of those difficult problems possessing so much interest at the present moment, such as the nature of the organised cell, or the properties of protoplasm. I would only remark that it is now very generally admitted that the cell wall (as Schwann indeed himself pointed out) is not a constant constituent of the cell, nor a source of new production, though still capable of considerable structural change after the time of its first formation. The nucleus has also lost some of the importance attached to it by Schwann and his earlier followers, as an essential constituent of the cell, while the protoplasm of the cell remains in undisputed possession of the field as the more immediate seat of the phenomena of growth and organisation, and of the contractile property which forms so remarkable a feature of their substance. I cordially agree with much of what Prof. Huxley has written on this subject in 1853 and 1869. The term "physical basis of life" may perhaps be in some respect objectionable, but I look upon the recognition of protoplasm which he has enforced, as a most important step in the recent progress of histology; adopting this general term to indicate that part of the tissue of plants and animals which is the constant seat of the growing and moving phenomena; but not implying identity of nature and properties in all the variety of circumstance in which this substance may occur. To Haeckel the fuller history of protoplasm in its lowest forms is due. To Dr. Beale we owe the minutest investigation of the properties by the use of magnifying powers beyond any that had previously been known, and the successful employment of reagents which appear to mark out its distinction from the other elements of the textures. I may remark, however, in passing, that I am inclined to regard contractile protoplasm, whether vegetable or animal, as in no instance entirely amorphous or homogeneous, but rather as always presenting some minute molecular structure which distinguishes it from parts of glassy clearness. Admitting that the form it assumes is not necessarily that of a regular cell, and may be various and irregular in a few exceptional instances, I am not on that account disposed to give up definite structure as one of the universal characteristics of organisation in living bodies. I would also suggest that the terms formative and nonformative, or some such other, would be preferable to those of "living and dead," employed by Dr. Beale, to distinguish the protoplasm from the cell-wall or its derivatives, as the latter terms are liable to introduce confusion.

*Embryology.*

To the discoveries in embryology and development I might have been tempted to refer more at large, as being those which have had, of all modern research, the greatest effect in extending and modifying biological views, but I am warned from entering upon a subject in which I might trepass too much on your patience. The merits of Wolff as the great first pioneer in the accurate observation of the phenomena of development were clearly pointed out by Prof. Huxley in his presidential address of last year. Under the influence of Döllinger's teaching, Pander, and afterwards Purkinge, Von Baer, and Rathke, established the foundations of the modern history of embryology. It was only in the year 1827 that the ovum of mammals was discovered by Von Baer; the segmentation of the yelk, first observed by Prevost and Dumas in the frog's ovum in 1824, was ascertained to be general in succeeding years, so that the whole of the interesting and important additions which have followed, and have made the history of embryological development a complete science, have been included within the eventful period of the life of this Association. I need not say how distinguished the Germans have been by their contributions to the history of animal development. The names of Valentin, R. Wagner, Bischoff, Reichert, Kölliker, and Remak are sufficient to indicate the most important of the earlier steps in recent progress, without attempting to enumerate a host of others who have assisted in the great work thus founded. I am aware that the mere name of development suggests to some ideas of a disturbing kind as being associated with the theory of evolution recently promulgated. To one accustomed during the whole of his career to trace the steps by which every living being, including man himself, passes from the condition of an almost imperceptible germ, through a long series of changes of form and structure into their perfect state, the name of development is suggestive rather of that which seems to be the common history of all living beings; and it is not wonderful therefore that such a one should regard with approval the more extended view which supposes a process of development to belong to the whole of nature. How far that principle may be carried, to what point the origin of man or any animal can by facts or reasoning be traced in the long unchronicled history of the world, and whether living beings may arise independently of parents or germs of previously existing organisms, or may spring from the direct combination of the elements of dead matter, are questions still to be solved, and upon which we may expect this section to guide the hesitating opinion of the time. I cannot better express the state of opinion in which I find myself in regard to the last of these problems than by quoting the words of Professor Huxley from his address of last year, p. lxxxiii.:—

"But though I cannot express this conviction of mine too strongly (viz., that the evidence of the most careful experiments is opposed to the occurrence of spontaneous generation), I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis ever has taken place in the past, or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call 'vital,' may not some day be artificially brought together. And again, if it were given me to look beyond the abyss of geologically recorded time, to the still more remote period when the earth was passing through physical and chemical conditions which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter." I will quote further a few wise words from the discourse to which many of you must have listened last evening with admiration. Sir William Thomson said—"The essence of science, as is well illustrated by astronomy and cosmical physics, consists in inferring antecedent conditions, and anticipating future evolutions, from phenomena which have actually come under observation. In biology, the difficulties of successfully acting up to this ideal are prodigious. Our code of biological law is an expression of our ignorance as well as of our knowledge." And again, "Search for spontaneous generation out of inorganic materials; let any one not satisfied with the purely negative testimony, of which we have now so much against it, throw himself into the inquiry. Such investigations as those of Pasteur, Pouchet, and Bastian are among the most interesting and momentous in the whole range of natural history; and their results, whether positive or negative, must richly reward the most careful and laborious experimenting."

*Organic Chemistry and Vital Force*

The consideration of the finest discoverable structures of the organised parts of living bodies is intimately bound up with that of their chemical composition and properties. The progress which has been made in organic chemistry belongs not only to the knowledge of the composition of the constituents of organised bodies, but also to the manner in which that composition is chemically viewed. Its peculiar feature, especially as related to biological investigation, consists in the results of the introduction of the synthetic method of research, which has enabled the chemist to imitate or to form artificially a greater and greater number of the organic compounds. In 1828 the first of these substances was formed by Wöhler, by a synthetic process, as cyanate of ammonia, or urea. But still, at that time, though a few no doubt entertained juster views, the opinion generally prevailed among chemists and physiologists that there was some great and fundamental difference in the chemical phenomena and laws of organic and inorganic nature. Now, however, this supposed barrier has been in a great measure broken down and removed, and chemists, with almost one accord, regard the laws of combination of the elements as essentially the same in both classes of bodies, whatever differences may exist in actual composition, or in the reactions of organic bodies in the more complex and often obscure conditions of vitality, as compared with the simpler, and, on the whole, better known phenomena of a chemical nature observed in the mineral kingdom. Thus, by the synthetic method, there have been formed among the simpler organic compounds a great number of alcohols, hydrocarbons, and fatty acids. But the most remarkable example of the synthetic formation of an organic compound is that of the alkaloid conia, as recently obtained by Hugo Schiff by certain reactions from butyric aldehyde, itself an artificial product. The substance so formed, and its compounds, possess all the properties of the natural conia—chemical, physical, and physiological—being equally poisonous with it. The colouring-matter of madder, or alizarine, is another organic compound which has been formed by artificial processes. It is true that the organised or containing solid, either of vegetable or animal bodies, has not as yet yielded to the ingenuity of chemical artifice; nor, indeed, is the actual composition of one of the most important of these, albumen and its allies, fully known. But as chemists have only recently begun to discover the track by which they may be led to the synthesis of organic compounds, it is warrantable to hope that ere long cellulose and lignine may be formed; and, great as the difficulties with regard to the albumenoid compounds may at present appear, the synthetic formation of these is by no means to be despaired of, but, on the contrary, may with confidence be expected to crown their efforts. From all recent research, therefore, it appears to result that the general nature of the properties belonging to the products of animal and vegetable life, can no longer be regarded as different from those of minerals, in so far at least as they are the subject of chemical and physical investigation. The union of elements and their separation, whether occurring in an animal, a vegetable, or a mineral body, must be looked upon as dependent on innate powers or properties belonging to the elements themselves; and the phenomena of change of composition of organic bodies occurring in the living state are not the less chemical because they are different from those observed in inorganic nature. All chemical actions are liable to vary according to the conditions in which they occur, and many instances might be adduced of most remarkable variations of this kind, observed in the chemistry of dead bodies from very slight changes of electrical, calorific, mechanical, and other conditions. But because the conditions of action or change are infinitely more complex and far less known in living bodies, it is not necessary to look upon the phenomena as essentially of a different kind, to have recourse to the hypothesis of vital affinities, and still less to shelter ourselves under the slim curtain of ignorance implied in the explanation of the most varied chemical changes by the influence of a vital principle.

*Zoology and Botany*

On the subjects of zoological and botanical classification and anthropology, it would be out of place for me now to make any observations at length. I will only remark, in regard to the first, that the period under review has witnessed a very great modification in the aspect in which the affinities of the bodies belonging to these two great kingdoms of nature are viewed by naturalists, and the principles on which groups of bodies of each are associated together in systematic classification; for, in the first place, the older view has been abandoned that the complication of structure rises in a continually increasing and continuous gradation from one kingdom to the other, or extends in one line, as it were, from group to group in either of the kingdoms separately. Evolution into a gradually increasing complexity of structure and function no doubt exists in both, so that types or general plans of formation must be acknowledged to pervade, presenting typical resemblances of construction of the deepest interest; but in the progress of morphological research, it has become more and more apparent that the different groups form radiations, which touch one another at certain points of greatest resemblance, rather than one continuous line, or a number of lines which partially pass each other. The simpler bodies of the two kingdoms of nature exhibit a gradually increasing resemblance to each other, until at last the differences between them wholly disappear, and we reach a point of contact of which the properties become almost indistinguishable, as in the remarkable Protista of Haeckel and others. I fully agree, however, with the view by Professor Wyville Thomson in his recent extraordinary lecture, that it is not necessary on this account to recognise an intermediate kingdom of nature. Each kingdom presents, as it were, a radiating expansion into groups for itself, so that the relations of the two kingdoms might be represented by the divergence of lines spreading in two different directions from a common point. Recent observations on the chorda dorsalis (or supposed notochord) of some Ascidians tend to revive the discussion at one time prevalent, but long in abeyance, as to the possibility of tracing a homology between the vertebrate and invertebrate animals; and, should this correspondence be confirmed and extended, it may be expected to modify greatly our present views of zoological affinities and classification. It will also be an additional proof of the importance of minute and embryological research in systematic determinations. The recognition of homological resemblance of animals, to which in this country the researches of Owen and Huxley have contributed so largely, form one of the most interesting subjects of contemplation in the study of comparative anatomy and zoology in our time; but I must refrain from touching on so seductive and difficult a subject.

*Natural Science in Schools*

There is another topic to which I can refer with pleasure as connected with the cultivation of biological knowledge in this country, and that is the introduction of instruction in natural science into the system of education of our schools. As to the feasibility of this in the primary schools, I believe most of those who are intimately acquainted with their management have expressed their decidedly favourable opinion—it being found that a portion of the time now allotted to the three great requisites of a primary education might with advantage be set apart, for the purpose of instructing the pupils in subjects of common interest, calculated to awaken in their minds a desire for knowledge of the various objects presented by the field of nature around them. As to the benefit which may result from this measure to the persons so instructed, it is scarcely necessary for me to say anything in this place. It is so obvious that any varied knowledge, however easily acquired or elementary, which tends to enlarge the range of observation and thought, must have some effect in removing its recipients from grosser influences, and may even supply information which may prove useful in social economy and in the occupations of labour. Nor need I point out how much more extended the advantages of such instruction may prove if introduced into the system of our secondary schools, and more freely combined than heretofore with the too exclusively literary and philosophical study which has so long prevailed in the approved British education. Without disparagement to those modes of study as in themselves necessary and useful, and excellent means of disciplining the mind to learning, I cannot but hold it as certain that the mind which is entirely without scientific cultivation is but half prepared for the common purposes of modern life, and is entirely unqualified for forming a judgment on some of the most difficult and yet most common and important questions of the day, affecting the interests of the whole community. I refer with pleasure to the published Essay of Dr. Lankester on this subject, and to the arguments addressed two days ago by Dr. Bennett to the medical graduates of the University, in favour of the establishment of physiology as a subject of general education in this country, with reference to sanitary conditions. It is gratifying, therefore, to perceive that the suggestions made some years ago in regard to this subject by the British Association, through its committee, have already borne good fruit, and that the attention of those who preside over education in this country, as well as of the public themselves, is more earnestly directed to the object of securing for the lowest as well as the highest classes of the community that wholesome combination of knowledge derived from education, which will duly cultivate all the faculties of the mind, and thus fit a greater and greater number for applying themselves with increased ability and knowledge to the purposes of their living and its improved condition. If the law of the Survival of the Fittest be applicable to the mental as well as to the physical improvement of our race (and who can doubt that in some measure it must be so), we are bound by motives of interest and duty to secure for all classes of the people that kind of education which will lead to the development of the highest and most varied mental power. And no one who has been observant of the recent progress of the useful arts, and its influence upon the moral, social, and political condition of our population, can doubt that that education must include instruction in the phenomena of external nature, including, more especially, the laws and conditions of life and health; and that it ought to be, at the same time, such as will adapt the mind to the ready acquisition and just comprehension of varied knowledge. It is obvious, too, that while this more immediately useful or beneficial effect on the common mind may be produced by the diffusion of natural knowledge among the people, biological science will share in the gain accruing to all branches of natural science, by the greater favour which will be accorded to its cultivators, and the increased freedom from prejudice with which their statements are received and considered by learned as well as by unscientific persons.

*Spiritualism*

I cannot conclude these observations without adverting to one aspect in which it might be thought that the appreciation of biological science has taken a retrograde rather than an advanced position. In this, I do not mean to refer to the special cultivators of Biology in its scientific acceptation, but to the fact that there appears to have taken place of late a considerable increase in the number of persons who believe, or who imagine that they believe, in the class of phenomena which are now called spiritual, but which have been known since the exhibitions of Mesmer, and, indeed, long before his time, under the most varied forms, as liable to occur in persons of an imaginative turn of mind and peculiar nervous susceptibility. It is still more to be regretted that many persons devote a large share of their time to the practice—for it does not deserve the name of study or investigation—of the alleged phenomena, and that a few men of acknowledged reputation in some departments of science have lent their names, and surrendered their judgment, to the countenance and attempted authentication of the foolish dreams of the practitioners of spiritualism, and similar chimerical hypotheses. The natural tendency to a belief in the marvellous is sufficient to explain the ready acceptance of such views by the ignorant; and it is not improbable that a higher species of similar credulity may frequently act with persons of greater cultivation, should their scientific information and training have been of a partial kind. It must be admitted, further, that extremely curious and rare, and to those who are not acquainted with nervous phenomena, apparently marvellous phenomena, present themselves in peculiar states of the nervous system—some of which states may be induced through the mind, and may be made more and more liable to recur, and are greatly exaggerated by frequent repetition. But making the fullest allowance for all these conditions, it is still surprising that persons, otherwise appearing to be within the bounds of sanity, should entertain a confirmed belief in the possibility of phenomena, which, while they are at variance with the best established physical laws, have never been brought under proof by the evidences of the senses, and are opposed to the dictates of sound judgment. It is so far satisfactory in the interests of true biological science that no man of note can be named from the long list of thoroughly well-informed anatomists and physiologists, who has not treated the belief in the separate existence of powers of animal magnetism and spiritualism as wild speculations, devoid of all foundation in the carefully tested observation of facts. It has been the habit of the votaries of the systems to which I have referred to assert that scientific men have neglected or declined to investigate the phenomena with attention and candour; but nothing can be farther from the truth than this statement. Not to mention the admirable reports of the early French academicians, giving the account of the negative result of an examination of the earlier mesmeric phenomena by men in every way qualified to pronounce judgment on their nature, I am aware that from time to time men of eminence, and fully competent, by their knowledge of biological phenomena, and their skill and accuracy in conducting scientific investigation, have made the most patient and careful examination of the evidence placed before them by the professional believers and practitioners of so-called magnetic, phreno-magnetic, electro-biological, and spiritualistic phenomena; and the result has been uniformly the same in all cases, when they were permitted to secure conditions by which the reality of the phenomena, or the justice of their interpretation, could be tested—viz., either that the experiments signally failed to educe the results professed, or that the experimenters were detected in the most shameless and determined impostures. I have myself been fully convinced of this by repeated examinations. But were any guarantee required for the care, soundness, and efficiency of the judgment of men of science on these phenomena and views, I have only to mention, in the first place, the revered name of Faraday, and in the next that of my life-long friend Dr. Sharpey, whose ability and candour none will dispute, and who, I am happy to think, is here among us, ready, from his past experience of such exhibitions, to bear his testimony against all classes of *levitation*, or the like, which may be the last wonder of the day among the mesmeric or spiritual pseudo-physiologists. The phenomena to which I have at present referred are in great part dependent upon natural principles of the human mind, placed, as it would appear, in dangerous alliance with certain tendencies of the nervous system. They ought not to be worked upon without the greatest caution, and they can only be fully understood by the accomplished physiologist who is also conversant with healthy and morbid psychology. The experience of the last hundred years tends to show that while there are always to be found persons peculiarly liable to exhibit the phenomena in question, there will also exist a certain number of minds prone to adopt a belief in the marvellous and striking in preference to that which is easily understood and patent to the senses; but it may be confidently expected that the diffusion of a fuller and more accurate knowledge of vital phenomena among the non-scientific classes of the community may lead to a juster appreciation of the phenomena in question, and a reduction of the number among them who are believers in scientific impossibilities.

## SECTION E.

### GEOGRAPHY

OPENING ADDRESS BY THE PRESIDENT, COL. H. YULE, C.B.

THE first natural duty in circumstances like the present is to pay a tribute, however inadequate, to the memory of the eminent geographer whom we expected to fill this chair. The long list of his works has been rehearsed in so many of the notices that have honoured his memory, as well as in the address of the Vice-President of the Geographical Society, when presenting the medal which he had won by so many years of faithful labour in the cause of Geography, that I need not now repeat them Indeed, when contemplating the catalogue of such an amount of work achieved, an amateur geographer like myself stands abashed; but feels at the same time that his own limited experience and desultory studies serve at least to furnish him with some just scale by which to estimate the vast labours involved in the accomplishment of such a life's work as Dr. Keith Johnston's.

I shall in this address attempt no general view of the geographical desiderata of the time, and of recent geographical progress in discovery and literature throughout the world. Living habitually far from new books and meetings of societies, I am not sufficient for these things, nor, if I were, could I easily vary from the comprehensive epitome of the year's geography, which but two months ago was issued, though, as we know with sorrow, not delivered, by him who has been so long the Dean of the Faculty of Geographers in Britain, and whose name is identified throughout the Continent with English geography. Sir Roderick Murchison has desired me to take occasion to express his deep regret at his inability to be present at this meeting. It is, he said, one of the most painfully-felt disappointments that his illness has occasioned. For he had looked forward with strong interest to taking part once more in a meeting of the Association at the chief city of his native country—with which city, I may remind you, he the other day bound his name and memory by strong and enduring ties in the foundation of a Chair of Geology in this University. Instead, then, of attempting a review which in my case would be crude, and therefore both dull and uninstructive, I propose to turn to one particular region of the old world with which my own studies have sometimes been concerned, and to say something of its characteristics, and of the progress of knowledge, as well as of present questions regarding it.

There are, however, one or two points on which I must first touch lightly. Of Livingstone, all that there is to tell has already been told to the world by Sir Roderick Murchison. We know the task that Livingstone had laid out for himself in dispersing the darkness that still hangs over some of the greatest features of Central African hydrography, by determining the ultimate course of the great body of drainage which he has followed northward from 12° south latitude—whether towards the Congo and the Atlantic, or towards Baker's Lake and so to the Nile; as well as the kindred question of the discharge of Lake Tanganyika; but of his progress in the solution of those questions we know nothing. I can but add that Sir Roderick himself has lost none of his confidence in the accomplishment of the task, and in the return of the great traveller at no distant period. That confidence of his has been so often before justified by the arrival of fresh news of Livingstone, however meagre, that we may well retain strong hope, even if it be not granted to all of us to rise from hope into confidence. We trust, then, that Livingstone will never have a place among the martyrs of geography.

One addition, however, has been made during the past year to that long list, in the name of the undaunted George Hayward, formerly a lieutenant in the 72nd Regiment, who had for some years resolutely devoted himself to geographical discovery. After having proved his powers in a journey to Yarkand and Kashghar, which obtained for him last year one of the medals of the Geographical Society, he had started again, with aid from that Society, to attempt an examination of the famous plateau of Pamir, hoping to succeed in crossing it, and to descend upon the Russian territory at Samarkand. In the Darkot Pass above Yassin, he was foully murdered by the emissaries of the chief of that district, Mir Wali by name. Public suspicion in India first turned upon the Maharajah of Kashmir, on whose alleged oppressions Hayward, in a private letter, had made severe remarks, which were rashly published by the editor of a local newspaper. The latest intelligence seems to exonerate the Maharajah, and to throw the guilt of complicity rather on the Mahomedan Chief of Chitral. If he be the guilty man, it may be difficult to punish him, so inaccessible is his position at present; for, to apply the old saw of the Campbells, "It is a far cry to Chitral." I may observe, however, that some sixteen or seventeen years ago, a similar murder took place on the persons of two poor French priests at the other extremity of India, and within the Thibetan boundary on the Upper Brahmapootra, and the apprehension of the criminal must have seemed almost as hopeless as in this case. Yet eventually he fell into the hands of our officers of the province of Assam, and paid the due penalty of his crime.

The geographical field on which, with your permission, I propose to expatiate for a little, is that of India beyond the Ganges. I mean in the largest sense of the expression, and inclusive, at least, in some points of view, of the Indian Islands. India, indeed, in old times was a somewhat vague term, or at least it had always a vague as well as an exacter interpretation. In the latter, it had the same application that we give it now when we speak with precision; it meant that vast semi-peninsular region roughly limited by the valleys of the Indus and the Ganges, which embraces many nations and many tongues and many climates, but yet all pervaded by a certain almost intangible character, which gives it a kind of unity recognised by all. In its vaguer sense, India meant simply the Far East. The traces of such use still survive in such expressions as the East Indies or the Indian Archipelago. Though this vague and large application of the name probably arose only from the vagueness of knowledge, it coincides roughly with a fact, and that is the extraordinary expansion of Hindoo influence which can be traced in the vestiges of religion, manners, architecture, language, and nomenclature over nearly all the regions of the East to which the name has been applied. Another name has been applied to the continental part of this region—Indo-China. This, too, expresses the fact that on this area the influences of India and of China have interpenetrated. But the influence of China has, except on the eastern coast, been entirely political, and has not, like India, affected manners, arts, and religion.

The address concluded with a long and interesting account of the land trade which has been maintained for many centuries between Western China and the Valley of the Iriwadi.

## SECTION F.

### ECONOMIC SCIENCE AND STATISTICS

OPENING ADDRESS BY THE PRESIDENT, LORD NEAVES

The greater part of this address deals with subjects beyond our scope; we may, however, make the following extracts:—

Economic science is sometimes spoken of as having a very modern date; but I think that this is an error. More or less the subject has entered into all the codes or systems of law that have been established from the earliest times. Alongside of political philosophy, which may be considered as peculiarly the science of government, great attention has always been bestowed upon matters which form an important part of political economy, or economic science—such as taxation, trade, commerce, wealth, and population. Those writers also who have presented us with ideal or imaginary states or Utopias are full of discussions and speculations of the same kind. The rival "Republics" of Plato and Aristotle afford abundant illustrations of this statement. It is peculiarly interesting to see this fact brought out so vividly in the admirable introduction to the "Republic" of Plato, prefixed to that treatise in Prof. Jowett's translation of that great philosopher; and if we had a similar translation and exposition of Aristotle's kindred work, which I think we might have from the hand of one of our own vice-presidents, to whom we owe so excellent an exposition of the "Ethics," we should see in a remarkable manner how many of the most interesting questions of the present day were considered and dealt with by those wonderful men according to the varying lights and tendencies which characterised their several minds. It is true that in more recent times a great advance has been made in economic science, and the chief feature and excellency of that change is the tendency to leave things as much as possible to their natural operation, and to the inherent laws of nature and society. It is to the credit of Scotland that she has produced the two greatest leaders in this altered movement—David Hume and Adam Smith—who are still high authorities on the whole subject, and whose principles have been made the basis of our recent legislation. The subject of Statistics is added to the title of this section as an auxiliary to the main subject of Economic Science.

The subjects to which statistics may be extended seem to be innumerable, and new ones are cropping up every day. In the pages of NATURE there lately appeared a letter of a somewhat curious kind, which may perhaps engage the attention of our fellow-associate member Mr. Tyler. The suggestion in that letter was that the degree of civilisation existing in any country is connected with the quantity of soap there consumed. The writer gave as a formula the equation of

$$x = \frac{S}{P}$$

$x$ being the amount of civilisation inquired for, S being the soap consumed, and P the population consuming it. So that the amount of civilisation depended on the proportion of S, the numerator, to P, the denominator. If S is large in proportion to P, then the civilisation is great, and *vice versâ*. How the civilisation of Scotland in the olden times would come out according to this test I shall not inquire; but if there is any truth in the proposition, it gives additional relevancy and interest to the question which is sometimes vulgarly put by some people to their friends as to how they are provided with that commodity. I have not yet seen any tables framed upon this principle, but I have no doubt that the Registrar-General will keep it in view. An inquiry of a more serious nature, and indeed peculiarly important and impressive, is connected with one of the most remarkable phenomena in human nature—I mean the occasional appearance in the world of men of great genius. From time to time men have arisen whose mental powers have far transcended the ordinary average of human intellect, and who have thereby been enabled, within the space of a single life, and by the effort of a single mind, to give an impulse to science and discovery which they could not have received through long generations of average mediocrity. Whether this singular boon and blessing to mankind can be traced to any law is a natural but mysterious inquiry. Some persons have considered the production of exceptional genius as quite an insulated fact; and Savage Landor declared that no great man had ever a great son, unless Philip and Alexander of Macedon constituted an exception. Mr. Galton, however, in his interesting work on "Hereditary Genius," has endeavoured to prove that genius runs in families, or, at least, that men of genius have generally sprung from a stock where great mental power is conspicuous; and he adheres to the view commonly taken as to the importance of the maternal character and influence in the formation of genius. I do not venture to give any opinion upon Mr. Galton's theory, but his book contains an important collection of facts bearing on the subject, and a great deal of very curious collateral speculation. Mr. Galton attributes great power in many ways to the principle of *heredity*, as it seems now to be called. He does not indeed go so far as the Irish statist, who, as mentioned by Sydney Smith, announced as a fact that *sterility was often hereditary*; but he states that comparative infertility is transmitted in families; and adduces as a remarkable example, a fact not generally known, if it be a fact, that in the case that frequently happens of Peers marrying heiresses, the family is apt to die out very soon, the heiress being naturally, in the general case, an only child, and bequeathing to her descendants a tendency to produce small families, who do not afford the usual chance of a numerous supply of descendants. Whatever may be said of some of his other opinions, I hesitate to concur with Mr. Galton in his proposition that as it is easy "to obtain by careful selection a breed of dogs or horses, gifted with peculiar powers of running, or of doing anything else, so it would be quite practicable to produce a highly-gifted race of men by judicious marriages during several consecutive generations." I doubt greatly the practicability of such a plan; and suspect there are some elements in human nature that would counteract it. Persons of proud family descent have often a horror of mesalliances; but I scarcely think it would be possible to inspire people of genius with the same *esprit de corps* or desire to wed with those on a par with them. Men of genius don't seem to me apt to fall in love with women as clever as themselves, and I rather suspect the tendency is to look for some difference of character, an instinct of which it is the object, or at least the result, to keep up the average of talent rather than to multiply the highest forms of mental power. At any rate we may here ask poor Polly's question, "Can love be controlled by advice?" and however we may in other respects agree with Horace's maxim, "Fortes creantur fortibus et bonis," I question whether a high mental stature could be maintained by coupling male and female genius together, or whether the experiment might not fail as signally as it is said sometimes to have done with Frederick William's attempts to breed Grenadiers. I strenuously advise, however, that a marriage with a fool of either sex should be always considered as a mesalliance, and I would particularly warn the ladies against such a step, taken, sometimes it is said, in the hope that their sway may in that way be more easily maintained. A fool is as difficult to be governed as a mule, and the couplet, I believe, is strictly true, that says—

Wise men alone, who long for quiet lives,
Wise men alone are governed by their wives.

## *SCIENTIFIC INTELLIGENCE FROM AMERICA* *

THE geological expedition under Prof. Hayden, at last advices, had reached Fort Hall, in Utah, on June 21, after a march from Ogden, during which much of interest was obtained by the party. The heat was very great, reaching from 95° to 105° in the shade during the day, with a difference of 25° to 35° between the wet and dry bulb thermometers. The party expected to pass Fort Ellis by the middle of July, on its way to the basin of the Yellow Stone Lake, where it will probably spend the greater part of the season. Mr. Thomas Moran, of Philadelphia, and Mr. Bierstadt, were to join the expedition before long for the purpose of making sketches for paintings.—In the August number of the *American Journal of Science*, will be found a con-

* Contributed by the Editor of *Harper's Weekly*.

tinuation of the important communications by Prof. Marsh, of Yale College, in regard to the results of his expedition to the Rocky Mountains during the past year. In addition to a number of new fossil mammals allied to the woodchuck, the gopher, the squirrel, the dog, and the fox, he presents a notice of sundry new species of birds from the Tertiaries of the West. Among these is an extinct species of eagle of large size, a turkey, and an owl.—In the search for new regions of exploration and discovery, it is not a little surprising to be assured that, taking the West Indies as a group, we know almost as little of their natural history as we do of that of Central Africa, especially of the islands east and south of the Greater Antilles. Thanks to the labours of Dr. Gundlach, and Prof. Poey in Cuba, of Dr. Bryant in the Bahamas, of Mr. March and Mr. Gosse in Jamaica, of Mr. A. E. Younglove in Hayti, of Dr. Bryant, Mr. Swift, and Mr. Latimer in Porto Rico, of Mr. Swift in St. Thomas, of Mr. Galody in Antigua, of Mr. Julien in Sombrero, and of Mr. Newton in Santa Cruz, we have a fair knowledge of the birds of the islands mentioned; but of Anguilla, St. Martin, Barbuda, Nevis, Montserrat, and Grenada we know nothing; and of St. Bartholomew, St. John, Saba, and Barbadoes, next to nothing. Dominica, Martinique, and Guadaloupe have been more or less explored by English and French naturalists, although with no very complete result. We are glad to see that the Zoological Society of London is printing a paper by Dr. Sclater upon a collection of the birds of Santa Lucia, sent to the Society by Mr. De Voeux, in which twenty-five species are enumerated, and among them three entirely peculiar to the island, one of them, a species of oriole, being hitherto undescribed. To such of our readers as have a spirit of enterprise, and are desirous of visiting a region which is sure to reward them with rich and undescribed treasures in natural history, we earnestly recommend the smaller West India islands, to which a trip can be made, especially in the winter season, with little or no risk to life or health, and with ample promise of satisfactory results.—We have before us the annual report of the trustees of the Museum of Comparative Zoology in Cambridge, for the year 1870, containing interesting communications from Prof. Agassiz, as the director, and his corps of able assistants. We are glad to learn that the temporary indisposition of the director (now happily past) has not crippled the efficiency of the establishment, and that so much progress has been made in arranging the immense stock of specimens that has been gathered within the walls of the museum from all quarters of the globe. The addition of a number of trained European naturalists, as Dr. Steindachner, Dr. Maack, Dr. Hagen, &c., has given great strength to the scientific corps, and has enabled Prof. Agassiz to do much toward realising the magnificent plan that he has proposed, for the permanent arrangement and utilisation of the collection.—We have referred, in a previous article upon American explorations into the fauna of the deep seas, to the proposed work, during the current season, of Mr. J. F. Whiteaves, the accomplished secretary and curator of the Natural History Society of Montreal; and we now give a more detailed account of his expected movements. This gentleman has been in America for several years, bringing with him an excellent record as a zoological investigator. Soon after his arrival in this country, he associated himself with the Montreal Society of Natural History, and has since that time been working sedulously in its interest. In 1867 he spent a fortnight in Gaspé Bay, where he prosecuted an extended system of dredging, and revisited the same region in 1869, extending his labours to the Gulf, between Cape Rozier lighthouse and Ship-head. Large numbers of marine invertebrates were collected by him, among them two species of shell new to America; but no dredging was prosecuted at a greater depth than sixty fathoms. The object of his expedition of the present year is to carry on work in deeper water, and for this purpose he expected to start in the schooner *La Canadienne* on the 5th of July, to cruise along the north shores of the Gulf as far as Anticosti, or beyond. He goes prepared to prosecute his labours in the deep sea (two to three hundred fathoms) on each side of that island; and from his experience in such researches, and the information derived from the later American and English deep-sea explorations, we have reason to hope for many important discoveries.—The cultivation of the natural and physical sciences has not been prosecuted with much success, as far as the announcement of new facts is concerned, by the Spanish-American races of the New World, although in nearly every State there is a society devoted more or less to such objects. Of late years, however, an increasing degree of vitality has manifested itself in these organisations, and there is reason to believe that in time they may be of considerable value. The most prominent institutions of the kind at present are in Mexico, namely, the Geographical and Statistical Society and the Society of Natural History; both of them publishing Transactions which embody much information in their pages. The Royal Economical Society of Havana has published a bulletin of its proceedings, although devoted more to historical than scientific subjects. The most active society in Havana, however, is the "Royal Academy of Medical and Physical and Natural Sciences." Of this Dr. Gutiërrez is President; Don Francisco de Sauvalle, Vice-President; Dr. Antonio Mestre, Secretary-General; Don José F. de Castro, Corresponding Secretary; Dr. Felipe Rodriguez, Assistant-Secretary; Dr. Ramon L. Miranda, Treasurer; and Dr. Juan Calixto Oxamend, Librarian—all holding their offices until 1873. Institutions in Brazil, Buenos Ayres, and Chili also exhibit a commendable degree of activity.

Prof. C. F. Hartt, of Cornell University, has lately issued a circular announcing his intention of starting on a fourth expedition of scientific research to Brazil. This gentleman has been long and favourably known for his efforts in regard to the exploration of that portion of South America, having made his first journey in 1865 as one of the *attachés* of the Thayer expedition under Prof. Agassiz. In 1867 he made a second journey alone to the Brazilian coast, taking Bahia as his centre, and covering an extensive area around that point, including a trip to the Abrolhos Islands. These two expeditions included a large part of the coast from Rio to Pernambuco, a district of about one thousand miles. A third visit was made in 1870 to the valley of the Amazon, to clear up certain points at issue between himself and Prof. Agassiz in regard to the geology of that country. The funds for this expedition were furnished in part by a friend of science, whose name he is not permitted to give, with a contribution by Colonel Edward Morgan of one thousand dollars, and by Prof. Goldwin Smith of five hundred, besides small sums from other parties. Assistance was rendered by the Brazilian authorities in furnishing a small steamer with a suitable amount of coal. The collections made on this expedition were very extensive, and embraced objects of all kinds, including ethnology and anthropology. The fourth expedition, now contemplated, is intended to complete the survey of the eastern part of the Amazonian Valley, especially in its zoological relationships, and further to investigate the Indian mounds of Marajo, and to collect data in reference to the languages of the people of the country. The sum estimated as necessary for this expedition is four thousand dollars, of which five hundred have already been contributed by Harvard University; and we trust that the friends of science who may have the means at their command will not fail to respond to the appeal of Prof. Hartt by furnishing pecuniary assistance, either without conditions or with the understanding that a certain portion of the collection is to be supplied to the contributors in return. It is understood that Messrs. Osgood and Co., of Boston, have engaged a series of articles upon the expedition, to be published in *Every Saturday*, and afterwards to be collected in book form.

The Hydrographic Office of the Bureau of Navigation of the United States has lately published a monogram upon the Marshall group of islands in the North Pacific. This group consists of two chains of islands, lying nearly parallel with each other, and running north-west and south-west from lat. 11° 50′ N. to 4° 30′ N., and from long. 167° E. to 173° E., covering an area of over 350 by 400 miles in extent, and very little known to navigators, the information hitherto on record being considered very unreliable. The eastern chain is known as the Radack, and the western as the Ralick, each numbering from fifteen to eighteen groups of low coralline islands, the greater number of which are fully formed atolls—that is lagoons of greater or less extent—with deep water and anchorages, surrounded by a chain of reefs, connecting islands, with one or more passages through the reefs into the lagoons, most of which are navigable for large vessels, besides which there are numerous boat passages. The earliest discovery of this archipelago is said to have been by Laévédra, in 1529; and the next visit made to them was by Anson, in 1742. Since then the islands have been touched at by different navigators at various times, although until the appearance of the report just referred to but little definite information had been brought together of the archipelago as a group. A missionary establishment was started on one of these islands in 1857, which continues to be successful to the present time. The inhabitants numbered, at the latest accounts, 10,000. They are expert navigators, and perform journeys throughout the group. They are dark, with straight hair, and are said to be intelligent and hospitable.—Mr.

Alphonse L. Pinart, the French naturalist, who is engaged in a scientific exploration of Alaska, announces his arrival at Unalaska on May 24, and his intended departure at an early day for Nusgajak. We hope to lay before our readers, from time to time, the important features of the progress of this expedition.—A correspondent of the *Weekly* writes to report the occurrence of a lunar rainbow at his residence, Oxford Depôt, New York, on the 2nd July last. At nine o'clock in the evening of that day a heavy rain-storm came up from the west, and when the sky was about half obscured a very distinct and beautiful rainbow made its appearance, having an arc estimated at 90°. The top of the bow was a deep blue, the lower side red; and between the two colours appeared a distinct hazy green. The moon was just rising at the time, and the perfect bow was visible for about five minutes, and partially distinguishable for a quarter of an hour.

## SOCIETIES AND ACADEMIES

### Paris

**Academie des Sciences**, July 24.—No elections took place, but the members were rather numerous, as a secret committee is to take place at the close of the public sitting to discuss the merits of candidates. The secret committee was rather long, and a lively conversation took place. M. Lacaze-Duthiers was presented at the head of the list. After him came M. Gervais, and on the third line MM. Dareste and Alphonse Milne-Edwards. Each of these four gentlemen has respectable qualifications. M. A. Milne-Edwards is the son of M. Milne-Edwards, the great naturalist, who is chief of the section where the vacancy is to be filled up.—In the public sitting, M. Chasles gave a new series of theorems, which are to be demonstrated; but as they belong to a certain family of properties, and arranged *seriatim*, the very enunciation of them is more than half of the work to be done. These theorems are sixty in number, and are styled "Properties of geometrical curves relating to their harmonic axes," but none of them are of primary importance.—An observation was sent to M. Leverrier with respect to the great bolide of the 18th July, which was seen at La Guerche (Cher). Its course was from ε Cygni to α Pegasi. No track but a great quantity of light, first white and afterwards red; local time 11h. 5m., duration 3". —Details were given by M. Sainte-Claire Deville of a bolide seen on the night of 17th and 18th March. A bolide was seen also in Italy by P. Denza at Moncalieri, but the accounts do not agree. It is supposed that P. Denza saw another bolide, which is not much to be wondered at, as the 18th of March is considered to be favourable for the appearance of large meteors, and P. Denza says he saw many of them on that very night when there was no moon.—P. Secchi sends a new letter "On solar protuberances and the relations between faculæ and spots; the communication cannot be condensed.—M. Delaunay presented a new volume of the "Annales de l'Observatoire National," the twenty-third of the collection, and full of observations. — The Academy appointed a committee for the Bordin prize, which will be given for the best memoir on the Comparison of the Natural Productions of South Africa, South America, and South Australia, as well as intermediate lands. The programme was very cleverly drawn up, and answers most admirably to the controversies on the "Origin of Species." The election was contested, and MM. Milne-Edwards, Brongniart, Elie de Beaumont, Quatrefages, and Decaisnes, were appointed. The report will be written with great care.—An invitation was addressed to the Academy by the International Congress, which will meet at Antwerp on the 22nd August; no formal answer is given to it.—M. Berthelot, who for some time had not published any report in the *Comptes Rendus*, attempted to give a very clever explanation of the immense explosive force of some organised compounds derived from nitric acid. He says that there is an intimate union between nitric acid and the organic matter upon which the acid has acted. But the action takes place with scarcely any heat being produced. The heat is kept in reserve within the molecules of the explosive body for future action. His theoretic views are supported by calorimetrical experiments. Thus an equivalent of nitric acid being employed in the manufacture of nitro-benzine, gives only 4,300 calorics, and in the fabrication of nitric ether 6,000 calorics. Nitric ether being inexplosible, the greater explosibility of nitro-benzine can be explained by the 1,700 calorics.—M. Milne Edwards presented, in the name of M. Joly, a paper on a transformation which *Palingenia virgo* undergoes before its final metamorphosis. These intermediate and imperfect metamorphoses are less scarce and exceptional than was supposed—M. Ledillat, who is a very learned Arabic scholar, as well as a very good astronomer, sent a paper to support his previous assumption relative to the immense number of Arabic words which are to be found in the French language. His views will be supported by every Frenchman acquainted with the Arabic language, and there are a good many owing to the occupation of Algiers, and all these Arabic etymologies were omitted systematically by M. Littré in his great Etymological Dictionary.—An electro-magnetic machine on a new plan for exciting continuous currents was exhibited on the 17th by M. Gramme, who was highly praised by M. Jamin. M. Bazin raised a claim for the priority of the invention, and a paper placed by him in the hands of the perpetual secretary in the sitting of the 10th July was opened. The description given by him is similar. But the construction of M. Gramme's machine was certainly in full operation by that time.—We learn with much concern that M. Saigey, a very clever philosopher and mathematician, who had contributed many very valuable papers to the *Comptes rendus*, and to several scientific periodicals, and who was the author of many interesting books on scientific matters, died from actual want during the Communist insurrection. He was found dead in his room on the 19th of May, after having been left unassisted during more than three days. M. Saigey was a genuine free thinker and a Republican by heart. He was expelled from the Normal School when twenty-four years old, under the Bourbons, in consequence of his liberal opinions. He remained true to his colours during his whole life, had never a single appointment from the State, and died of starvation when seventy-four years old.

July 31.—M. Faye in the chair.—M. Lacazes-Duthiers was elected a member by a large majority to fill the room of M. Longet. The new academician is a very accurate observer, who inaugurated his scientific career by discovering the extraordinary reproductive system of corals by the inspection of corals living on the Algerine sea coasts. Every year he has spent his summers at some sea-side station in order to enlarge his knowledge of inferior organisms. He will be a very useful member.—The election for a free member will take place on August 7. M. Belgrand, a meteorologist and an engineer, will very likely be returned. Some opposition is expected, although according to every probability it will not be successful.—A letter from M. Janssen was read with respect to the observation of the next total eclipse, but the discussion was postponed till after the next meeting of the Section of Astronomy, which intends to propose an expedition.

## BOOKS RECEIVED

English.—Handbook of British Fungi, 2 vols.: M. C. Cooke (Macmillan and Co.).—Sir Isaac Newton's Principia, edited by Sir W. Thomson and H. Blackburn (Glasgow: J. Maclehose).—A Digest of Facts relating to the Treatment and Utilisation of Sewage, 2nd edition. W. H. Corfield (Macmillan and Co.).—The Estuary of the Firth and Adjoining Districts Viewed Geologically: D. M. Home (Edinburgh: Edmonston and Douglas).

## CONTENTS

THURSDAY, AUGUST 17, 1871

## *THE STATE AND THE INDIVIDUAL*

TWO opposite views may be held as to the relation which ought to subsist between the Individual and the governing power of the State—views which, in their extreme form, may be expressed thus—the Paternal, in which the State does everything for the Individual, and the Independent, in which he is left to shift for himself in every respect, except protection from actual aggression by foreign or domestic foes. On the one hand, we are told that it is the duty of the State to have a paternal care over the morals and the welfare of its citizens; on the other hand, that the province of the Government is simply the protection of his person and his goods. To a certain extent both views are correct. The true function of the Executive Government may be laid down as the protection of the individual citizen, and of everything that belongs to him, against adverse influences that are not under his own control. A man's morals are his own concern, and the law has no right to interfere with them or to regulate them, any more than it has to interfere with his religion, provided that in carrying out his views of morality he in no way interferes with his neighbour's welfare or comfort. Then at once the injured party has the right of appeal to the assistance of the law to check his neighbour's aggressive morality or immorality.

Now the evils which a man may suffer from causes not within his own control are of two kinds—evils from his fellow-man, and evils from the forces of nature. Every Government acknowledges the right of its citizens to claim protection at its hands from the physical violence of others, whether foreigners or fellow-citizens. The whole of our military, naval, and police forces, and our criminal courts, form but a gigantic and enormously expensive machinery for securing this end. In the same manner our civil law-courts are designed as a security against the attacks of a man on his neighbour's pocket. But there are many other ways in which one man may suffer from another's misconduct, of which the State takes very little account, besides actually having his pocket picked.

To one of these we have recently alluded, in speaking of the loss of health which may be endured from one's neighbour's uncleanliness, or otherwise unwholesome mode of life. But, to refer more especially to pecuniary loss; if I wish to buy an article of a man, and send him the money, and he neglects to send me his merchandise, while pocketing my money, I have my remedy against him, if I can catch him, by a civil suit or criminal prosecution. If, however, he does send me something in exchange for my cash, but something that he knows perfectly well is not worth half the money, I am equally swindled; but my remedy against the cleverer rogue is extremely difficult, if I have any remedy at all.

A striking instance of the necessity for legislation in this respect is furnished by a report recently presented to the Royal Agricultural Society. The Agricultural Society employs a consulting chemist, the eminent Dr. Voelcker, whose business it is to analyse manures, feeding stuffs, and other materials sent to him by members of the society. The committee who have charge of this matter report that "they have to call the attention of the Council to the great number of inferior and adulterated manures and feeding stuffs sold to agriculturists, but that they hope that the determination of the Council to publish these reports will eventually do much to check such fraudulent transactions." Accordingly, here we have published analyses of manures, &c., in some cases with the names of the manufacturers or vendors attached, and including such items as the following:—"Carbonate and sulphate of lime, 48·77 per cent.;" "sand, 29·60 per cent;" "alkaline salts, chiefly sulphate of soda, 44·61 per cent.;" "a mere trace of ammonia, no phosphates whatever, a worthless mixture of green vitriol, crude sulphate of soda, gypsum, and sand, sold as the 'British Economical Manure,'" &c., &c.; many of these manures being warranted to contain a fixed proportion of organic matter, or sold as pure unadulterated phosphates. An oil-cake was found, on microscopic examination, to contain the husks of castor-oil beans, and to be totally unfit for feeding purposes; and this had already caused serious illness among the stock of several farmers.

So little dependence can be placed on the assistance of our complicated legal provisions in cases of this kind, that the only remedy suggested by the Agricultural Society is the publication of the names of the fraudulent manufacturers. Indeed, in one instance, the Society itself had to appear as defendant in an action for libel, and to pay nominal damages, while, at the same time, it was complimented by the judge on the benefit it was conferring on the community! Surely Justice was in this instance very blind.

There is no doubt that this publication, like the elaborate series of reports on the adulteration of articles of food published some years ago in the *Lancet*, will have exceedingly beneficial results, and high praise is due to the society for undertaking the work. But why should work of this kind devolve on any society or private journal? Why should individuals or private bodies be subject to the annoyance incidental to undertaking such a crusade? If it is the duty of the State to prosecute a man who picks my pocket in one way, why is it not the duty of the State to prosecute the man who picks my pocket in another way? The difficulties in procuring the necessary evidence might be greater, but would not be insurmountable. If it were impossible to summon in such cases a jury of experts to try the matter, any jury composed of men of business of ordinary intelligence would know how to value such evidence from a man of Dr. Voelcker's credit and experience, as that an artificial manure sold at 5*l.* 5*s.* per ton "would be dear at 2*l.* per ton;" that the "British Economical Manure" is "utterly worthless;" or that an oil-cake is "totally unfit for feeding purposes." If there is another side of the question, let us hear it; but as the evidence stands before us, there could be no difficulty in convicting these manufacturers of deliberate swindling.

Let us now turn to the cases in which a man suffers injury from the forces of nature beyond his own control. Here, again, there are certain principles acknowledged by the State; or rather, long precedent has sanctioned the application of principles in certain fixed directions, and in those only. No one will dispute that the Government is exercising its legitimate functions in building harbours of refuge at great expense, in instituting a series of astronomical observations, and issuing an elaborate series of charts for the protection of mariners against the unavoidable risks and dangers of a seafaring life. The extent to which pure Science should be assisted by the State is still one of the grave questions for discussion of the day. But when we come to purely domestic, and especially to agricultural matters, few people seem to think that the State has any rightful authority here. Except the mariner, the farmer's welfare is more dependent on his knowledge of natural phenomena than that of any other class of the community. Who can calculate the enormously increased material gain to the country, were we able, from any series of meteorological observations, to predict with moderate certainty the weather for a week? Since the first dawn of agriculture, crops have been ravaged by insect enemies: "the palmer-worm, the canker-worm, and the caterpillar," have been the farmer's foes for the past three thousand years; and at the present day very little more is known of the causes of, or the remedies for, these plagues than in the days of Israel in Egypt. Our apples, our turnips, our hops, our vines, our potatoes, to say nothing of our gooseberries and our roses, are subject every few years to all but absolute destruction, and we are content to sit by idle, and to trust that next year will be a good year because this year has been a bad year. The continued existence among the farmers of some parts of England, of sparrow clubs, notwithstanding what has been written about the benefit conferred on the crops by these birds, is a standing evidence of the dense impenetrable ignorance in which the mass of our population is steeped.

In other countries they manage matters differently. Few can doubt that a laborious series of investigations as to the causes of and the best means of preventing the potato blight or the turnip-fly, aided by the light of the most recent discoveries in biology, such as M. Pasteur has conducted in the case of the silkworm disease in France, would be productive of most important results. We have before us the "Third Annual Report on the noxious, beneficial, and other Insects of the State of Missouri, made to the State Board of Agriculture, pursuant to an appropriation for this purpose from the Legislature of the State, by Charles V. Riley, State Entomologist." The report contains descriptions, with woodcuts, of the most pestilent insects of the State, of their mode of propagation, and of the result of experiments in different methods for their destruction; and others of the American States have annually granted sums of money for similar purposes, money which we cannot doubt has fructified hundredfold for the benefit of the thrifty western farmers. The Central Government of the United States publishes "Monthly Reports of the Department of Agriculture," containing an immense mass of information as to the progress of agriculture at home and abroad, the rearing of cattle, market prices, meteorological observations for the month, scientific notes, and innumerable subjects of interest and practical value to the farmer. Is it not worth consideration whether we might not spend a little of the public money that is now wasted in perfectly useless non-scientific experiments, to forward practical researches which have for their chief object the benefit of large classes of our fellow subjects, and the increase of the prosperity of the country at large?

## MACNAMARA ON CHOLERA

*A Treatise on Asiatic Cholera.* By C. Macnamara, Surgeon to the Calcutta Ophthalmic Hospital. Pp. 557. (London: Churchill, 1870.)

THE literature of Cholera progresses with far greater strides than the scientific knowledge of Cholera. Here we have another large book devoted to the history of one disease, containing a digest of past information regarding the history, theories, and treatment of Cholera, and leaving us at the end with another theory of the disease, which is supposed to include the main practical facts in a useful form.

Discussions on professional matters are beyond our sphere, but as the subject of Mr. Macnamara's book is one of great public interest, especially at the present time, it may not be out of place to glance at it very briefly, from the scientific aspect of some of the questions dealt with by the author. The historical part of the work consists of statements and opinions of different writers of the most opposite kind. It is scarcely too much to say that these describe Cholera to be unquestionably contagious and as unquestionably non-contagious; that it is importable by ships and not importable; that its progress can be arrested by quarantine and that quarantine is a useless precaution; that it is communicable by clothing and that it is not so communicable; that it does and does not attack people living under the same unhealthy conditions; and that it can be cured by certain methods of treatment and that it can't be so cured. And then to crown the whole, we have theories of the disease which are as contradictory to each other as the facts. It appears to us that when we are confronted with evidence such as this, we can only arrive at one of two conclusions, either that the observations were one-sided and the logic wofully defective, or that there were reasons for the apparent contradictions requiring careful scientific study. Mr. Macnamara's own views about Cholera may be briefly summarised as follows:—

1. That the cause of Asiatic Cholera "is invariably a portion of the fomes of a person suffering from the disease." 2. That this must be in what is called the "vibrionic stage" of decomposition. 3. That it must be swallowed. 4. That it causes changes in the intestinal epithelium similar to its own, and that the epithelium is as it were washed off by the efflux of serous matter and passes away in the discharges. 5. That the organic cause of Cholera may be preserved dry for years. 6. That water is the most common medium of its diffusion, but that it may be carried and may act in foul air; and, lastly, the author says, "with the exception of the specific Cholera-infecting matter, I entirely ignore all other causes or combination of causes as capable of producing this disease." This last position, which is left unproved, is, indeed, the foundation of the theory. The theory is much the same as that put forward by Dr. Snow, Dr. Budd, Dr. Farr, and others, with a theoretical addition from another quarter as to the manner in which the poison acts.

The first remark which we would make is, that it there exists a Cholera germ, matter, *Cholerine*, or whatever else it may be called, its existence can be proved.

But the supposed pathological action of the matter, if it exists, may be considered as disposed of by Dr. Lewis's Report on the Scientific Inquiry into Cholera in India, in which he tells us "that the flakes and corpuscles in rice-water stools do not consist of epithelium or its *débris*." There being no *corpus delictum*, "Cholera infecting matter" cannot act in the way supposed. The whole structure falls to pieces whenever the light of scientific observation is brought to bear on it. We are thus left to deal with the other half of the theory, namely, the "Cholera germ," which is supposed by some to be of fungoid nature. But when we look for proof of its existence, we find only inference. The "fungoid" bodies which, by another modification of this theory, were supposed to be the agents in removing the epithelium, have been shown by Dr. Lewis to differ in nothing from similar bodies in healthy discharges, so that this fungoid theory has stood the test of observation no more than the "vibrionic" theory.

Instead of supplying the place of fact by inference or theory, would it not be better once for all to discard both, and try another method of arriving at truth regarding epidemic diseases? We agree with Prof. Tyndall as to the importance of physical research in such questions. Its methods are precise and rigorous, and by taking no cognizance of what is unproved, it may eventually do much in reconciling all the diversities of medical observation, and in opening out entirely new fields of investigation. Under the Government of India a most important scientific inquiry into Cholera is now being carried on in that country; and to all appearance the time is at hand when the most competent scientific men in Europe will have opportunities enough of dealing with the subject. Let the inquiry be strictly scientific. Let us refuse absolutely to admit anything of which we have not scientific proof, and we shall at least be able to divide between the known and the unknown.

Notwithstanding these criticisms on scientific points, Mr. Macnamara has written an interesting book which will well repay perusal. Amongst other things, he gives an account of the various practical sanitary proceedings which have been in use for mitigating attacks of Cholera.

Setting aside all theories about their action, it is satisfactory to know that with temperance in diet, attention to clothing, pure water for drinking and cooking purposes, and rigid cleanliness of towns, houses, and persons, as well as in ships, there is little to fear from Cholera epidemics. There is no theory needed to help us to understand these things. They simply require to be done. Volumes of instruction will not make the duty of doing them plainer than it is. These are, moreover, the things which are especially required for India, and we heartily second Mr. Macnamara's appeal to Lord Mayo, and to our present scientific Minister for India with which he concludes his work:—"The question for the consideration of the Government, is nothing less than this: Shall Cholera be allowed by our mismanagement or neglect to become permanently localised throughout the civilised world. It is to the condition of the inhabitants of the Gangetic valley that our attention and efforts must be primarily directed, if Asiatic Cholera is ever to be effectually controlled by human agency."

## OUR BOOK SHELF

*Papers on the Great Pyramid.* By St. John Vincent Day, C.E., F.R.S.E., &c. (Edinburgh: Edmonston and Douglas, 1870.)

*The Great Pyramid of Jizeh: the Plan and Object of its Construction.* (Cincinnati: R. Clarke and Co., 1871.)

THE investigation of the history and origin of the Pyramids, and the attempt to arrive at the truths that are hidden in these, the greatest monuments of antiquity, is undoubtedly of the first importance, but must nevertheless be entered upon with caution. There is a danger about such a study which few seem to escape, a danger of being enslaved by some theory which becomes absolute master of the man who originated it, which makes him see everything through a false medium, and in support of which he perverts facts in the most marvellous manner. Mr. Day, the author of the "Papers on the Great Pyramid," has avoided the danger to this extent, that he brings forward no new theory of his own, but places his entire faith in Prof. Piazzi Smyth, the Astronomer Royal for Scotland, to whom this volume is dedicated. The papers are three in number; the first is a critical examination of Sir Henry James's "Notes on the Great Pyramid of Egypt," and would not have been written, the author tells us, had not Sir Henry himself opened and continued a correspondence with him on the subject, and had not he felt "the promptings of duty to expose fallacies so authoritatively flung into the midst of mankind." The two other papers are entitled, "The Measurements of the Great Pyramid recorded in history," and "An examination into the condition and works of mankind from the Creation to the building of the Great Pyramid." More than half of the entire volume is occupied by the first paper, and in it Mr. Day examines in the most minute manner every one of the eight "notes" he undertakes to controvert. He has succeeded in showing that Sir Henry James has been, to say the least, careless in his assertions, and even in his arithmetic, considering how positive his statements were. The general impression left upon the mind of the reader is that until the measurements of the pyramid have been ascertained without a shadow of doubt, no man has a right to base upon them positive assertions as to standards of length. Notwithstanding defects in his mode of treatment of the subject which it is hardly within our province to criticise, the book recommends itself to those who are interested in the Great Pyramid controversy, as it is evidently the result of careful study.

We also wish to notice in this place a small pamphlet on the Pyramid of Jizeh, which has come to us from across the Atlantic. The author does not profess intimate acquaintance with his subject, but acknowledging that such suggestions must be made with much reserve, points out certain relations he has discovered between the measurements of the pyramids and "time, extension, and earth space." They are certainly ingenious, but hardly, we think, much more. As illustrating what was said about the danger of indulging in such speculations, the author concludes by abruptly exclaiming that the fact of the English inch and English foot running "in such admirable rhythm with time and pyramid measure," may be a link of connection between the Anglo-Saxon and Hebrew races. Then plunging still deeper into this dangerous line of thought, he says, "were the blind eyes opened, it is quite possible that here in this new world of ours one would suddenly come to the realisation that he was dwelling in the midst of the teeming multitudes of Israel; terminating their emigration in a land long promised, long reserved; under government of a commonwealth restored; free from every taint of caste condition, or of kingly rule."

Although this pamphlet seems distinguished more by ingenuity than by any real value, it possesses the merit of being short, and is written with a reserve proper when dealing with such a subject. R. B. D.

*Domestic Botany: an Exposition of the Structure and Classification of Plants, and of their Uses for Food, Clothing, Medicine, and Manufacturing Purposes.* By John Smith, A.L.S., Ex-Curator of the Royal Botanic Gardens, Kew. (London: L. Reeve and Co., 1871.)

THE greater part of this thick volume consists of a brief description of the distinguishing characters of the most important orders of Flowering and Flowerless Plants, with a longer account of the more striking species belonging to each order which are cultivated in or imported into this country on account of their economical properties. The author's official connection with the Gardens at Kew gave him unusual opportunities for an acquaintance with plants of this class, and we do not know any work which contains so much useful and interesting information on the subject. Under the head of the Palm Family, for instance, we find no less than forty species mentioned, from which are obtained so large a proportion of the articles of food, dress, domestic use, and commerce, that supply the scanty needs of the inhabitants of tropical countries, cocoa-nuts, dates, oil, wax, toddy, sago, betel nuts, vegetable ivory, umbrellas, fans, &c. The book is, in fact, a repertory of information as to the history of articles derived from the vegetable kingdom. The work being evidently intended mainly for popular use, we doubt the wisdom of the word-coining so extensively adopted by the writer in structural definitions, especially as, in the preface, he refers to the deterrent influence on the study of botany, of the many technical terms with which other works on the subject abound. The following description, for example, of the habits of the Arum family, will convey but little idea to the general reader, even if the botanist can extract a definite meaning from it:—"Palmids, phyllacorms, epiphytal ampelids, or rhizocorm herbs, generally of a soft texture, destitute of pubescence."

We wish we could speak with equal praise of the earlier portion of the work, the "Explanation of the Parts, Structure, Life, Organism, and Classification of Plants." This has evidently been prepared too hastily, and not subjected, as it should have been, to a careful revision by some one familiar with at least the elements of Structural Botany, which would have prevented the use of such barbarous terms as "involucræ" and "phyllodæ," or such a definition as that "an ovary, with its pistil, is termed a carpel." These chapters by no means answer to their title of an "Exposition of the Structure and Classification of Plants;" a student trusting to them, instead of to one of the many excellent manuals already in existence, for his knowledge of structural botany, would be wofully misled; and the author has made a grave mistake in attempting a treatise on this subject. This is the more to be regretted, as the inaccuracies in the earlier part may deter the reader from proceeding to the main portion of the work, which is really useful and trustworthy. The coloured illustrations with which the book is adorned are very well executed; the woodcuts are on too small a scale to be of much assistance. Notwithstanding the valuable features of this book which we have pointed out, an exhaustive work on Economic Botany is still a desideratum in our literature, and one that would repay the labour of any one who possessed the needful information, and the power of putting that information into scientific and yet easily intelligible language.

A. W. B.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Ocean Currents

I HAVE been watching with interest for some time past the discussions in your pages regarding Dr. Carpenter's theory. I have also read with much pleasure in your number for July 27 Prof. Wyville Thomson's excellent paper on "The Distribution of Temperature in the North Atlantic." In it he justly remarks that "what we expect of Dr. Carpenter before we are called upon to accept to the full his magnificent generalisation is a calculation and demonstration of the amount of the effect of the causes upon which he depends, acting under the special circumstances." And he further adds, that he has several times put the question to specialists in such physical inquiries, but they have always said that it was a matter of the greatest difficulty, but that their impression was that the effects would be infinitesimal.

I have examined this subject with great care, and may be permitted to say that, in so far as the point at issue is concerned, the problem, if properly treated, is by no means difficult, but on the contrary is one of great simplicity.

Taking Dr. Carpenter's own data, I have, in regard to the Gibraltar Current and to his General Oceanic Circulation, determined the absolute amount of those effects on which his circulation depends. Taking a given quantity of water, say one pound, placed under the most favourable circumstances, according to his theory, I have determined, first, the absolute *force* of gravity acting on the pound of water tending to produce motion, and, secondly, the absolute amount of *work* which gravity can perform upon the pound during its entire circuit.

I can form, of course, no estimate of the amount of the work of the resistances to the motion of the water along its course. But, imperfect as our knowledge is on this point, we can nevertheless easily satisfy ourselves that the work of the resistances greatly exceeds the work of gravity, and that, consequently, there can be no such circulation as that for which Dr. Carpenter contends.

My results are embodied in a rather lengthy paper on the subject, the publication of which has been delayed, owing to circumstances over which I have had no control; but I expect that it will appear in the *Philosophical Magazine* for October next.

Edinburgh JAMES CROLL

### The August Meteors

HAVING been engaged during the past week in observations on the August Meteors, I thought a few of the results might be interesting to some of your numerous subscribers. My regular observations extended from Sunday night to Friday night; and, as the following table will show, the weather was, with the exception of one night, as favourable as could reasonably be desired. From over 120 meteors mapped down (out of about 330 seen) it is evident that the principal radiant point, or rather line, is a line drawn from $\alpha$ Persei to $\gamma$ Persei, and onwards towards $\eta$. One bright meteor was seen on the 8th, just below $\eta$ Persei, which did not move more than $\frac{1}{6}°$ in a second of time, and left a *cloud* behind it lasting about two seconds. A remarkable feature was the *outlying* radiants, as they appeared to be, one of which was situated at or near $\theta$ Cassiopeiæ, another near the star $c$ of Camelopardalis. The radiant situated between $\delta$ Cygni and $\gamma$ Draconis is very well marked; also a radiant near $\gamma$ Cephei (where another almost stationary meteor was observed); and one just below $\epsilon$ Pegasi, towards $\alpha$ Aquarii; associated apparently with the last is a radiant near the small lozenge in Delphinus, above $\alpha$ Aquilæ.

In the list below of 312 meteors observed here, 242, or about 77 per cent. were from the Perseus radiant or radiants:—

*Meteors seen August* 1871, *at York.* !

| Date. | Hours. | State of sky. | No. observed. | Do. from Perseus. | Proport. |
|---|---|---|---|---|---|
| 5th | 10. 0—10. 10 | Fine. | 6 | 5 | ·83 |
| 6th | 10.20—12. 0 | do. | 34 | 28 | ·82 |
| 7th | 10. 0—12. 0 | do. | 49 | 30 | ·61 |
| 8th | 10. 0—12. 0 | do. till 11 45 then cloudy. | 50 | 31 | ·62 |
| 9th | 10.30—11.30 | Cloudy and hazy | 6 | 4 | ·6 |
| 10th | 9.55—12. 5 | Few clouds at times, and very slight haze. | 120 | 106 | ·88 |
| 11th | do. | do. | 47 | 38 | ·80 |

Generally two watching, sometimes three, and once or twice but one. For the 10th I had a list of twenty-six others handed me, observed by a friend close at hand, of which nineteen were from Perseus.

20, Bootham, York, Aug. 14 J. EDMUND CLARK

### Daylight Auroras

I HAVE frequently seen the appearances described by Mr. Winstanley in your issue of August 3, and I think I must have seen the one he mentions. I have on two occasions watched similar phenomena caused by the moon. These phenomena require the cloud or clouds, from which they are formed, to be of about the same azimuth as that of the sun (or moon), and vary with the form and motion of the cloud, being, I think, simply a deflection of the sun's rays from the more salient points of the cloud. The streamers I saw on June 27 (see NATURE of July 9) were of an entirely different nature, rising near the south horizon somewhat to the east of the meridian, and flashing towards the moon, which had recently passed the meridian, while the sun was near setting in the N.W.

I have had nearly thirty years' experience in observing, and have no doubt that what I described were true streamers of an aurora australis. JOHN LUCAS

Radcliffe Observatory, Oxford, Aug. 17

### The Late Thunderstorm

THE thunderstorm which has just broken up the spell of hot weather for the past week presented some peculiar features as seen from this place. Commanding rather a wide horizon, I noticed about 8.30 last evening two distinct centres of disturbance, one S.W., the other N.E. It is impossible to estimate the distance by sound, as the thunder was inaudible. The electric spark, however, was visible enough, and I noticed that with one exception it went invariably from S. to W. in the one case, and from N. to E. in the other, and always horizontally from cloud to cloud. When I came out again at 10.15 I found the two centres had moved, so that one was slightly S. of E., and the other N. of W., but still directly opposite each other. It seems, too, that they had gone in the direction in which the electric spark had passed.

This seemed to me interesting, and I thought your readers might find it so too.

Upton-on-Severn, Aug. 14 W. M. ROBERTS

### Sir William Thomson and the Origin of Life

I AM sure that Sir William Thomson will feel gratified rather than annoyed to be informed that he has been anticipated in his remarkable hypothesis regarding the origin of life on our globe.

In a very curious book called "A Visit to my Discontented Cousin," published some months ago, there is a portion of one chapter headed "The Aerolite." The "Discontented Cousin" having seen and heard a discussion on a meteoric stone, went home and "dreamt a dream," in which he saw the surface of the mass undergo various changes, and organic dots appear, one of which began to wriggle, rose to its microscopic legs, and confronted "the dreamer with a bold and self-confident mien."

This microscopic man, after having enjoyed "a glass of something stiff and a pipe," told the story of his own planet, beginning with the not very complimentary remark, "We know all about you, old boy, and the British Association; and we don't think much about you, either."

For the story itself I must refer to pp. 186-192 of the book.

Torquay, Aug. 8 G. E. D.

### Meteorology at Natal

IN your issue of November 10 last I was glad to see that you were alive to even humble efforts to assist science in so distant a place as Natal by your remark on the new Meteorological Observatory for the Durban Station.

As it may be interesting also to know what has been done, I append a list of the instruments ordered by the Natal Government. They are to supplement a few already on board. Unfortunately for the furtherance of this object, the state of affairs at present will not admit of any large expenditure of public money in this direction, but I have been enabled to get the sum of 10*l*. in 1870 and 25*l*. in 1871 for the purchase of instruments. The Government, at the same time, pay the observer 12*l*. per annum for the trouble of registering the results regularly.

At present the military authorities have an observatory at Fort Napier, in Pietermaritzburg, at a height of 2,200 feet above and forty-five miles from the sea. A station on the coast was required to complete the observations, and the Government liberally came forward with the means. Hitherto a system of registration has been followed in which the instruments were suspended in wooden boxes against the sides of dwelling-houses. Though such a system, perhaps, is advisable as giving the temperature ordinarily felt by residents, it nevertheless is more or less uncertain for the purposes of comparison, from the fact of some of the dwellings being built of wood and others of stone, and again some roofed with slates and others with galvanised iron.

The present system is that pursued by the military authorities. The thermometers are placed under a Glaisher's stand in the open air. Whether the military authorities are right or wrong matters little so long as one universal system is pursued, so that exact comparison can be made between each station. The results will always be sent to the Meteorological Society of England, and published in the Colonial Blue Book for each year.

VINCENT ERSKINE

Pietermaritzburg, Natal, May 16

P.S.—I expect the Observatory to be in thoroughly good order from the 1st of January, 1872.

### On the Colours of the Sea

THE following is submitted on the above subject, referred to in NATURE of July 13, as showing that the colour of the sea is not altogether dependent on the purity and depth of the water. At Zante, and southward as far as I have seen, it is of a deep blue at midday, but in the evening it is that described by Homer as "wine-like." I have observed this particularly when passing Navarino and Cape Mataplan. At night, looking down upon it from the steamer, it is quite black, lighted up, where the waves are broken, with white phosphorescent light.

I had once the gratification of seeing the whole of the solar spectrum spread out upon the sea, at Zante, on December 26, 1861. The weather was very unsettled at the time. During an interval, when the rain had ceased, a little before 10 A.M., the light of the sun descended from behind a cloud, and was reflected up to the height on which I was standing. Purple was the most remote colour; red the nearest; the space between was occupied by the other colours, of which green, yellow, and blue were the most marked. JOHN J. LAKE

### Origin of Cyclones

IN NATURE of 23rd of June, 1871, there is an account of a paper, by Mr. Meldrum, on the origin of storms in the Bay of Bengal, showing reason to believe that the cyclones of the Bay of Bengal and the Southern Indian Ocean originate in the meeting of the trade-winds of the northern and southern hemispheres at some distance north or south of the equator. I do not know of any equally complete evidence on the subject for the cyclones of other parts of the world, but there is very strong reason for thinking that they always so originate. The line along which the two trade-winds meet each other approximately coincides with the equator: when it actually or nearly coincides with the equator, no cyclones are formed, because the rotation of a cyclone depends on that of the earth, and the earth at the equator has no rotation round an axis drawn vertical to the horizon. Over the greater part of the Pacific, cyclones do not appear to be formed: the reason of this probably is, that in consequence of the temperature of the sea changing but little with the seasons, the two trade-winds over the Pacific meet each other nearly on the equator all the year round; though I do not know how far this is confirmed by observations on the winds of that ocean. But we know that in the Indian Ocean the trade-winds cross the equator and are deflected into monsoons, so that in the summer of the northern hemisphere they meet to the north of the equator, and in the summer of the southern hemisphere they meet to the south. (This statement as to seasons will have to be qualified presently.)

We may consequently expect to find that the farther the sun is from the equator, the farther from the equator will be the meeting of the trade-winds, and consequently also the cyclones. This is the fact. In Dove's "Law of Storms," translated by Mr. Scott, at page 193, there is a chart of the tracks of the cyclones of the Chinese Sea, which shows that they occur in all months from June to November, and that the later in the season the nearer to the equator is usually their track. In the Chinese Sea, where they are called typhoons, they are most numerous in the summer months; in the Bay of Bengal they are most numerous after the equinoxes. This will appear quite intelligible if we regard the cyclone region of the Chinese Sea as an extension of that of the Bay of Bengal; it will then be seen that the cyclones follow the sun. This, however, must be understood with the qualification that they follow the sun at some distance; the number of cyclones in the Indian Ocean appears to reach its maximum a month or two after the equinoxes. This is for the same reason that the warmest period of the year is not at but after Midsummer.

The distribution of cyclones in the West Indian Seas is to be explained in the same way. The two trade-winds meet in the Atlantic a little to the north of the equator; for this reason cyclones are frequent in the West Indies but unknown over the South Atlantic, and they are most numerous at the end of summer. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

### Saturn's Rings

AN absence in the country prevented my seeing Lieut. Davies's letter in time for an earlier reply. I will answer him on all points, and have done with him, for he employs unfair arguments to impeach me.

1. I defy him to point out the smallest word or slightest expression in my remarks on his work that justifies him in asserting "that I commenced my notice very much under the impression that Prof. Clerk Maxwell having investigated the stability of Saturn's rings, no one else is to venture into any discussion on their nature and origin." I never for a moment entertained, much less expressed, such a thought. I simply pointed out that Prof. Maxwell had shown rings of satellites to be the only ones which could exist, and I said merely that Lieut. Davies, "having espoused this theory, had sought an explanation" of it. Lieut. Davies's allusion to a *caveat* is therefore an empty flourish.

2. Lieut. Davies says I "assert" that he has not seen Prof. Maxwell's work. This is an unpardonable misstatement. I said he "appears not" to have seen the work; and I was driven to this assumption, since Lieut. Davies, while actually using Maxwell's labours, never mentions his name. Certainly he is at liberty to choose his own starting point: but he should credit the well and not the bucket (Mr. Proctor will pardon me) for his inspiring draught.

3. As to faith in figures. I take for granted that no rational man would publish numerical data unless he believed his figures really to mean what they stand for. Now, the rate of the solar motion is not known to within a thousand miles an hour, and the solar parallax is only certain to the first place of decimals. As Lieut. Davies prints the first of these data to a mile, and the second to four decimal places, he has clearly too great faith in figures. He may say that "other observers" (why observers?) do the same; that does not excuse him. Further, a different rate of solar motion must alter his spirals; if there were no motion he would have no spiral.

4. I know that "very clever men" have held the meteoric theory of the sun, but I also know that "very clever men" have held other theories. Lieut. Davies, in denying my assertion that he is "blindly enraptured" with the meteoric theory, actually supports me; for when he says that "none of the modern theories, 'cumbrous vagaries of the brain,' can compare with it," it is clear that he cannot see the fairness of any crow but his own, and this is blind infatuation.

5. Either we are not agreed upon the meaning of "cyclonic," or Lieut. Davies is sun-spot blind. I call such a spot as that reproduced on page 232 of Mr. Proctor's book "cyclonic," and I have seen many, both on the sun and in drawings, of his character. Lieut. Davies's range of observation must be limited if he has not seen some also. Mr. Carrington's work is quite beside the question; he did not delineate spots, he merely measured and counted them.

YOUR REVIEWER

### Extinction of the Moa

THE very interesting article on the Moa in your issue of July 6th by Dr. Hector adds considerably to the facts already ascertained as to its existence along with man, and also as to the probability of its recent disappearance. Visiting in 1866 and 1867 many of the places mentioned by the Doctor in the Middle Island, I had opportunities of seeing portions of their remains in various conditions, either in caves, river sides, or in the open country where cultivation was going on, or on the sides of the hills in the interior, and certainly the impression produced was, that not perhaps more than fifty years had elapsed since some of the remains had formed part of living birds.

On the Kourow range of hills in the north of Otago, I saw Moa bones and those of a wild pig in close proximity; and, though certainly those of the former were more weathered, taking into consideration the greater density of the latter, it did appear as if there had not been a great many years between the deaths of each. I have also some bones in my possession, and there are others in the Geological Society's Museum in Edinburgh, found on the surface of the Carrick ranges, a place alluded to by Dr. Hector, the unexposed portions of which do not seem very aged. While agreeing with Dr. Hector that there is reason to believe that the last of the race have only of late disappeared, viewing the question from the point of an agriculturist, I differ somewhat as to the causes. He says that the facts he adduces "afford strong evidence that the bird has been exterminated by human agency, though the race was expiring from natural causes." It seems to me that in such a country as New Zealand their loss has arisen from natural causes, though the Aborigines may have assisted somewhat to diminish their number. Dr. Hector admits that there are still portions of Otago where the foot of man has scarcely trod, notwithstanding the search for gold, perhaps the most eager which can exist. The Moa had these districts to retire to. In the Middle Island very few Maories dwelt, and their numbers were kept down by the forays of the more warlike inhabitants of the North Island. The pigs, supplied to the natives by Captain Cook, have spread over the island and increased largely, and have only been prevented from still further increasing by the use of the musket and the occupation of the country by the settlers. While the Kiwi still maintains its place, it is hard to believe that man has exterminated the Moa. The natural causes, however, it seems to me, are quite sufficient. Dr. Hector speaks of large fires having spread over the centre of Otago. It would appear that the pine woods, which have covered so many of the hill sides of the interior, had reached a certain stage of decay, and, from the occurrence of droughts less severe than those of Australia, perhaps, fires lighted by the natives had spread to these woods, and though an undergrowth of fern and moss might retard the progress when once the pine timber, with its resinous qualities, thoroughly caught, there was little chance of it going out save from breaks caused by rivers or bare places. As these fires spread over the interior, destroying everything in the shape of bush or tree, the native grasses took their place, none of which seemed to me to afford fitting food for such a bird, and with these grasses a plant called by the native Tutu occupies much of the country. The leaves of this plant are, under certain conditions, destructive to live stock; while the berry which it produces may be eaten with impunity, provided the stone it contains be not swallowed; the settlers making a wholesome jelly of the pulp, and sometimes wine. Here, then, we have a vast portion of the country cleared of its food supplies, from the trees and shrubs which produced it being destroyed, and we have a poisonous plant abounding, which does not grow freely under wood. Dr. Hector speaks of counting thirty-seven skeleton heaps of Moas on the side of the Wakatipu Lake, and supposes that they had been driven there by fire. This may be so, but I have seen a great many similar heaps, in the centre of each of which two or three handfuls of quartz pebbles lay on the flat alluvial lands on the sides of streams and near the seashore, where beds of gravel then were, to which the birds could have retired, had they been pressed with fire, as no vegetation could have existed there. It therefore seems to me that the reason why so many skeletons are found on the surface near streams or water, arises from the fact that these creatures, pressed by hunger, partook of the Tutu berries, and that thirst, which so often accompanies poisoning, caused them to take to such places for drink. I have heard it stated that in periods of drought the Emus of Australia travel great distances for water. Though water is far more abundant in New Zealand, it is often only in the streams that it can be had. JAMES MELVIN

## NOTES

THE following is the programme of the subjects to be submitted for discussion at the International Congress of Anthropology and Archæology, to be held at Bologna from the 1st of October next:—The stone age in Italy; 2. The caverns of the shores of the Mediterranean, especially of Tuscany, compared with the caves of the south of France; 3. The lake habitations and mounds of the north of Italy; 4. Analogies between the Terrawaris and the Kjoekenmoeddings; 5. The chronology of the first substitution of iron for bronze; 6. Craniological questions relative to the different races which have inhabited the various districts of Italy.

Dr. Benjamin T. Lowne has been elected Lecturer on Physiology at the Middlesex Hospital School of Medicine.

The Society of Civil Engineers of Paris has just elected as its president for the coming year, M. Yvan de Villarceau, the chief astronomer of the Observatory, and has conferred the title of honorary president on M. Tresca, vice-director of the Conservatoire des Arts et Métiers, as a testimony of high admiration for his conduct during the siege of Paris, and under the reign of the Commune.

We understand that it has been decided to erect a statue to Sir Humphrey Davy in his native place, Penzance. By the exertions of a working committee, a sum of 500*l.* has been raised in subscriptions A very eligible site has been obtained from the Town Council immediately in front of the Market-house and facing the main entrance of the town. The Messrs. Wills, of 172, Euston Road, have been commissioned to execute the statue. The statue is designed after Sir Thomas Lawrence's portrait, painted for the Royal Society. The total cost of the statue and of erecting it on the site provided is estimated at 600*l.*

A portion of the surplus funds from the International Horticultural Exhibition of 1866 was invested in trustees and applied to the purchase of the botanical library of the late Prof. Lindley, to be called the Lindley Library, and to serve as a nucleus of a consulting library for the use of gardeners and others. Considerable additions were made to the library by gift, a catalogue was prepared, and the books deposited in the rooms of the Royal Horticultural Society at South Kensington, but here the matter was allowed to lie dormant for a considerable time. The trustees have now just issued a circular, stating that the library is now open for the use of the public under certain regulations. Fellows and officers of the Horticultural Society have access to the library at all times when it is open, gardeners and others not fellows or officers of the society by application to one of the trustees, or to the assistant-secretary of the society. Under certain restrictions those using the library can have the books out on loan; and, as it contains a very large number of standard botanical and horticultural works, it is hoped it may be of great practical service. The trustees will be very glad of assistance in completing imperfect sets of periodicals and works published in parts, and in adding recently published treatises, for which the funds at their disposal are quite inadequate.

The *American Naturalist* states that among the signs of the scientific life of the present day in that country, one of the most encouraging is the increasing frequency and enthusiasm of those delightful occasions of scientific study, intercourse, and recreation, called field meetings.

At a meeting of the Faculty of the Museum of Comparative Zoology of New York, held May 6, 1871, the Humboldt Scholarship was awarded to J. A. Allen, in consideration of his paper upon the "Mammals and Winter Birds of East Florida," and the proceeds of the Humboldt Fund for one year were granted to him in aid of his exploration of the Fauna of the Rocky Mountains.

Here is a transatlantic hint to our scientific colleges and schools:—Mr. Albert H. Tuttle has been appointed instructor in the use of the microscope at Harvard University.

We find in the *American Journal of Science* for July a more detailed statement of the result of the Williams College expedition than has heretofore been published. This consisted of five members of the present senior class, under the leadership of Mr. H. M. Myers, who gained much experience in the line of exploration in connection with the Venezuelan branch of Professor Orton's expedition of some years back. We have already referred to the movements of this party, and it is only necessary to add that large numbers of birds were obtained by the expedition at Comayagua, as well as two statues, exhumed at Chorozal, south of Belize. The collections made by the party will go to enrich the Williams College Lyceum of Natural History, and will add much to its already extensive treasures.

The late Mr. James Yates, M.A., F.R.S., has left 200*l.* to the Geological Society, and 50*l.* to the Linnean, and 100*l.* to Prof. Levi towards the adoption of a universal decimal system of weights, measures, and coins, in addition to the large sums of money devised to University College, London, towards the foundation or augmentation of professorships in mineralogy and geology and of archæology. To the same College he leaves all his books on mineralogy and geology, together with his specimens and his collection of ancient coins and other antiquities.

The Sub-Committee appointed by the Asiatic Society of Bengal to consider the desirability of undertaking Deep Sea Dredging in Indian waters, have presented a memorandum on this subject, signed by Thomas Oldham, Ferd. Stoliczka, and James Wood-Mason. After recapitulating the important results which have accrued from European Dredging Expeditions, the Sub-Committee state that they are confident that explorations of the Deep Sea in Indian waters will not only furnish data which will illustrate the modification of certain supposed laws regulating animal and vegetable life in countries geographically and climatologically different, but that they will undoubtedly supply much and most important material for the study and explanation of many yet obscure facts in zoology, geology, physics, and the collateral branches of science. They, therefore, earnestly hope that Government may be led to regard the undertaking of Deep Sea Dredging in Indian waters as the most important source whence great progress to natural history and physical science will result. The Committee suggest the examination of the Bay of Bengal by a line of dredging right across from new Juggurnath Black Temple to Cape Nagrais, to be followed by another transverse from near Madras to the Andamans or the Nicobars, and again by a line from Ceylon to the coast of Sumatra. It would be necessary that, say three persons acquainted with the mode of inquiry should accompany each expedition, and it is hoped that sufficient accommodation could readily be found for them on board. They then describe the apparatus that would be required, and state their belief that an annual grant of 2,000 Rs., placed at the disposal of the Dredging Committee, would be sufficient for the objects desired.

Mr. Thomas Bland, who has long studied the land shells of the West Indies, is now endeavouring to elucidate their distribution by the help of the depth of the sea between the different islands. The materials are as yet imperfect, but in a paper read before the American Philosophical Society in March 1871, he announces that the depths so far as known agree with the distribution of the various groups of shells. He finds that the whole West Indies may be divided by a line south of Santa Cruz and St. Bartholomew, and north of St. Christopher and Barbuda, and that all islands south and east of the line show an affinity to Venezuela and Guiana in their shell fauna, while those to the north and west of it are similarly allied to Mexico. All the southern islands, as far as St. Vincent, are situated on a submerged bank of about 2,000 feet deep, extending from the main land of South America, and these all possess shells of a more especially continental character than any other part of the West Indies. Some very interesting results may be expected when the sea bottom of the Gulf of Mexico shall have been more accurately surveyed.

The last Report of the Juvenile Literary Society of the Friends' School, Croydon, shows that natural history is in no way neglected by the members. Twelve boys have been collecting British plants, two collections are being made to illustrate botanical terms, and two to exemplify the British natural orders. Nearly three hundred "varieties" (? species) of plants in flower have been exhibited in the school-room, and some additions to the flora of the district have been discovered. Observations on the weather and the recurrence of natural phenomena have been kept up; and collections illustrating the ornithology and conchology of the district are in progress. Additions to the library and museum are acknowledged, and the treasurer's report shows a balance in hand.

The Transactions of the Maidstone and Mid-Kent Natural History and Philosophical Society for 1870 are chiefly remarkable for their total want of local matter. Papers are printed on "Sericiculture," "The Nervous System," "Skin and its Appendages," "Natural Selection," "The Similarity of Various Forms of Crystallisation to Minute Organic Structure," and "The Geometrical Structure of the Hive-Bee's Cell," none of them containing anything new, although of average ability; but we look in vain for any information as to the fauna or flora of the district. Classes in connection with South Kensington in Mathematics, Electricity and Magnetism, and Inorganic Chemistry, have been established, and the number of members is on the increase.

We learn from the *Melbourne Argus* that the past efforts of the Acclimatisation Society, and of private individuals working with similar objects, have been only too successful. Rabbits and sparrows are now so abundant that in many districts they are a complete nuisance, and vigorous efforts are being made to extirpate them, or at any rate to reduce their numbers. Hares are so numerous in the neighbourhoods of Melbourne and Geelong that it is proposed to modify the restrictions hitherto imposed upon their destruction, and to allow clubs, upon payment of a moderate licence fee, to course them.

The account which has been published of the terrible ravages caused by the plague in Buenos Ayres, reads like so many pages from the description of the Great Plague in London. During the months of March and April last the city was almost entirely deserted, everyone who could fleeing into the country. The deaths increased from the daily average of 120 in January to 640 on the 4th of April and 720 on the 5th, whilst on the 6th of April 500 entries at the cemetery were registered up to noon. From this time, owing to the exodus of people, the ravages of the plague began to diminish, and there is every reason now to hope that it may soon be stamped out. In one cemetery alone 20,000 corpses were buried, and for this purpose large trenches were dug, in which the bodies, some coffined, but many merely swathed in their bed clothes, were shot out of carts and quickly covered with lime. Attempts of all sorts were made to stay the plague, but unavailingly, and whilst the native doctors fled the spot, to the credit of the few English medical men there, it is universally allowed that they worked most nobly and disinterestedly through all the terrible time. We read of "coffins being hawked about the streets, while empty carts touted for their silent passengers; of people stricken with fever deserted by their friends and relations and even their children, and left to die without medical attendance or even food and water; of the shrieks and cries of delirious patients that made night hideous; and of the corpses that were constantly found by passers-by in the early morning of people who had been seized with the death agony in the streets during the night time." The cause of all this horror and misery is described as purely local, and due to the total absence of drainage and the terrible overcrowding of the houses and localities where the poor reside, and the long continued neglect of the most ordinary sanitary precautions. Surely this is a terrible lesson to those who wilfully and criminally neglect the reiterated teachings of science.

A singular instance of canine madness in a horse is recorded in a recent number of the "Zeitschrift für Parasitenkunde." A horse which had been some time before bitten by a dog supposed to be mad, was brought to the hospital of the Royal Veterinary College at Berlin, suffering from an uncontrollable propensity to bite, not only men and other animals but any hard substance, and even its own body, by which it had severely injured its mouth and broken several of its teeth. After its admission to the hospital, this propensity was violently manifested in fits, preceded by remarkable convulsive movements, after which it would fall suddenly, and remain for a time perfectly motionless, becoming gradually weaker after each attack. It had refused food for two days, and died without a struggle on the evening of the day on which it was admitted. An examination showed no organic disease, but considerable internal inflammation.

We have received from Prof. Hinrichs, of the Iowa State University, U.S., the first two numbers of the *School Laboratory of Physical Science* edited by him. The object is to supply a defect stated in the prospectus to be as flagrant in America as it is in England, that their schools, while very excellent in regard to the literary branches, neglect nearly all departments of science. The numbers which have already reached us contain original articles on Physical Science, laboratory notes and news, chronicles of observations, and reviews of books. They are illustrated by lithographs, and published at a low figure. We commend the publication to all those interested in the progress of Physical Science in America, and anxious to further the same. We may add that the publication is maintained at a considerable loss to the editor, and it is doubtful whether it can be carried on unless it receives the extraneous support which it so well deserves.

A terrible and most disastrous tornado is reported from Dayton, Ohio, U.S., on the 9th of July, by which eight people were suddenly killed and more than fifty seriously injured. The damage done to property was immense, hundreds of houses and churches were unroofed, bridges were carried away, trees were lifted up by their roots, and locomotion of all kinds was stopped, and in the country very large quantities of wheat and grain were completely ruined.

Since we noticed the appearance of the first part of Messrs. Sharpe and Dresser's "History of the Birds of Europe" (Nature, April 27), three more parts have appeared, each containing eight or nine beautiful plates, and the usual copious letterpress. Among the former we may notice those of the pigmy owl (*Glaucidium passerinum*), the white-tailed lapwing (*Chettusia leucura*), the great black woodpecker (*Dryocopus martius*), and the red-backed shrike (*Lanius collurio*), as being especially admirable pictures of bird life; while the fact that twelve pages of letterpress are devoted to the bearded reedling (*Calamophilus biarmicus*), fourteen to the great black woodpecker, and the same to the hobby and eider duck, will give some notion of the labour and research devoted to bringing together all the reliable evidence on the habits, distribution, structure, and affinities of the several species. Instead of making the pictures everything, as has sometimes been done in illustrated works on natural history, we have here really a "history" of all the more important known facts relating to each European bird. We sincerely hope that a work which the authors evidently spare no pains to make as good as possible, may meet with the liberal support it deserves.

We understand that the eruption of Mount Vesuvius, which has been more or less continuous during the past six months, and which has lately increased considerably in violence, is causing great apprehension as to the safety of the Italian observatory of Vesuvius. The lava has already partially submerged the hill of the Canteroni on which the observatory stands, and the immediate erection of a strong dyke of scoria so as to divert the stream of lava is urgently asked for.

We are requested to state that the terrible earthquake at Bathang in China, of which we lately published an account (Nature, vol. iv. p. 45), occurred on April 11, 1870, and not this year, as might be inferred from the description.

The American Polar Expedition in the steamer *Polaris* (Capt. Hall) left Brooklyn on June 29th. Dr. E. Bessels, of Heidelberg, who was Scientific Director of the German Expedition to Nova Zembla in 1869, is appointed to the same position on this expedition. The vessel is provisioned and equipped for two-and-a-half years' absence, but the explorations may be continued longer if Capt. Hall desires it, and fresh supplies will be sent. The expedition is undertaken principally for geographical discovery, but every opportunity will be made use of to make scientific observations and experiments, for which purpose a long series of instructions have been drawn up by a committee of members of the Academy. These consist of Prof. Henry on meteorology; Prof. Newcomb on astronomy; Prof. Higlard on magnetism; Prof. Baird on natural history; Prof. Meek on geology; Prof. Agassiz on glaciers. Orders have been given that small copper cylinders containing letters, scientific intelligence, &c., shall be frequently thrown overboard during the progress of the expedition, and these, when found, are to be sent to the Navy Office and afterwards published.

A most important discovery is announced from the Isthmus of Panama. In the district between Aspinwall and Panama, and extending over a large area, valuable beds of coal have been discovered and recently fully explored. The quality of the coal has been tested and most favourably reported on. These mines can be worked to great advantage, and the seams are rich and extensive, and there is ample water communication to the coast by means of the river Indis. If further investigations confirm this preliminary report, great benefit cannot fail to result to commerce in the Atlantic and Pacific Oceans from this opportune discovery.

## THE GUN-COTTON EXPLOSION AT STOWMARKET

The disastrous explosion of gun-cotton, which occurred on Friday last on the premises of the Patent Safety Gun-cotton Company, is a calamity of unusual significance. Besides the large number of killed, amounting, we believe, to five-and-twenty persons in all, there were as many as seventy maimed and injured, many of them too, in such a manner as only violent explosions are known to torture and lacerate their victims; and when it is taken into consideration that in all probability a dozen tons of the material actually exploded, the grave nature of the accident is in truth not surprising. The whole group of factory-buildings and out-houses were levelled to the earth at one fell swoop, and for miles away the effect of the catastrophe was acutely felt.

But it is not only from a social point of view that the affair is to be deplored. As a result seriously affecting the science of explosives, the occurrence is peculiarly unfortunate; for the belief in the safety of gun-cotton as an industrial and military agent will now be gravely shaken. It is all very well for scientific men to adduce a plausible reason for the occurrence, and to prove conclusively that with due care and precaution a disaster of this nature could not possibly have happened; but the public unfortunately will not be satisfied with a theoretical assurance of this kind; and indeed measures should certainly be taken, not only to guard against such wholesale death and destruction, but to render the same absolutely impossible.

The true cause of the disaster we can scarcely hope to discover; but, leaving out of consideration any personal carelessness on the part of the workmen, the ignition of the cotton must either have occurred through the accidental firing of a cartridge, or primer, or through spontaneous combustion. It is well known that pyroxilin may be exploded in two totally distinct ways—that is to say, either by inflammation or detonation. In the first instance the cotton, unless confined, only burns fiercely, and does not explode like gunpowder on the instant; while, on the other hand, if it is ignited by detonation or percussion, the material acts in the same violent manner as nitro-glycerine or fulminate powder. Is the catastrophe at Stowmarket, then, the result of detonation, or of the milder form of explosion, such as inflamed gun-cotton confined in lightly-built magazines would produce? If sporting cartridges, such as contain a small charge of fulminate or detonating primers, were at all near the spot, the culpability of the authorities is very great indeed; for the approximation of the two agents constitutes obviously a source of extreme danger, and it is really hard to believe that so thoughtless a proceeding could have been possible. At the same time, if a detonation actually did take place, as in fact some of the results would lead us to believe, then there is no other way of explaining the occurrence.

In regard to the theory of spontaneous combustion, we must not be too eager to draw conclusions, as the careful experiments recently made by Prof. Abel distinctly prove that decomposition in this wise is almost impossible, provided the pyroxilin has been carefully manufactured. Truly, if such has not been the case, and there existed impurities or imperfectly converted masses in the store of gun-cotton at Stowmarket, then a valid reason for the explosion is no doubt at hand. Still it must be remembered that pyroxilin only takes fire at a high temperature (300° or 350° F.), and therefore we must suppose that not only was the recent hot sun allowed to shine uninterruptedly upon the magazines, but that the latter were, moreover, very badly ventilated, and altogether ill cared for. Again, to have produced such wide-spread devastation, the stores or outbuildings containing the cotton must have been somewhat strongly and firmly built, otherwise there would have been no resistance to the burning mass, and consequently no violent explosion, for it must be borne in mind that the more completely the charge is confined, the more energetic will be the result.

Under any circumstances, then, we must come to the conclusion that either the gun-cotton was strongly confined in cases or magazines and simply inflamed, or that the material was detonated by a charge of fulminate powder; and in whichever way the accident happened, the same was in great measure due to neglect and carelessness. Why, indeed, such a large store of dry gun-cotton should have been kept so near a populous factory it is hard to understand; and inasmuch as the compound is always prepared in a wet, and, consequently, harmless condition, it would appear that the desiccation of the mass is afterwards carried on in close proximity to the less dangerous departments of the works. It is truly lamentable that, after the prolonged researches of Schönbein, Abel, Brown, and others, the information and particulars brought to light should not have been more appreciated and made use of by those so directly interested in the matter; for one cannot help thinking that if the business of the Stowmarket Company had been carried on under competent scientific supervision, we should not now have to lament so deplorable an accident.

While then we must all deeply regret this sad occurrence it is to be hoped that the favourable judgment passed upon gun-cotton by scientific men during the last ten years will not be completely ignored; but that, on the contrary, a proper use may be made of the valuable information at our disposal by employing it in the framing of regulations to govern more strictly and efficiently the manufacture of explosives.

H. B. P.

## *PENDULUM AUTOGRAPHS*

### I.

PERHAPS I shall best put the reader in possession of all that I have to say, and shall best explain the nature of the accompanying figures, by giving some account of the successive steps that first led me to their discovery—a genuine discovery, so far as I was concerned, though I know there must be many to whom these curves and their mathematical properties are familiar, and who will smile at the tardy stages of experiment through which I had to pass, while they cannot refuse to congratulate me on my final success.

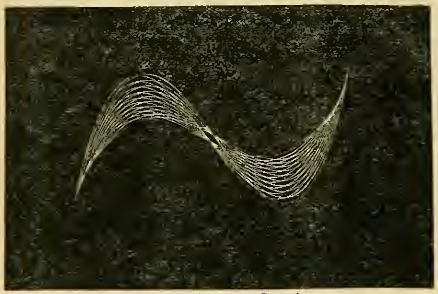

FIG. 1.—Proportion 1 : 3.—Cusped type.

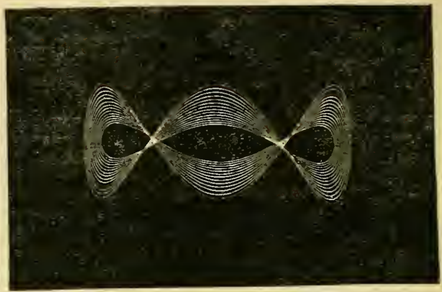

FIG. 2.—Proportion 1 : 3.—Looped type.

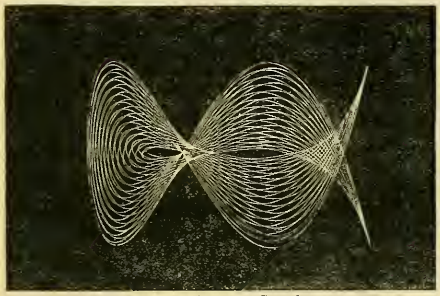

FIG. 3.—Proportion 2 : 5.—Cusped type.

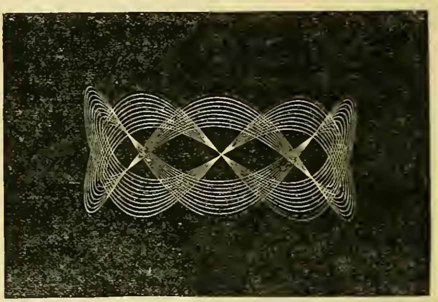

FIG. 4.—Proportion 2 : 5.—Looped type.

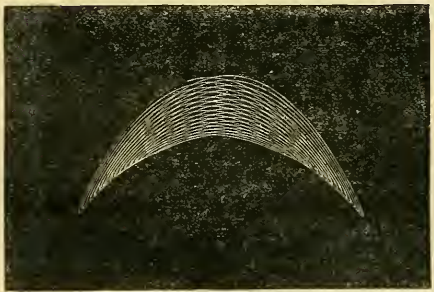

FIG. 5.—Proportion 1 : 2.—Cusped type.

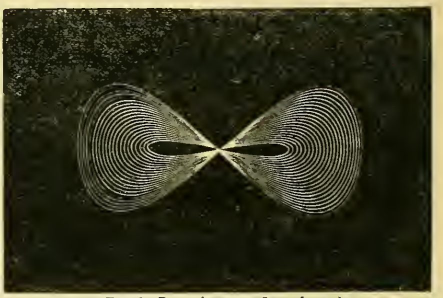

FIG. 6.—Proportion 1 : 2.—Looped type.*

It was a happy chance that directed my fingers, in an idle mood, one day in March of last year, to the top of a stiff twig that sprang from the stool of an old acacia, and rose to a height of about three feet, where it had been lopped by the gardener's knife. Pulling the twig aside, and letting it fly back by its own elasticity, I noticed the path which its top traced in the air; it was not difficult to follow its course, for the raw section of the wood was white and caught the eye, and the motion was not very rapid, the twig being rather slender for its height. I had often noticed—everyone must have noticed—odd behaviour in springs of various kinds before this, but the motion had been too quick or too slow to show the law that governed it. On the present occasion I could see that the twig began at once to deviate from the plane of its first vibration, and to describe an elliptic path, the ellipse growing wider and shorter till it was nearly circular, then still wider and still shorter, till its width exceeded its length, and it was again elliptic, but the long axis now occupied nearly the position of what was the short axis before. The new ellipse still grew narrower at every vibration, and

* Figs. 7—12 will be found in the second part of this article.

at last became a straight line in a second plane at right angles (roughly speaking) to the first. The vibration continuing, the twig began to retrace its path, and returned to the plane in which it started, by a complete recantation of its former errors, though the gradually failing strength of its oscillation was gradually diminishing the range of its orbit. No sooner was the original primary plane regained, than it was again forsaken for the secondary, the errant twig repeating its delirious maze of elliptic gyration, but always with a method in its madness, across and across, again and again, till it finally came to rest in the centre of its web, still striving to the very last perceptible tremor to persevere in its life-long career of consistent vacillation.

Repeating the experiment again and again, I found that there were two planes, at right angles, in either of which the twig would vibrate obediently, without deviation to one side or the other, and that the primary and secondary planes of the first experiment made equal angles with either plane of obedient vibration. When the twig was started only a few degrees on one side of either plane of obedience, its elliptic error carried it into a secondary plane only a few degrees on the other side, and then back again and again; while if the primary plane was chosen half-way between those planes of obedience, in opposite quadrants, then the secondary plane was found to lie half way in the alternate quadrants, at right angles to the primary.

How to explain this phenomenon was a puzzle, till my father hinted that its law might lie in a difference of periods of oscillation in those two planes of obedience, caused probably by the curved shape of the twig or perhaps by its elliptic section, at any rate caused by some condition which made the twig vibrate as a short spring with short period in one direction, and as a long spring with long period in another direction at right angles to the first.

This hint gave the key to the puzzle, and it was easy to demonstrate that all the phenomena would necessarily follow on such an assumption. Laying down two systems of rectangular co-ordinates to represent the spaces described in so many units of time (the motion of the twig being resolved in those two directions at right angles), and making $n$ such spaces in one direction and $n + 1$ in the other, we had a diagram on which we could trace the twig's path, beginning at one corner and drawing the diagonals in the successive rectangular spaces. If there were $n$ such spaces in both directions (which would represent equal periods of oscillation), our course of diagonals would only carry us into the opposite corner, with no alternative but to retrace the same line to and fro without deviation; but since in one direction there remains one space over when we reach the border of our diagram, our course of diagonals carries us across the corner, and our path returns with the width of one space between it and its former self; in like manner, on reaching the border of the diagram near the starting-corner, the course of the diagonals carries us across to the other side of our first track, and we make a second journey only to wander still farther from our first path in the return. The error increases at every turn, till at last the path of our imaginary twig finds itself wholly forgetful of the corners with which its shuttle-play began, and giving all its allegiance to the alternate pair. At last our diagonals are all described, and we find that they end in one corner or the other according as $n$ is even or odd, and the twig must then be supposed to retrace its maze. If we make our spaces all equal, the track of our twig looks very angular, like the path of a cracker; but if we endeavour to imitate the truth by greatly diminishing the marginal spaces, our diagonal track becomes bent into a series of quasi-elliptic curves, which represent with tolerable accuracy the path of our twig, if we suppose it to vibrate without frictional retardation (see Fig. 13).

We shall get the due diminution of the marginal spaces by drawing our two sets of parallel lines through two sets of points in the circumference of a circle, equidistant for each set, but allowing only $n$ equal spaces in the semicircumference for the $n$ period, and $n + 1$ for the other.

Introducing friction, we have a gradual diminution of the orbit, which brings our twig finally to rest in the centre of the diagram. But this friction has greater effect in the direction of shorter period, because our twig has to make $n + 1$ journeys in that direction to $n$ in the other, consequently the range of the orbit in the former direction will undergo more rapid contraction than in the latter, and the twig will sooner come to rest in the one plane than in the other; so that if there is large disproportion between $n$ and $n + 1$, there will remain a residue of surplus vibration in the direction corresponding to the long period after all motion in the cross-plane has been arrested. This is easily seen by experiment on a twig that vibrates much more rapidly in one direction than in the other.

Having a desire to get a permanent record of the fleeting footsteps of my acacia twig, I forced the butt-end of a small dance-pencil into the soft pith in the centre of the top-section, and set the twig vibrating with one hand, while with the other I held a sheet of note-paper in contact with the pencil-point. As might be supposed, the result was not satisfactory, but very suggestive. The twig was not strong enough to overcome the resistance of friction between pencil and paper, and the hand-suspension for the latter was very inefficient. I soon found an upright hazel-stem nearly an inch in diameter, possessing all the vibratile properties of my slender acacia-twig with much

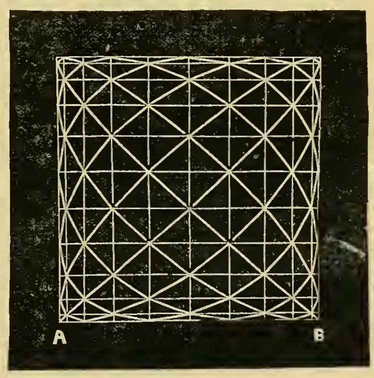


FIG. 13.—Diagram showing approximately the theoretical path of a spring vibrating without friction, with periods of vibration in cross-planes in the proportion of $n$ to $n + 1$. ($n = 10$.) A and B are the beginning and end of the cycle, perpetually retraced, and are analogous to the two cusps of Fig. 9 or Fig. 11.

greater strength, and transferred my pencil to its new abode. For suspension of paper I erected a wigwam of four poles round the hazel, and stretched a quarto leaf by india-rubber bands from the four poles to the four corners close above the pencil. Then pulling the hazel aside, I adjusted the paper-suspension till I was sure of good contact with the pencil, and then let go:—buzz—a momentary rustle under the paper, and the thing was done; and, on loosing the elastic bands, I found the path of my pencil-point faithfully traced in delicate lines, which the eye could follow from the starting-point till lost in the mazy confusion of the centre where the manifold crossings and recrossings were inextricably entangled. By starting the hazel again and again, leaving the paper undisturbed, I procured three or four path-tracks superposed, including vibrations in the planes of obedience, by which it was easy and instructive to examine at leisure the geometrical relations of the various planes. Fig. 14 is a specimen of these twig-tracings with cross-vibrations to show the

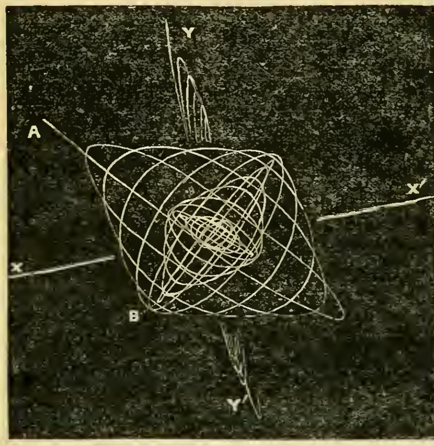


FIG. 14.—Specimen (obtained from nature) of the path of a stiff vibrating hazel shoot. Much friction. A, the starting-point. B, the end of the first cycle X X', the path of the twig set vibrating exactly in the plane of slow vibration. Y Y', nearly in the plane of quick vibration.

planes of obedience (XX', YY'.) A is the starting point, and B the point where the first retrograde step begins in the secondary plane. It will be seen on examination that from A to B the twig has accomplished exactly six quasi-elliptic journeys resolved parallel to the plane XX', and six and a half resolved parallel to YY'. So XX' is the plane of slow vibration, and YY' is the plane of quick vibration, and the periods of vibration in those two planes respectively are in the proportion of 13 to 12.

While considering these points, it occurred to me that similar results would be given by the oscillation of a pendulum jointed in such a manner as to swing in one plane only by one joint, and in the cross-plane only by a second joint at a different level from the first. The oscillation from the lower joint would be more rapid than that from the higher, and we should have exactly the same conditions of simultaneous motion in two planes with unequal periods as we had in the case of the acacia-twig. This was easily tried. From a cross-bar on an extempore tripod-stand I hung a rod by string-hinges, with an intermediate piece having its joint-edges at right angles, so that the rod was swinging in one plane by the joint between the cross-bar and the intermediary, and in the cross-plane by the joint between the intermediary and the rod. In any intermediate plane the rod could only swing by *both* these joints; its motion being really and veritably resolved in those two planes at right angles; with a longer period for the part resolved in the plane allowed by the upper joint than for the part resolved in the plane allowed by the lower. With the help of a weight of lead at the bottom of the rod, my make-shift pendulum gave a capital illustration of the problem, and the gravity and deliberation of its behaviour afforded better opportunity for study than was given by the more brilliant but less persistent energy of the acacia-twig.

The next step that naturally suggested itself was to obtain a permanent authentic record of the grave gyrations of my pendulum. I wanted something more permanent than pencil-marks, and more delicate than the daubs produced by a paint-brush full of colour. Clearly I wanted a pen that would deliver its ink in any direction all round universally. Such a pen I obtained by taking a small piece of glass tube four or five inches long and about a quarter of an inch in diameter, and melting one end in the flame of a Bunsen's burner, and drawing it out to a capillary tube, then breaking the point off square, and smoothing the broken edges of the pore in the flame, to run smoothly on the paper. By suction I drew up a small quantity of ink into the tube through the microscopic pore at the point, and then fastened my pen in a groove at the end of the pendulum-rod by elastic bands, so that it could be raised or lowered within short limits at pleasure. Then having adjusted the elastic suspension of the paper so that it hung evenly beneath the pen with a slight concavity to accommodate the nearly spherical "locus" of the pen-point, I drew the pendulum aside, and lowered the pen till it was on the point of touching the paper, then let the pendulum recede till the pen actually touched the paper, and then let go. It was beautiful to see the unerring certainty with which the pen-point struck its curves in obedience to the law imposed by its two-fold suspension. The very first back-stroke began the deviation from the primary plane, and every successive stroke made the ellipse wider and shorter by steps whose regularity was marvellous to watch. Slowly and surely the figure was filled up, line within line, line across line, as the ever-changing ellipse oscillated slowly from one side to the other of the plane of slow vibration.

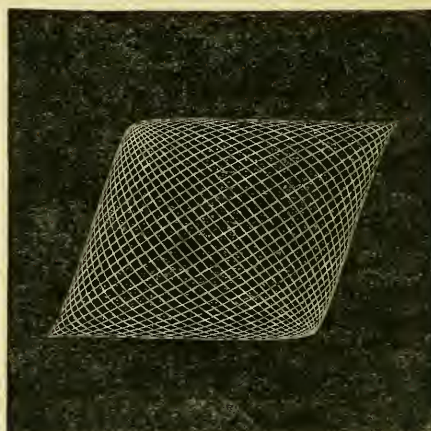

FIG. 15.—Curve traced by double jointed pendulum, with proportion near unity, about 50 : 51.

Fig. 15 illustrates this stage of experiment. Contact in this case was purposely broken at the moment when the oscillation had reached the secondary plane. The tubular glass pen did its work very well, delivering the ink with ease and regularity, and gliding almost noiselessly over the paper with very little friction. But even that small amount of friction, added to hinge-rub and air-resistance, required great weight in the pendulum to overcome it. I kept adding one lump after another till there were forty or fifty pounds of lead lashed to the rod immediately above the pen. The elastic suspension of the paper, by india-rubber bands attached to the four corners, was very serviceable, and with a little care it was not difficult to adjust the level of the paper, so that very little displacement was called for to meet the requirements of the pen's descent towards the centre of the figure, and that little was granted at once by the facility with which the india-rubber yielded to the demand. When the suspension was ill-adjusted, so that the pen-point pressed too heavily on the paper, there was a slight lateral displacement; but this danger was reduced almost to *nil* by using at each corner two elastic bands at right angles, instead of only one, ensuring resistance to any rotatory jerk in either direction.

A little more practice in the manufacture of the glass pen enabled me to improve the delicacy and uniformity of the lines. The chief difficulty lay in breaking the capillary tube square to the axis. The tube delighted in oblique fracture, which gave an oblique pore when the edges were smoothed in the flame, and the oblique pore was apt to write unequally in different directions, often refusing to write at all on one side of the figure, when the pore was not facing its work. Only now and then was the first fracture fortunately square; generally I had to pick at it with the finger-nail to reduce its obliquity. Latterly, I tried to ensure success by coating the point with a thin layer of bees' wax, tracing a fine groove in the wax round the slender tube, and allowing a drop of solution of fluoric acid to adhere round the groove until the acid had eaten into the glass and made it ready to break at the ring of corrosion. Then the remainder of the wax was washed off with turpentine, and the point was ready for its "baptism of fire." In this way I succeeded well with one or two pens, but the process was rather troublesome. That "baptism of fire" was another dangerous crisis in the early life of the pen, for the risk was great that it might be exposed to the flame just a fraction of a second too long, sealing the liquid lips for ever. A good way of testing the size of the pore—it was much too small to be examined by the naked eye—was to blow through it and notice the size of the current of air disturbing the pale blue flame of the Bunsen's burner. Alternately dipping the point for the tenth of a second in the outskirts of the furnace, and quickly withdrawing it, and trying its calibre by the breath, it was seen that the air-current grew smaller and smaller after every dip in the flame, till I dared not dip again, and then I had recourse to a powerful pocket-lens to examine the size of the pore and the smoothness of its lip. The diameter of pore of the best pen I have succeeded in making is 1-500th of an inch.

I grew discontented with common black ink for my pendulum-curves; it was apt to coagulate and choke the pore, frequently requiring the solvent power of sulphuric acid to restore free passage. Besides, I wished to have several figures superposed on the same paper, yet so that each should remain distinct. So I procured a set of coloured inks at the stationer's, price 1*d*. per bottle, and with these I was able to give additional interest to the sheets that were rapidly accumulating from all these trials of new ink, new pens, and new pendulums. For I soon grew discontented also with my first pendulum; its tripod was not strong enough, and its cord-hinges were very weak, and were fast fraying under the strain of 50 lbs. of lead in habitual oscillation below, and I feared a snap and a crash. I kept it in my bedroom, and at midnight I heard it creak, and could not rest until I had insinuated a rush-bottomed chair between the legs of the tripod, immediately below the lead, to break the fall which I fully expected. However, nothing happened, and in the morning I changed the frayed string for a trustworthy cord, and slept securely next night. I also made a new tripod with the aid of three surveying-poles, and improved the attachment of the pen by making it slide in a hole bored in the end of the rod, with a lateral screw to fix it at any required height.

HUBERT AIRY

(*To be continued.*)

## THE BRITISH ASSOCIATION MEETING AT EDINBURGH

EDINBURGH, *Friday morning*

THE work—and play—of the Edinburgh meeting of the British Association is now over; the visitors have all left, except such as have remained to do honour to the memory of the great Magician of the North; indeed, for the last two days the Southerners have divided their allegiance between the British Association and the Scott Centenary. Instead of Spontaneous Generation and the Germ Theory of Disease, the Solar Eclipse and the great Dredging Expedition, we have heard quite as much of Abbotsford and Dryburgh, Jock o' Hazeldean and the Laird of Dumbiedikes.

As announced in our letter of last week, the concluding meeting of the General Committee was held in the University on Wednesday at one o'clock, Sir William Thomson in the chair. Dr. Hirst read certain regulations which were proposed by the Committee on Recommendations to be adopted relative to the proceedings of the sections. They had reference to the organisation and constitution of the Sectional Committees, but were merely in regard to matters of detail. In an appended circular, authors of papers were reminded that, under an arrangement dating from 1871, the acceptance of memoirs, and the days on which they were to be read, were now as far as possible determined by organising committees for the several sections before the beginning of the meeting. It had therefore become necessary that an author should prepare an abstract of his memoir, of a length suitable for insertion in the published transactions of the Association, and that he should send it, together with the original memoir, to the general secretaries in London a certain time before the meeting. If it should be inconvenient to the author that his paper should be read on any particular day, he was requested to send information thereof to the secretaries in a separate note. These resolutions, after some discussion, were adopted. The next subject referred to the General Committee on Recommendations had reference to Dr. King's proposal that there should be a subsection of Anthropology. Before the consideration of that suggestion was concluded, another came from Section D of a more definite nature; and, on considering both propositions together, the Committee on Recommendations decided that they could not recommend the adoption of Dr. King's motion, but that they could strongly recommend the adoption of the other. They therefore recommended—"That in future the division of the Section of Biology into the three departments of Anatomy and Physiology, Anthropology, and Zoology and Botany should be recognised in the programme of the Association meetings; and that the president, two vice-presidents, and at least three secretaries shall be appointed; and that the vice-presidents and secretaries, who shall take charge of the organisation of the several departments, should be designated respectively before the publication of each programme." That would virtually amount to the direct recognition of the three departments of Section D. Logically, it would be impossible to take any of these departments from Biology to make a separate section of it; but they were recognised distinctly, and the gentlemen who would preside over these departments would be stated by name. The recommendation was agreed to.

The following recommendations were then read and unanimously adopted:—

"That the President and Council of the British Association be authorised to co-operate with the President and Council of the Royal Society, in whatever manner may seem to them to be best, for the promotion of the circumnavigation expedition specially fitted out for carrying the physical and biological Exploration of the Deep-sea into all the great oceanic centres.

"That the President and general officers, with power to add to their number be requested to take such steps as may seem to them desirable in order to promote observations on the forthcoming Solar Eclipse.

"That the Council be requested to take into consideration the desirability of the publication of the periodic records of the advances made in the various branches of science represented by the British Association.

"That it is desirable that the British Association apply to the Treasury for funds to enable the Tidal Committee to continue their calculations; and that it is desirable that the British Association should urge upon the Government of India the importance for navigation and other practical purposes, and for science, of making accurate and continued observations on the tides at several points on the coast of India.

"That the Council of the Association be requested to take such steps as to them may seem most expedient in relation to the proposal of Dr. Buys Ballot to establish a telegraphic meteorological station at the Azores.

"That the Council be requested to take such steps as they deem wisest in order to promote the introduction of scientific instruction into the elementary schools throughout the kingdom; and, secondly, that the Council of the Association be requested to take such steps as may appear to them desirable with reference to the arrangement now in contemplation to establish leaving examinations, and to report to the Association on the present position of science teaching in the public and first grade schools."

Dr. Thomson read the report on the resolutions involving applications for grants of money, which were as follow:—

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|---|---|
| KEW OBSERVATORY. | |
| The Council—Maintaining the establishment of Kew Observatory | £300 |
| MATHEMATICS AND PHYSICS. | |
| Cayley, Professor—Mathematical Tables | 50 |
| Crossley, Mr.—Discussion of Observations of Lunar Objects | 20 |
| Tait, Prof.—Thermal Conductivity of Metals | 25 |
| Thomson, Prof. Sir W.—Tidal Observations | 200 |
| Brooke, Mr.—British Rainfall | 100 |
| Thomson, Prof. Sir W.—Underground Temperature | 100 |
| Glaisher, Mr.—Luminous Meteors | 20 |
| Huggins, Dr.—Tables of Inverse Wave Lengths | 20 |
| CHEMISTRY. | |
| Williamson, Prof.—Reports of the Progress of Chemistry | 100 |
| Williamson, Prof.—Testing Siemens' new Pyrometer | 30 |
| Gladstone, Dr.—Chemical Constitution and Optical Properties of Essential Oils | 40 |
| Brown, Dr. Crum—Thermal Equivalent of the Oxides of Chlorine | 15 |
| GEOLOGY | |
| Duncan, Dr.—Fossil Crustacea | 25 |
| Lyell, Sir C., Bart—Kent's Cavern Exploration | 100 |
| Harkness, Prof.—Investigation of Fossil Corals | 25 |
| Busk, Mr.—Fossil Elephants of Malta (renewed) | 25 |
| Harkness, Prof.—Collection of Fossils in the North-west of Scotland | 10 |
| Ramsay, Prof.—Mapping Positions of Erratic Blocks and Boulders | 10 |
| BIOLOGY | |
| Stainton, Mr.—Record of the Progress of Zoology | 100 |
| Balfour, Prof.—Effect of the Denudation of Timber on the Rainfall in North Britain (renewed) | 20 |
| Sharpey, Dr.—Physiological Action of Organic Compounds | 25 |
| Foster, Prof. M.—Terato-Embryological Inquiries | 20 |
| Foster, Prof. M.—Heat Generated in the Arterialisation of the Blood (part renewed) | 15 |
| Christison, Prof.—Antagonism of Poisonous Substances | 20 |
| GEOGRAPHY | |
| Murchison, Sir R.—Exploration of the Country of Moab | 100 |
| ECONOMIC SCIENCE AND STATISTICS | |
| Bowring, Sir J.—Metric Committee | 75 |
| MECHANICS | |
| Rankine, Prof.—Experiments on Fletcher's Rhysimeter | 30 |
| | £1,620 |

The whole of the proposed grants of money were approved of, the wording of the last being modified as follows: "Experiments to measure the speed of ships and currents by means of the difference in heights of two columns of liquid."

Dr. Thomson read a number of recommendations adopted by the Committee on Recommendations not involving grants of money, which were also approved of.

At the concluding meeting of the Association, held in the Music Hall at half-past two o'clock, Sir William Thomson in the chair, Dr. Thomson read the recommendations for grants of money and also the recommendations not involving money grants, which had been adopted by the General Committee. Mr. Griffiths stated that the number of tickets issued for this meeting had been as follows: Old life members, 246; new life members, 28; old annual members, 311; new annual members, 127; associates, 976; ladies (transferable tickets), 754; foreign members, 21—total, 2,463. The money received for these tickets was 2,575*l.*

It will be seen that the recommendations were almost more important than in any previous year. The last in particular, relative to the introduction of scientific instruction into the elementary schools of the country, covers a wider ground than is often included in the action of the Association. Wisely carried out, this recommendation may be pregnant of the most important results in the future; and serves to show that at least our leading scientific men are alive to the need there is for a strenuous effort to place the education of the country on a level with the requirements of the times. The scheme to which we referred in our leader last week, relative to the extension and improvement of the present system of giving scientific lectures to the people, was warmly taken up, and a committee appointed to carry it out. The application to the Government asking for 2,000*l.* in aid of the observation of the Total Eclipse in December next was sent off the same day.

Among the more important papers read during the present week were two on Tuesday in Section A, which it was agreed should be taken together: *On Government Action on Scientific Questions*, by Col. A. Strange, F.R.S.; and *On Obstacles to Teaching Science in Schools*, by the Rev. W. Tuckwell. In both these papers, of which we shall give full reports, very important issues were raised. The discussion on them was a highly interesting one, and was led by Prof. Tait, who said there existed an absolute necessity for a State system of instruction in Science; and was carried on by the Rev. T. G. Bonney, Mr. G. J. Stoney, Mr. James M. Wilson of Rugby, Mr. Pengelly, Mr. Boyd Dawkins, and others. An entire unanimity was displayed as to the pressing importance of both the subjects introduced. On Wednesday, Section D was enlivened by another Spontaneous Generation controversy, introduced by Dr. H. C. Bastian, who was supported by Dr. Burdon Sanderson, to the extent that we have at present no evidence that fungus or other germs are contained in the air in a vital condition. In closing the discussion, the President of the Section said that the subject was still one which must be considered as undecided. The proposal to enter into the discussion of Mr. Crookes's "Psychic Force," and the whole phenomena of so-called "Spiritualism," was rejected for want of time.

The excursions, which were arranged for yesterday, were uniformly well carried out and successful. About eighty ladies and gentlemen paid a visit to Hopetoun House and Dalmeny Castle. Forty ladies and gentlemen availed themselves of the trip to Rosslyn and Penicuik, and over 300 visited Melrose, Dryburgh, and Abbotsford.

The excursion of the Geologists was to Siccar Point and Fast Castle, under the leadership of Prof. Geikie. The object of the excursion was to visit the coast-line of Berwickshire, and examine the natural sections there, which have become classic in geology through the writings of

Hutton, Playfair, and Hall. The chief features of geological interest examined were:—First, the manner in which, at Siccar Point, the vertical and highly-inclined Lower Silurian strata are covered unconformably by the gently-inclined Upper Old Red sandstone. It was this section which furnished Hutton with one of the most telling arguments for his "Theory of the Earth," and his search and discovery of which have been so graphically described by Playfair. The sections presented indeed a magnificent example of uncomformable stratification; the Old Red strata lying almost horizontally on the vertical Silurians, a phenomenon expressed by the local papers by the phrase, not devoid of a certain dry humour, that "the Old Red rested uncomfortably on the Silurian!" We are bound however to state that on the whole the Edinburgh press was well up to the occasion; the Reports of the Addresses, Lectures, and Sectional Proceedings were good and full, and no pains were spared to make them really first-rate. The reports in the *Scotsman* should be mentioned in particular as unusually excellent.

The second point examined was the plication of the Lower Silurian rocks. Along this wild coast-line the greywacke and shale are thrown into many anticlinal and synclinal curves, extending from top to bottom of the cliffs, which are here in some places more than 500 feet high. Along the part of the coast to be examined by the excursionists the best folds occur at Fast Castle. It was the curving of these rocks which attracted the attention of Sir James Hall, and led him to investigate the subject in his well-known paper on "The Vertical Position and Convolutions of certain Strata," some of the illustrations from which may now be found copied into almost every text-book of geology.

A party of naturalists, numbering about sixty, joined in the Dredging Expedition off the Bass Rock. Amongst the gentlemen present were:—Prof. Wyville Thomson, Admiral Sir Edward Belcher, Sir Walter Elliott, Prof. Crum Brown, Prof. Margo, of Pesth; Prof. Purser, of Belfast; Dr Colding, of Copenhagen; Dr. Lütken; Dr. Copeland, of Parsonstown, Ireland; Dr. Lindeman, of Bremen; Mr. Shapter, Mr. G Barclay, Mr. Ray Lankester, Mr. Shepherd, and Mr. Davis. There were also a number of ladies in the party.

About a hundred members and associates of the Association took part in a botanical excursion to the top of Ben Ledi, under the leadership of the veteran Prof. Balfour

The Conversazione held on Tuesday evening was a very good one; over 1,400 ladies and gentlemen attended. In addition to the varied contents of the Museum of Science and Art where the Conversazione was held, and which are themselves of no ordinary interest, the following were the most interesting objects exhibited:—Mr. Fowler's flint implements of the drift, Spencer's local heliostat, Dr. Gladstone's experiments in the crystallisation of metals by electricity under the microscope; flint implements from Palestine.

It only remains to be added, that, thanks to the admirable arrangements of the energetic local secretaries, Dr. Crum Brown and Mr. Rollo, everything went off well during the meeting; and the third Edinburgh meeting of the British Association will be looked back upon as one of the most enjoyable of a long series, as it certainly has been the most important for many years.

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## SECTION A.

THE greater part of the first day's session in this Section was occupied by a paper *On the Thermodynamics of the General Oceanic Circulation*, by Dr. W. B. Carpenter, and the interesting discussion which followed.

The inquiries in which the author, with his colleague, Prof. Wyville Thomson, has recently been engaged, into the physical condition of the deep sea, have furnished a new set of facts in regard to its thermal condition, which seem to point to conclusions very different from the doctrines usually received in regard to the movements of oceanic waters and their influence on climate. It may now be asserted as probable that the temperature of the bed of the ocean below 2,000 fathoms is everywhere, even under the equator, but little above 32° F., while it may be as low as 29·5° F. in particular channels of less depth, such as that which lies between the Shetland and the Faroe Islands. That this depression of temperature has no dependence on depth *per se*, appears to be conclusively proved by the fact that it does not show itself in the Mediterranean, for though depths of 1,600 fathoms have been sounded in its western basin, and 2,000 in its eastern basin, the temperature below the surface stratum of about fifty fathoms, heated by direct solar radiation, remains at a uniform level of about 54° to the very bottom, being in fact the average winter temperature of this vast mass of water, which may be regarded, as to all but its surface, in the light of an inland lake.

Now, if the condition of the Mediterranean be compared with that of the eastern border of the Atlantic under the same parallels, we find a most striking contrast in their thermal conditions. The superheating of the surface-stratum by direct solar radiation shows itself in the latter as in the former; below the surface-stratum there is a very gradual descent of the thermometer from about 53° to 49°, which last is the temperature at 800 fathoms; in the 200 fathoms below this there is a rapid loss of 9°, bringing the thermometer down to 40° at 1,000 fathoms; whilst beneath that line there is a further gradual descent with increase of depth, 36°·5 being the lowest temperature yet observed in this region. The author regarded this anomaly as due to the fact that the former was virtually cut off from the great oceanic circulation that diffuses over the latter the waters that have been chilled in the polar seas. The author found that the *primum mobile* of this circulation was not in equatorial heat (which being applied to the surface could exert no motor force beneath the thin stratum which it directly affects), but in polar cold, which by its action on the surface-water would produce the same kind of movement from above downwards, as heat applied at the bottom does from below upwards.

Supposing the whole surface of a limited basin of sea-water to be exposed to intense cold, the surface film, when rendered heavier by reduction of temperature, will sink, to be replaced from the warmer stratum beneath. The new surface-stratum will then be cooled; and the same process would be repeated until the temperature of the whole basin comes to be reduced as low as the cooling action will carry it—it may be down to 27° or even 25°. But suppose that only a portion of the surface area of the basin be exposed to cold, the phenomena would be different. (1) As each surface-film cools and sinks, its place will be supplied, not from below, but by a surface influx of the water around; and (2) the bottom stratum will flow away over the deepest parts of the basin, while, since the total heat of the liquid is kept up, there will be an upper stratum which will be drawn towards the cold area, to be precipitated to the bottom and repeat the action. Applying this principle to the great oceanic area that stretches between the Equator and the Poles, we should expect to find the upper stratum moving from the Equator towards the Poles, and its lower stratum from the Poles towards the Equator. That such a movement really takes place is indicated, as it seems to me, by various facts.

(1.) The general prevalence of a temperature not far above 32° over the deepest parts of the great ocean basin, which could scarcely be maintained if there were not a continual flow of cold water from the polar area.

(2.) The distinction between the upper and lower strata of Atlantic waters is shown by the change of temperature between 800 and 1000 fathoms.

(3.) The existence of a movement of warm surface-water towards both polar areas. From a consideration of these facts in detail, the author was led to the hypothesis of a north-easterly movement of a vast stratum of oceanic water, having a depth of at least 600 fathoms. In the remaining portion of his paper Dr. Carpenter discussed the different causes of horizontal and vertical currents. He was inclined to believe that the propulsive force of the trade winds produced only a horizontal motion.

Sir W. Thomson said that Dr. Carpenter's explanation had been so lucid and demonstrative that he thought little remained to be said. It seemed to him that Dr. Carpenter thoroughly established his case. The distinction drawn by him between horizontal and vertical circulation was important. When the path of least resistance was in a wide circuit along the surface, then the chief return would be along the surface. In an open ocean where there was a prevalence of winds in a certain direction over one part of it, it seemed necessary that the currents produced by that wind should be as Dr. Carpenter had maintained. The only case in which he could conceive of a return along the bottom produced by wind was one of great interest, but in which the circumstances were precisely opposite to those of an open oceanic circulation, viz. in the case of a frith or fiord. The elevations of 6, 8, or 10 feet, which we know result from high wind, must be thus explained; the return circulation cannot but be along the bottom. In the case of a frith, if the whole surface is carried up in a current, the water must get away somewhere. If there were a strong breeze in narrow waters when the whole surface was broken up, there would be a great deal of surface drift; but even without breaking up a surface there was a current necessarily accompanying waves at sea when the height of the wave was not infinitesimal, *i.e.*, when it was not very small in comparison with the length; sometimes there was a great surface current amounting to three or four knots in these circumstances, and there must be an equal outflow at the bottom. Dr. Carpenter's explanation of the vertical circulation seemed to make the whole thing perfectly clear. Ocean currents were altogether unknown, with the exception of a few isolated cases, and even regarding these the knowledge was not nearly practical enough for the ordinary purposes of navigation. In the operations of 1866 to recover the cable of 1865, it was discovered that the success or non success altogether depended on the management of the ocean current. Captain Moriarty, who was chiefly concerned in finding the ship's place, came to the conclusion that the whole subject of ocean currents ought to be made a matter of hydrography, and certainly it was an object of all others appropriate to a nautical country. The question of temperature was also of great practical importance, as the temperature of the sea bottom along which the cable was to be laid was of enormous importance to the enterprise. If a cable showed certain signs at 49° F. it was good, if it showed the same signs at 40° F. it was bad. Another most serious practical want was to know precisely the temperature of the cable when laid, in order that if there was a fault its temperature might be accurately determined.

Prof. Stokes said that if he had risen first, he would have pointed out what had been so well stated by Sir W. Thomson, that the only case in which a vertical circulation could be produced in the horizontal blow forward by wind, was in the case of a narrow channel. If a portion of a widely-extended ocean were blown on by the wind, the water would be propelled forward, but the tendency would be to take it in from all directions, not merely from one, so that the inflow would be lost in minuteness. That a surface-current is a necessary accompaniment of waves, seemed pretty obvious. If waves are already in existence on the surface of water, it is evident that their backs must be more strongly acted on than their fronts by the wind; there must be a horizontal resultant forward which must push on the water somehow or other; the fact of the existence of these waves implied that there was already a surface current of a certain amount.

Mr. Robert Russell said he could only go a certain distance with Dr. Carpenter; he considered the effect of polar cold and equatorial heat to be comparatively small compared with the wind. The Atlantic itself narrows so much towards the North Pole, that its vast surface is forced by the south-west wind to the northern ocean, and is forced into it in spite of polar cold.

Prof. G. C. Foster said that a possible cause of the formation of currents was the coexistence of different specific gravities in neighbouring quantities of water.

Dr. Carpenter said it gave him great satisfaction to hear the general agreement of Sir W. Thomson with the views that he had advanced; he had expressly spoken of the open ocean, and mentioned as excepted such cases as the Gibraltar current. With regard to cables, Capt. Sherard Osborne had mentioned to him that the cable recently laid down in the Eastern seas towards China was generally in shallow water and therefore warm, so as to diminish the conducting power of the wire, but at one point it dipped down into a hole, and there the temperature having fallen the conducting power was greatly improved. Everyone knew that when the cable was cut and buoyed in 1865, there was a long wire rope with a buoy attached to it. It got adrift, and was seen by an Atlantic mail steamer 10° to the southward. One would have expected it to the North-East, through the influence of the Gulf Stream. It was suggested that the long rope had broken away at the bottom; that its long tail was hanging in the sea, and the action of the great lower movement to the south might have been stronger on the tail than the action of the surface water on the upper portion of the rope.

Mr. Buchan said that the Scottish Meteorological Society were conducting investigations which would settle what were the winds and currents over each degree of a portion of the Atlantic.

Mr. Scott said he hoped to give Mr. Buchan charts of the currents over the area such as were never before possessed. They were derived from all available sources of information.

Sir W. Thomson said it seemed demonstrable that in all water above five or ten fathoms deep, the current under return due to surface drift was insensibly small, and he thought that this demonstrated Dr. Carpenter's statement, that the main current could not be produced by wind, though the wind might produce very considerable surface currents.

Prof. Colding, who stated he had been working at the same subject for many years since, made some remarks on the effect of the earth's rotation on the currents, and

Prof. Tait remarked that the discussion well illustrated the use of the British Association.

*Observations Physiques en Ballon*, by M. Janssen.

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## SECTION B.

THIS section did not sit on Saturday, and on Monday the proceedings commenced with two short papers by the President, Dr. Andrews, *On the Dichroism of the Vapour of Iodine and on the Action of Heat on Bromine.* The fine purple colour of the vapour of iodine arises from its transmitting freely the red and blue rays of the spectrum, while it absorbs nearly the whole of the green rays. The transmitted light passes freely through a red copper or a blue cobalt glass. But if the iodine vapour be sufficiently dense, the whole of the red rays are absorbed, and the transmitted rays are of a pure blue colour. They are now freely transmitted as before by the cobalt glass, but will not pass through the red glass. The solution of iodine in bisulphide of carbon exhibits a similar dichroism, and according to its density appears either purple or blue when white light is transmitted through it. The alcoholic solution, on the contrary, is of a red colour, and does not exhibit any dichroism. If a fine tube be filled one half with liquid bromine and one half with vapour of bromine, and after being hermetically sealed, is gradually heated until the temperature is above the critical point, the whole of the bromine becomes quite opaque, and the tube has the aspect of being filled with a dark red and opaque resin. A measure of the change of power of transmitting light in this case may be obtained by varying the proportion of liquid and vapour in the tube. Even liquid bromine transmits much less light when heated strongly in a hermetically sealed tube than in its ordinary state. In connection with this subject, Mr. Dewar exhibited an experiment illustrating the action of light upon peroxide of chlorine.

The report on the Utilisation of Sewage was presented by Mr. Grantham. It was divided under the following heads:—(1) Experiments on Britton's Farm, Mr. Hope. (2) Comparison of results during winter of Croydon, Norwood, and Britton's Farm experiments, Dr. Corfield. (3) Report on Analysis in connection with above, Dr. Corfield. (4) Upward Filtration of Sewage at Ely, Dr. Corfield. (5) Phosphate Process, Dr. Corfield. (6) Dry-Earth System at Lancaster, Drs. Corfield and Gilbert. Dr. Bischof read a paper *On the Examination of Water for Sanitary Purposes*, in which he sought to show that the appearance of the residue obtained by evaporation when seen under the microscope afforded a ready method of detecting sewage contamination.

Dr. Otto Richter contributed a paper *On the Chemical Constitution of Glycollic Alcohol and its Heterologues as viewed in the light of the Typo-nucleus Theory.* The Abbé Moigno gave an account of the history and working of the photographic post, and exhibited a number of collodion films containing microscopic photographs of letters and despatches. Every film reproduced twelve or sixteen folio pages of printing, and contained on an average 3,000 despatches. The whole of the official and private despatches came by pigeons during the investment of Paris, numbered about 115,000, weighing in all about two grammes.

Dr. Wright gave an account of some experiments *On the Essential Oil of Orange Peel.* It has been shown that this oil consists principally of a hydrocarbon, hesperidene, $C_{10}H_{16}$, and an amorphous resin of the formula $C_{20}H_{30}O_3$. When hesperidene is boiled "per ascensum" with sulphuric acid and potassium bichromate, carbon dioxide is slowly evolved, and acetic acid produced, whence it is inferred that the structure of the hydrocarbon is $CH_3$

$CH (C_8H_{12})$."

## SECTION C.

After the reading of Prof. Geikie's Report on the Progress of the Geological Survey, Mr. James Thomson, F.G.S., read a paper *On the Age of the Stratified Rocks of Isla.* He gave an account of the general character and relations of the beds, which are much affected by intrusive igneous rocks, and illustrated the subject by two sections—one of the east coast of Isla and a transverse section of the same. His paper contained much detail, which was hardly of sufficient interest to a general audience, but it clearly showed an amount of careful investigation that will prove of great value to geologists.

In the discussion which followed, Mr. Geikie differed from the author in his identification of the Fundamental Gneiss, and he thought sufficient evidence of its presence had not been brought forward. Professor Harkness regarded the Gneiss as corresponding with the newer gneiss of the Highlands. Mr. Thomson, in reference to some remarks upon *Eozöon*, stated that having sent to Dr. Carpenter some specimens of the rock, he reported that *Eozöon* structure was not sufficiently distinct to warrant him in calling it *Eozöon.*

The *Third Report of the Committee on Earthquakes in Scotland* was communicated by Dr. Bryce, F.R.G.S., F.G.S. There was nothing, however, of importance to make known, but a few slight earthquakes having been felt—one at Lochaber and another in the upper part of the Firth of Clyde. In regard to the latter, very little information that could be depended upon had been obtained, but there was less doubt respecting the other. It occurred in a district in which some of our most severe earthquakes have taken place. However, in the absence of any recording instruments, it has been impossible to state with certainty the intensity of the shocks. The Committee recommend the adoption of a much simpler form of Seismometer than that at Comrie belonging to the Association; they also proposed placing such an instrument at a number of the meteorological stations which are within the area liable to disturbance.

Mr. Henry Woodward, F.G.S., read his *Report on the Structure and Classification of the Fossil Crustacea*, first noticing the new forms discovered and described during the past year, which amounted in all to 21 species, including 6 Decapods, 1 Amphipod, 2 Isopods, 1 Eurypterid, and 13 Phyllopods. He referred to the wide distribution of a new Cretaceous Isopod (*Palæga Carteri*) which had been found in Upper Silesia, at Turin, and in three localities in England, and pointed out that if the conclusions arrived at by Mr. Billings and himself as to the Trilobites possessing legs be established by further research, then that group would carry the Isopodous class back in time to our earliest Palæozoic rocks.

The structure of *Dictyoxylon* (*D. Grievii*), a new species of which bad been discovered by Mr. G. J. Grieve, near Burntisland, formed the subject of some remarks by Prof. W. C. Williamson. He regarded the form as of a type belonging to the Coal Measures.

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## SECTION D.

### SUB-SECTION.—ZOOLOGY AND BOTANY

The Committee for the Foundation of Zoological Sections in Different Parts of the Globe, reported that since the last meeting at Liverpool steps were taken by Dr. Dohrn to secure the moral assistance of some other scientific bodies, that the Academy of Belgium had passed a vote acknowledging the great value of the proposed Observatories. Besides, the Government at Berlin had given instruction to the German Embassy at Florence and to the General Consul at Naples for Germany to do everything to secure success to Dr. Dohrn's enterprise. Next October the building at Naples will commence, under Dr. Dohrn's personal superintendence, who will be accompanied by the assistant architect of the Berlin Aquarium. The contractor is to finish the building in one year, so that in January 1873 the Aquarium in Naples may be hoped to be in working order.

The Naples Observatory being thus arranged for, the Committee urged the importance of establishing a Zoological Station in the British Islands, and to the opportunity, which is now offered for such a proposition in consequence of the cessation of the grant to the Kew Observatory. In the same way as the Association took the initiative in the foundation of Meteorological Observatories, so may they legitimately, and with every prospect of success, take in hand the foundation of Zoological Observatories. Until a recent date the Association has given considerable sums of money to dredging explorations; but in consequence of the advance in Zoological Science the problems are so much changed, and their nature is of such a character as to demand the assistance of the Association in other directions. The careful study of the development and the habits of marine animals can only be carried on by aid of larger Aquariums and cumbrous apparatus, which an individual could hardly provide for himself. This and the copious supply of animals for observation can be provided by such a co-operative institution. There can be little doubt of the convenience to Naturalists and the benefit to science which would be brought about by the foundation of a Zoological Station in the British Isles.

The Committee recommends that a Committee of the Association be formed for the purpose of erecting a Zoological Station at a convenient place on the South Coast of England, say Torquay, and that a sufficient sum of money be placed at their disposal either by a single or a series of annual grants.

Prof. E. Perceval Wright suggested that Bantry Bay would be a good place for establishing such a station. Here scientific research could be carried on at a very trifling expense, and although no return would be obtained from visitors to the Aquarium, yet from this station other Aquaria might be supplied at a remunerative rate. Prof. Lawson remarked that such a station might be turned to good account for the investigation of the marine Flora as well as Fauna. Prof. Dunns trusted the Department would make a very hearty recommendation to the Council of the Association on this subject, in which Prof. Wyville Thomson concurred; and Dr. Sclater, who had read the Report, promised that the matter should be laid before the full Committee of the Section.

The Report was signed by Dr. A. Dohrn, Prof. Rolleston, and Dr. Sclater.

### SUB-SECTION.—ANTHROPOLOGY

Prof. W. Turner opened the Section with an address from the chair, in which he traced the rise and growth of the science of Anthropology, and the vicissitudes in the fortunes of the subsection over which he presided. Anthropology was first allowed place in the proceedings of the British Association under the head of Zoology and Botany; then it was assigned to the department of Geography, and at last, in 1865, resumed again its old place under the newly named department of Biology, which embraced not merely Zoology and Botany, but the whole science of organisation. The science of man obviously has an organic connection with Biology, and ranges itself naturally under that master science. Within its scope falls everything which has a direct bearing on man, and as nearly every branch of human knowledge has a relation, more or less, to man, questions may occasionally arise whether papers brought before the sub-section come within its province or more naturally belong to the other sections. The most satisfactory way of solving this difficulty would be for the different sections concerned to come to a common understanding, that all papers, which treat of the origin and progress of mankind, should be forwarded to the Department of Anthropology. The term Anthropologists—ἀνθρωπολογοι—was first used by Aristotle, as to denote "gossips," or talkers about men rather than facts. And if we lay claim to the title, let it not be in this sense, but in the nobler and wider sense of humble and patient students of the great science of human nature.

Dr. Beddoe then read a paper *On the Degeneration of Races in Britain*, in which he urged the necessity of systematic inquiry into the physical changes which are now taking place in our population. Of the four countries—England, Wales, Scotland, and Ireland—the first, which is the richest, and considered to be the most advanced in material civilisation, and whose habits and modes of life are more and more imitated by the others, is, according to Edward Smith's reports on the subject, the one in which the people are most scantily and ill-nourished. The scarcity of milk especially, as to its supply to children in towns and in dairy districts, is a growing evil, and one of national importance. Here may be mentioned, as having probably a relation to the quality of the food, and possibly to this very defect of milk, the apparently growing evil of unsound teeth, which, again, seems to advance *pari passu* with the advance of material civilisation, and is worst among the English and the townsmen of the United States, not so conspicuous among the Scotch, and decidedly at the minimum among the Irish. Certain changes in the process of natural selection, as it operates on our people, seem to be on the whole detrimental to the standard of physical type. Emigration drains away large numbers of the stronger and more energetic young men from the best of oue districts; so do the military and civil service in India; and thr voids are supplied to a less extent than they used to be from the rural population, wherein the rates of marriage and of birth are much less than in that of the towns. The classes that yield the largest number of births are, beginning with the least important —(1), fishermen; (2), miners, especially coal-miners, and the like; (3), the proletariat of large towns. Whatever may be said of the two former, this last and most important is, physically, about the worst developed in the kingdom. Formerly it did not tend to increase in numbers, relatively, to other classes, because the death-rate in the worst quarters of towns was so high as to balance or overbalance the birth-rate—such was the case not long ago in Liverpool, for example. But the effect of sanitary improvements has been so considerable, that the rates of sickness and death in these quarters are being decidedly ameliorated; and this improvement, regarded dispassionately, is no more an unmixed good than are good things in general; for the increase in the number of survivors brings about a disproportionate augmentation in the numbers of the class in question, and thus lowers the average standard of physical development.

## SECTION G.

### MECHANICAL SCIENCE

### Opening Address by the President, Prof. Fleeming Jenkin

In addressing you on the subject of Mechanical Science in our ancient university, I propose to speak on the somewhat threadbare topic of technical instruction. The panic with which some persons regarded the rapid improvement made abroad in manufactures has subsided, but I hope that you will be all the more ready on that account to listen to a few suggestions as to steps which may be immediately taken to improve the education of those who apply science to practical ends. The subject does not owe its prominence to any events of to-day or of yesterday; it has long been, and will long be, of paramount importance to this country that the education of the producers of wealth should be such as will enable them, not merely to compete on advantageous terms with foreigners, but rather to master the great forces of nature by which we work. That we have gained some triumphs can be no reason for relaxing our efforts. With each advance further advance becomes more difficult, and requires more knowledge. The first rude implements and processes employed by man certainly required for their explanation or acquirement no book-learning; but as processes become complex, and implements develop into machines, as the occupations of men differ more and more, practice alone is found insufficient to give skill, and study becomes the necessary preparation for all successful work. Our first engineers were not learned men; strong good sense and long practice enabled them to overcome the comparatively simple questions with which they dealt. All honour to those great men; but we who have to deal with more complex, if not with vaster problems, cannot trust to good sense alone, even if we possess it, but must arm ourselves by the study of science and its application to the arts. This being granted, how shall it be done? I need not trouble you by refuting the absurdities of a few men, who would have those things taught at schools which have hitherto been taught by practice. What has been taught by practice must still be taught by practice. The business of the school is to teach those things which practice in an art will not teach a man. Let us apply this principle to engineering —the most scientific of all professions. It will be most useless to lecture on filing and chipping; it will be useless to describe the mere forms and arrangements of vast multitudes of machines; one kind of knowledge of the properties of materials can only be acquired, as it always has been acquired, by actually handling them; and the knowledge of the arrangement of a machine is far better learnt by mere inspection than from fifty lectures; moreover, it can be acquired by an intelligent man, even if he be wholly unlettered. Book learning about estimates, the value of goods, methods of superintending work, and dealing with men, is foolishness. Written descriptions of puddling a clay embankment, excavating, and such operations, give no knowledge; and yet a vast mass of such knowledge must, at some time of his life, be acquired by the engineer, and the student cannot be employed as an engineer until he has laid up a store of such knowledge. Colleges cannot give him this; he must serve an apprenticeship in fact if not in form; young foreigners taught in colleges serve their apprenticeship, at the cost of their employers, during the first few years of their professional life. We call the tyro an apprentice or pupil, and he pays his master instead of being paid by him. I have the strongest feeling against any attempt to substitute collegiate teaching for practical apprenticeship; so far as colleges attempt to teach practice, they are and will be a sham in this country and in all others. The work of a college is to teach those sciences which are applied in the arts; but it can go a little further, and indicate to its students how the application is made in at least a few selected instances. Applying this dictum to the education of an engineer, his college can teach him mathematics, natural philosophy, chemistry, and geology. No one can doubt that a youth well trained in these branches of knowledge will, even with no further teaching, learn more during his apprenticeship, and during his whole professional life will take a higher standing, than the man of equal intelligence untrained in science. College can however, do more than this; it is found that a lad will go through a considerable number of books of Euclid, and yet see so dimly how his knowledge is to be connected with practice that he may be unable even to compute the area of a field the dimensions of which are well known to him; and far more is it seen that a man may be fairly grounded in mathematics, and yet have very little idea how to apply his knowledge to mechanical problems. It is the business of those who hold such chairs as mine to point out the connection between pure science and practice; to show how mathematics are employed in mensuration and in mechanical calculations; to show how the truths of physics are made use of in designing economical machinery, as when we teach the connection between the laws of heat and the steam engine. The student who has once grasped the fact that there is a real connection between practice and theory will seldom be at a loss how to find or search for that connection in after life. The student thus prepared knows what he has to learn from practice, and need not lose precious time in blundering over the numberless scientific problems which practice is sure to suggest but can never solve. The education of the architect, the practical chemist, the manufacturer, and the merchant, must be similar, *mutatis mutandis*, with that of the engineer. Assuming, then, that the education of those who are to follow more or less scientific pursuits must consist in acquiring first, that theoretical knowledge which practice cannot give, and, secondly, the practical knowledge which schools should not attempt to give, there remains the question whether the theoretical preparation should be given in special colleges, or universities such as our own. I have no hesitation in preferring the university. Mathematics, physics, chemistry, geology, botany, languages, all form elements required in various combinations in the education of all students. There is but one kind of mathematics, one kind of pure physics, and so forth. Surely it is better that we should teach the men belonging to different professions side by side, so long as the matter taught is to be the same. There are many dangers in an opposite course. There are not a sufficient number of competent teachers to allow of much differentiation. Segregation at an early age is not apt to foster professional peculiarities and narrow-mindedness. There is great danger, if physics are to be taught specially to engineers, that a special kind of physics, erroneously supposed to be specially useful to them, will be invented. Lastly, the contact of students and professors of one faculty with the students and professors of other faculties is very beneficial to all. Do not, therefore, cripple old universities by withdrawing from them a portion of their students and their professors, to set up special, professional, or technical colleges of a novel kind, but rather add by degrees to the power and usefulness of old institutions, and found new colleges and universities after the model of those which are found to have done good work. As an example of what may be safely done, I consider that in Edinburgh we require a chair of architecture, and lectureships on navigation and on telegraphy. There is, further, much want of a teacher of mechanical drawing. The professors of physics and chemistry require additional accommodation for practical laboratories, and additional assistance. If these additions were made, our college would, in my opinion, meet all the requirements for superior technical education in this part of Scotland. For 2,000*l.* per annum all these additions might be made. Notwithstanding the acknowledged importance of education establishments for giving the higher kinds of instruction are never self-supporting, and students must everywhere be bribed to come and learn. Immediate prizes, in the form of bursaries, scholarships, and fellowships, are required to induce men to cultivate the older fields of learning, and similar bribes are needed to promote the tillage of the more recently colonised domains of applied science. The Whitworth scholarships are a noble example of munificence thus directed, although, in my opinion, the examination requires considerable reform. I hope that further benefits of this kind will be conferred on those colleges which give efficient teaching. Local ambition is most effectually stirred by local prizes, and I regret to find a certain apathy among students here with respect to the Whitworth competition. This appears to arise partly from dissatisfaction with the mode of examination, and partly from the fact that the examiners are men not well known in Scotland. Leaving the question of technical training for the upper classes, and the still larger question of scientific teaching in second grade schools, the consideration of which would lead us too far a-field, I purpose to say a few words on the technical education of the skilled artisan. This we must treat on the same principles as have been applied to professional teaching. We must endeavour to prepare the lad in school, by teaching him those things which he cannot learn in workshops, but which will enable him to work with greater intelligence while acquiring and applying his practical knowledge. I shall not now speak of the general education which should make him a good man, and which should open to him those great sources of rational enjoyment arising from culture; I will restrict myself entirely to his preparation for becoming an efficient workman. I have in many places said, and I cannot say too often, that the great want of the workman is a knowledge of mechanical drawing. Unfortunately, I can obtain little attention from the general public to this demand for the workman. Very few persons not being engineers know at all what mechanical drawing is. I am sorry to say that some examiners in high places, who direct the education of the country, know very little more than the general public, and teachers who should give bread give chaff. I have lived much abroad, and come into close contact both with English and foreign workmen, and I unhesitatingly say that the chief, if not the only, inferiority of Englishmen has been in this one branch of knowledge. I must explain to some of my hearers what mechanical drawing is. It is the art of representing any object so accurately that a skilled workman, upon inspecting the drawing, shall be able to make the object of exactly the materials and dimensions shown, without any further verbal or written instructions from the designer. The objects represented may be machines, implements, buildings, utensils, or ornaments. They may be constructed of every material. The drawings may be linear, shaded and coloured, or plain. They must necessarily be drawn to scale, but various geometrical methods may be employed. The name of mechanical drawing is given to one and all those representations the object of which is to enable the thing drawn to be made by a workman. Artistic drawing aims at representing agreeably something already in existence, or which might exist, and for the sake of the representation; mechanical drawing aims at representing the object, not for the sake of the representation, but in order to facilitate the production of the thing represented. Now, I say that it is this latter kind of drawing which is so vastly important to our artisans, and hence to our whole wealth-producing population. Very few workmen, or men of any class, can hope to acquire such excellence in artistic drawing that their productions will give pleasure to themselves and others; but a great number of workmen must acquire some knowledge of the drawings of those things which they produce, and there is not one skilled workman or woman who would not be better qualified by a knowledge of mechanical drawing to do his work with ease to himself and benefit to the public. Mechanical drawing is a rudimentary acquirement, of the nature of reading, writing, and arithmetic. In order that a man may understand the illustrated description of a machine, he must understand this kind of drawing. To the general public an engineering drawing is as unintelligible as a printed book is to a man who cannot read. The general public can no more put their ideas into such a shape that workmen can carry them out, than a person ignorant of writing can convey their meaning on paper. Reading and writing on mechanical or industrial subjects is impossible without some knowledge of the art I am pressing on your attention. This art is taught abroad in every industrial school; a great part of the school-time is given up to it. In a Prussian industrial school one-third of the whole time is given to it. A French commission on technical education reported that drawing, with all its applications to the different industrial arts, should be considered as the principal means to be employed in technical education. Now, I deliberately state that this subject is not taught at all in England, and that the ignorance of it is so great that I can obtain no attention to my complaints. A hundred times more money is spent by Government to encourage artistic drawing than is given to encourage mechanical drawing, and I say that mechanical drawing is a hundred times more important to us as a nation. Moreover, the little *quasi*-mechanical drawing which is taught is mostly mere geometrical projection, a subject of which real draughtsmen very frequently, and with little loss to themselves, are profoundly ignorant. Descriptive geometry and geometrical projection are nearly useless branches of the art, and the little encouragement which is given is almost monopolised by these. Mechanical drawing proper is confined to those who pick it up by practice in engineering offices. These draughtsmen are often excellent, and on their behoof I claim no other teaching. I speak for the artisan who makes and for him who uses machinery. There are two ways in which our shortcomings may be remedied. First, the schools of art now established in this country should be enlarged so as to teach real mechanical drawing, and the examinations conducted by the Science and Art Department should be greatly modified; secondly, the drawing which is to be taught in the schools under the superintendence of the new school boards may be and ought to be mechanical drawing. Freehand drawing, as a branch of primary education, will, I fear, be a useless pastime; but whether that be so or not, I am certain that the accurate and neat representation of the elementary part of machinery and buildings would be popular with the pupils, and could be effectively taught. This kind of drawing educates hand and mind in accuracy, it teaches the students the elements of mensuration and geometry, and it affords considerable scope for taste where taste exists. The chief difficulty will be to obtain competent teachers. I should occupy you too long were I to attempt to show how these must themselves be trained. My chief aim to-day has been to claim attention for a most important and wholly neglected branch of education. I shall probably be expected to urge the teaching of other natural sciences in our primary schools; nothing, indeed, would give me greater pleasure than to think this could be done. I confess I doubt it, and while our second grade schools are what they are in this respect, and while the Cambridge examination for a degree in applied science is what it is, I dare not think of natural science classes in our primary schools. I shall be delighted if I am mistaken, but I am certain that mechanical drawing deserves our first attention, as most immediately useful to the artisan, and most easily taught. The very books on natural science which are published in England cannot be properly illustrated for want of competent draughtsmen, and children would be unable to follow the illustrations and diagrams, if ignorant of the principles on which they are constructed. I look rather to good reading books, explained by intelligent masters, as the best manner of teaching the elementary and all-important truths of natural science. No man could do better service than in compiling such reading-books, and there are few wants more urgent than that of masters competent to enlarge upon texts which would thus be put into their hands. The education of our workmen is far more incomplete than that of our professional men. Small additions to existing institutions will meet the want of the latter; but for the former the institutions have to be erected almost from the foundation.

## SCIENTIFIC SERIALS

In the *Scottish Naturalist* for July, Dr. Lauder Lindsay finishes his article on Natural Science Chairs in our Universities, and concludes by pointing out that in this country the most eminent of our naturalists, Darwin, Owen, Huxley, Hooker, Bentham, Berkeley, Murchison, Lyell, Lubbock, Sclater, Wallace, Gwyn Jeffreys, are not, and never were, University professors, while many of the occupiers of natural history chairs have never properly discharged the duty of professors, and their opinions carry no authority in scientific matters. The remainder of the number is occupied by short articles and notes on various points of Scottish natural history.

The greater part of the *American Naturalist* for July is occupied by two long articles, entitled, "The Ancient Indian Pottery of Marajó, Brazil," by Prof. C. F. Hartt, and "Application of the Darwinian Theory to Flowers and the Insects which visit them," both illustrated with cuts. The latter is a re-translation of Prof. Delpino's annotated translation into Italian of Dr. Müller's address at Lippstadt in 1869. The former is a very interesting account of the pottery exhumed from the Indian burial places at various localities in the Valley of the Amazonas. These vases were used for the reception of the remains of the dead, and are found associated with rude idols. We have no historical record of the tribe that built the Marajó mounds, and no record of the existence of any tribe in the Lower Amazonas within historic times that buried its dead in jars. Prof. Hartt does not agree with von Martius in supposing these mounds were made by Indians of Tupi descent. He thinks, on the other hand, that there are many resemblances between the pottery of Marajó and that of Peru and North America that are well worth study.

## SOCIETIES AND ACADEMIES

### Paris

Association Scientifique de France July 29.—M. Leverrier in the chair. The meeting took place in the hall of the Society for the Encouragement of National Industry, in the Rue Bonaparte, and was the first meeting since M. Leverrier was Director of the National Observatory. Subsequently to his dismissal an Imperial decree, dated July 13, 1870, had proclaimed the association to be an institution of public interest; but no meeting took place in consequence of the events of the war. The number of members amounts to ten thousand, subscribing eight shillings each, and the funds of the society are to be employed in promoting scientific experiments. The society is governed by a standing committee. M. Glais Bizoin, a member of the delegate government, M. Barral, the celebrated agriculturist, and many other scientific gentlemen, are counsellors. Many of the subscriptions were discontinued during the war, and it is expected that not a few members will resign, owing to the pressure of the times; but an active propagandism is contemplated. The national exchequer being impoverished by the war indemnity, and every scientific expense being curtailed or suppressed, much is to be hoped from private exertions for saving France from scientific degradation. It is rumoured that the laboratories established at the Sorbonne and other public establishments by the Empire during M. Duruy's ministry will be closed for want of money.—M. Sanson, the general secretary, read a report adopted by the council at the meeting of July 18, which was also adopted by the General Assembly. Every member is asked most earnestly to pay at once all the contributions in arrear, and the contributions which become due up to the month of March 1872. In doing so the Association will be enabled to enlarge the field of its opepations, and to start with new life. The Association publishes every month a periodical, which is sent free to all its members, and is sold at the very low price of 2*s.* 6*d.* a year. This periodical publishes the account of the monthly meetings, as well as much scientific news of general interest. It was resolved that the immediate attention of the Association should be devoted to the determination of the reports of the amount of rain in France, a subject of the highest importance for all agricultural purposes; and to the observation of falling stars, a subject not less useful for the science of the constitution of the earth. The meteorological correspondents of the Society are instructed to notice the variations in the distribution of rain, which can be attributed to the presence of woods or their destruction, for agricultural purposes, as well as any facts relating to the pluviometrical history of the country. A special instruction is to be sent to those who have volunteered for the observation of falling stars, everyone is to be qualified by a previous instruction in the knowledge of the constellations. The society published two or three years ago special maps, similar to the maps published by the British Association for the same purpose, but differing in many important details. M. Pierret, the director of the telegraphic lines, has given strict orders that telegraphic lines could be made use of for the comparison of the chronometers used in the stations. The exchange of telegrams will take place on the 9th, 10th, and 11th August, at four o'clock in the evening, and at eight in the morning, between the different places, where temporary observations are to be made. Paris, Evrecy (Calvados), Mans, Chartres, Rochefort, Poitiers, Bordeaux, Limoges, Toulouse, La Guerche, Montpellier, Marseilles, Tournus, Lyons, Barcelonette, Toulon, Nice, Genoa, Turin, Bayonne, Agde: twenty-one stations and several in Italy or in Spain in connection with the French system. Competent calculators are to reduce and compare observations. If the funds of the Society are sufficient, the labours will be paid for. The watch will be kept during the nights from 9-10, 10-11, 11-12. M. Leverrier will revise the calculations, give the proper directions for observations and draw the general report.—M. Bert, who was formerly a prefect at Lille during the investment of Paris, has resumed his labours at the Jardin des Plantes, and read a very able paper on respiration.—M. Dagron, a photographer, who escaped from Paris by balloon, read a paper on microscopic photograph, which he organised at Tours and at Bordeaux. The photograph is executed on a film of collodion, which he calls a pellicle, and which is lighter than paper; it is, besides, perfectly homogenous, and can be submitted to very powerful instruments. M. Dagron obtained extraordinary effects, which can be judged from the following facts. Each pellicle has a weight of less than $\frac{1}{20}$ of a gramme, and the matter photographised on it is sufficient to fill from twelve to sixteen folio pages of ordinary print. A single pigeon carries 50,000 messages, weighing less than a gramme. During the investment of Paris 115,000 messages were sent in succession, but several of them were sent fifteen times. The total number of messages sent, counting each repetition a new one, was 2,500,000; of the carrier-pigeons very few found their way to Paris, and these chiefly at the end of the investment. But owing to the repetition system, almost every message was received. Some of them were late, it is true, several carrier-pigeons having returned in February only. Observations are asked for a large bolide of the 19th of July, which might possibly have been observed in England.

## BOOKS RECEIVED

English.—A Course of Natural Philosophy: R. Wormell (Groombridge and Sons).—An Elementary Course of Theoretical and Applied Mechanics, 2nd edition: R. Wormell (Groombridge and Sons).

American.—Twentieth Annual Report of the Regents of the University of the State of New York on the Condition of the State Cabinet of Natural History.—Annual Report of the Board of Regents of the Smithsonian Institution, 1869.—Report of the Commissioner of Agriculture for 1869.—Monthly Reports of the Department of Agriculture for 1870.—Reports on the Diseases of Cattle in the United States.

## PAMPHLETS RECEIVED

English.—Life and the Equivalence of Force, pt. II.; Nature of Force and Life: J. Drysdale.—On the Undercurrent of the Ocean: Capt. Spratt.—Lisdoonvarna Spas and Sea-side Places of Clare: Dr. Mapother.—Abstract of the Reports of Survey, and of other Geographical Operations in India, 1869-70.—On Recent Investigations and Applications of Explosive Agents: Prof F. A. Abel.—Reply to Prof. Allen Thomson's Address to the British Association (Section D): R. H. Collyer.—Review of the *Lancet's* article on the History of Anæsthetic Discovery: R. H. Collyer.—Mysteries of the Vital Element: R. H. Collyer.—John Hampden Triumphant.—A Shilling's Worth of Political Economy: N. A. Nicholson.—Brazilian Republican Address.—Handbook of Devonshire: Exeter.

American and Colonial.—On the Evidence of a Glacial Epoch at the Equator: Prof. J. Orton.—The Huron Race and its Head-form: D. Wilson. Note on the Spectrum of the Corona: Prof. C. A. Young.—The Western Educational Review, July.—Embryological Studies on Diplax, Perithemis, and the Thysanurous genus Inostoma: A. S. Packard, junr.—Volcanic Manifestations in New England: W. T. Brigham.—Proceedings, Communication, and Bulletin of the Essex Institute; a parcel.—Proceedings of the Albany Institute, vol. I, pt. I.—In Memoriam Francis Peabody.—On Insects inhabiting Salt Waters, No. 2: A. S. Packard, jun.—Bristle-tails and Spring-tails: A. S. Packard, jun.—List of Insects collected at Pebas, Equador: A. S. Packard, jun.—Early Stages of Ichneumon Parasites: A. S. Packard, jun.—Morphology and Ancestry of the King Crabs: A. S. Packard, jun.—Embryology of Limulus Polyphemus: A. S. Packard, jun.—Catalogue of the Balænidæ of California:—A. S. Packard, jun.

Foreign.—Bulletin Mensuel de la Société d'Acclimatation.—Sulla influenza delle materie minerali, nei processi nutritivi dell' organismo umano: Dr. G. Polli.

## CONTENTS

THURSDAY, AUGUST 24, 1871

## COOKE'S HANDBOOK OF BRITISH FUNGI

*Handbook of British Fungi.* By M. C. Cooke, M.A. 8vo., pp. 982, tab., fig. 408. (London: Macmillan and Co., 1871.)

THE study of Fungi in this country has gradually attained an importance which is sufficiently indicated by the appearance of the present much-needed work, comprising as it does the characters of no less than 369 genera and above 2,800 species. The works of Bolton and Sowerby at the latter end of the last and the commencement of the present century had laid a solid foundation for a study which, however, attracted but comparatively few students. There was, however, no genera treatise on fungi, in our own language, to which referencel could be made, till 1821, when Gray's "Natural Arrangement of British Plants" gave the English botanist an opportunity of becoming acquainted with the labours of Nees von Esenbeck and other continental botanists, a very important share of the labour having been undertaken by Dr. J. E. Gray. A storm of opposition was raised against it because of its recommendation of a natural system, a recommendation which was then thought sufficient to justify an exclusion from well-deserved honours; a virulent attack was made in the *British Critic*, and the work fell in consequence, notwithstanding its merits, almost dead from the press. Some ten or fifteen years later, Sir W. J. Hooker undertook the completion of the English Flora, which had not gone beyond the higher Cryptogams, his own "Scottish Flora," Greville's "Flora Edinensis," and the "Scottish Cryptogamic Flora" having already done much for fungi, when the preparation of the part of the work relating to those plants was entrusted to the Rev. M. J. Berkeley, who had made an especial study, especially of the higher Fungi, and had already discovered the true structure of the hymenium, which had, however, long before been indicated under *Agaricus comatus* in the "Flora Danica." From the time of the publication of his volume, continual accessions were made, especially by Mr. C. E Broome and Mr. G. H. K. Thwaites, who has since done so much for this interesting tribe as well as in the higher orders of plants in Ceylon, and which have been incorporated in a series of memoirs in the "Magazine and Annals of Natural History," either singly by Mr. Berkeley or jointly with Mr. Broome; nor must we omit Mr. Currey's very important contribution to the knowledge of our British Sphæriæ, of which it is scarcely possible to overrate the value as regards the characters of the fructification. It was then proposed by Messrs. Reeve to publish Outlines of British Fungology, confining, however, the description to those species which did not require much microscopic aid, but adding a list of all the known species so far as the existing state of information went. Mr. Broome and Mr. Currey, with several others, have persistently carried on the study of these plants, the knowledge of which is every day advancing, and as Mr. Berkeley's work was confessedly imperfect, we have great reason to be thankful to Mr. Cooke for undertaking the very laborious, and, we fear, scarcely remunerative labours which he has so successfully accomplished. No student of Fungi can be without the two volumes, and they certainly ought to have a place in every botanical library of the slightest pretension. The work has throughout been conducted in the most conscientious way, and infinite pains have been taken to verify the obscurer species, in which the author has had the ready assistance of those botanists in this country who have paid most attention to these difficult plants.

Mr. Cooke has very wisely been content to follow the more generally established systems without attempting any new arrangement, which at present would only entail needless obscurity. He has, we think, very judiciously given the characters of all species which, with any degree of justice, have been considered as autonomous; though more than reasonable doubts have been thrown on many of them by the labours of Tulasne, and the real nature of such genera as Cytispora, &c., had been long since previously indicated by Fries. When all the different stages of development have been thoroughly studied, the number of genera will doubtless be much restricted, as it has been already by the elimination of mere mycelia. It would, however, be premature to pass by numbers of Sphæronemei, Mucedines, &c., because some of them have been clearly ascertained to be mere conditions of ascigerous species. We are glad, too, that Saprolegniæ are included; though this very curious set of plants has been less studied in this country than on the Continent. The occurrence of zoospores is now no obstacle to their being considered as conditions of Fungi, since we have distinct zoospores in such genera as Peronospora, and the whole tribe of Myxogastres. It is but justice to state that Mr. Cooke has had some valuable assistance amongst the higher Fungi from Mr. W. G. Smith, who is so well known as a botanical artist, and whose communications cannot fail to have materially enriched the work, the execution of which throughout has been beyond all praise, in which should be included the copious index It is not to be supposed that in so extensive and difficult a subject a critical eye could not find a few errors, but they are few in number and of little importance. The gravest to which we might advert is that in the characters of several of the genera proposed by Tulasne, there is no mention of the secondary forms on which several of them are established, though they are not omitted where species are concerned. This is, however, a matter of comparative unimportance, and a few spots on which the finger might be placed do not detract from the general merit of the work, which we cordially recommend on many accounts to our readers, assuring them that the moderate price at which it is published could scarcely be better employed in any other scientific direction.

## OUR BOOK SHELF

*Matter for Materialists.* By Thomas Doubleday. (London: Longmans, Green, Reader, and Dyer; Newcastle-upon-Tyne: Andrew Reid, 1870).

*The Beginning: its When and its How.* By Mungo Ponton, F.R.S.E. (London: Longmans, Green, and Co., 1871.)

THIS age is essentially a materialistic one, but few are found who adhere to systems of philosophy based on the assumption that matter has no real existence. Mr. Doubleday, however, is one of the few, and he has published "a series of letters in vindication and extension of the principles regarding the nature of existence of the Right Rev. Dr. Berkeley, Lord Bishop of Cloyne." His argument is that our notions of time, motion, and magnitude are merely relative; that the idea of space in the abstract is entirely beyond the grasp of the human mind, and leads to a series of absurdities and contradictions. But without such a conception, our notions as to matter are untenable, and hence we are driven to seek for other principles to explain the nature of existence. These Mr. Doubleday believes are to be found in the system of philosophy which Bishop Berkeley founded, or rather the idea of which he indicated, although he did not live to bring it to perfection. This, the most purely idealistic system ever promulgated, entirely denies the existence of matter, and holds that there are only spirits, thinking beings whose nature consists of conception and volition; whose sensations are derived from one superior Spirit *in* whom they exist. Mr. Doubleday, after endeavouring to show that unless we adopt this view we are led into innumerable contradictions, asserts that materialism is the parent of scepticism, since a mind which finds itself involved in a hopeless struggle to reconcile inconsistencies, takes refuge in believing nothing. All this the author expresses clearly and concisely, so that even those who are not inclined to accept his views will read his work with pleasure, and are sure to glean some new ideas from it. At the same time, when opinions almost universally held are attacked, it is necessary that he who assails them should be scrupulously accurate even in matters of little importance. Therefore it is a bad fault that we find in this work chemical formulæ, given at the very outset, in which P is taken as the symbol of Platinum, and Ch as that of Chlorine. It is also astonishing to find any one who supports the "emission" theory of heat, and who does so chiefly by quibbling about the expressions used by those who have so conclusively shown that heat is a mode of motion.

"The Beginning," the other book at which we have to glance, is one of those volumes which seem a mere confusion of facts, which, though they may be interesting in themselves, lose their value from having no proper connection or arrangement. Consisting of nearly six hundred pages, this work has in it a little of everything; but to find out what it all leads to, and what is the general drift of the whole, is next to impossible. Just at the end the author devotes a separate and comparatively small space to considering the possibility of reconciling the Hebrew records relating to the Beginning with modern scientific discoveries. In this more method is found than in the body of the work, and the conclusion arrived at, that we must "exclude all other suppositions save that of regarding the creative epochs as periods of indefinite and immense duration," is one to which few will be disposed to object. Yet in this also stray facts seem to lie upon the pages as if scattered indiscriminately from a pepper-box. The plates with which the work is illustrated are certainly very good; but we fear that it is one of those expensive books that find few purchasers.

*Our Sister Republic.* A Gala Trip through Tropical Mexico in 1869-70. Adventure and sight-seeing in the Land of the Aztecs, with Picturesque Descriptions of the Country and People, and Reminiscences of the Empire and its Downfall. By Colonel Albert S. Evans. With Numerous Engravings. (Hartford, Conn.: Columbian Book Company. London: Trübner and Co., 1871.)

THE author of this book accompanied the Hon. W. H. Seward in an apparently semi-official tour through Mexico, lasting from September 1869 to January 1870 The volume before us, in somewhat flowery and very "smart" style, tells what the author saw and a good deal of what he heard during the progress through that American battle-ground, of which we hear so much and know so little. The author writes with much enthusiasm and hopefulness of the people, the products, and the progress of the country, where he was received with such exuberant hospitality; although, considering the short time he was in the country, and the conditions under which the tour was made, anything like a full and reliable account of the political, social, and commercial condition of the country was not to be looked for. We believe, however, most readers will know much more about the life-manners of the Mexicans after than before reading the work. The author has fervid Republican propensities, and we fear writes too often with red ink. He has nothing but little words for the Maximilian episode, and regards the unfortunate would-be Emperor as an unprincipled heartless adventurer. We are glad to see the author has paid considerable attention to the state of education in the country, and if we can at all rely upon his statistics, it is in a much more hopeful state than Europeans are generally inclined to believe. There appears to be plenty of funds set apart chiefly by the benevolent for educational and charitable purposes; indeed, according to Colonel Evans, the wealth and resources of Mexico are almost enormous, but, as might be expected in such a chronically revolutionary country, the management of them is wretched. The Colonel is evidently not a scientific man, and although he frequently alludes to the products of the country, it is generally either from a commercial or picturesque point of view. We commend the book as an exceedingly interesting and graphically written record of a four months' trip through Mexico, and as a work which affords a very fair notion of the present actual condition of the country and of its interesting antiquities.

*Horses: their Rational Treatment and the Causes of their Deterioration and Premature Decay.* In Two Parts. By Amateur. (London: Baillière, Tindall, and Co., 1871.)

IN Part I of this work the author tries to explain scientifically the errors of the present routine mismanagement (as he calls it), and how it is opposed to the natural system and health of the horse; and in the second part he considers and explains the practical management of the horse under what he calls the Rational System. The author advocates a return to the natural feeding of the horse, such as grass and similar soft food, and an abandonment of the present almost universal system of forcing with an abundance of dry food, on the ground that thus the horse would live to a much greater age, and perform a far greater amount of work. The subject certainly deserves the serious consideration of all who are interested in horses, and to all such we would recommend the perusal of this little book by one who has evidently given the subject long and serious study. In the second part both sides of the question are well stated in a correspondence between the author and Sir James Yorke Scarlett.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Mr. Stone and Professor Newcomb

MR. NEWCOMB has reviewed Mr. Proctor's book on the Sun in your number of May 18, and Mr. Proctor has replied in the number for June 1. In each of these articles I find my work and name mentioned in a way that is scarcely satisfactory to me. Mr. Proctor's reply is, however, of course, only intended to defend his own work, not mine. In Mr. Newcomb's review I find the following very strong passage. Mr. Newcomb says:— "We find ligaments, black drops, and distortions sometimes seen in interior contacts of the limbs of Mercury or Venus with that of the sun described as if they were regular phenomena of a transit; without any mention of the facts and experiments which indicated that these phenomena are simple products of insufficient optical power and bad definition, which disappear in a fair atmosphere with a good telescope well adjusted to focus." With respect to facts, I must be allowed to observe that I believe the facts are entirely the other way. This is a point which can only be tested by appealing to the facts themselves.

Wales and Dymond observed the Transit of Venus in 1769 in the Hudson's Bay. This is their account of what they observed:—"We took for the instant of first internal contact the time when the least visible thread of light appeared behind the subsequent limb of Venus, but before that time Venus's limb seemed within that of the sun, and his limb appeared behind hers in two very oblique points, seeming as if they would run together in a broad stream, like two drops of oil, but which, nevertheless, did not happen, but joined in a very fine thread at some distance from the exterior limb of Venus. This appearance was much more considerable at the egress than at the ingress, owing, as we apprehend, to the bad state of the air at the time. We took for the instant of internal contact at the egress, the time when the thread of light disappeared before the preceding limb of the planet, from which time W. W. took notice that he had told about 24$^s$, when the limbs of the sun and Venus were apparently in contact; a circumstance which he did not venture to attend to at the ingress."

The observers evidently saw these phenomena both at ingress and egress. From the detailed account at ingress the definition must have been very good. I have printed the whole passage, including the part which may be turned against my argument, that "the appearance was more considerable at egress, owing, as we apprehend, to the bad state of the air." That the appearance of such a ligament seen under great atmospheric tremor may have been more striking, I can well believe.

Again, Chappe writes:—

"A l'entrée totale de Vénus, j'observai très destinctement le second phénomène que avait été remarqué par la plus grande partie des astronomes en 1761. Lebord du disque de Vénus s'alongea comme s'il étoit attiré par le bord du Soleil. Je n'observai point pour l'instant de l'entrée totale celui où le bord de Vénus commençoit a s'alonger; mais ne pouvant pas douter que ce point noir ne fît partie du corps opaque de Vénus, j'observai le moment où il étoit à sa fin; de façon que l'entrée totale ne peut être arrivée plutôt, mais peut-être plus tard de deux ou trois secondes. Le point noir étoit un peu moins obscur que le reste de Vénus. Je crois que c'est le même phénomène que celui que j'observai à Tobolsk en 1761." I might quote other extracts. The phenomena are noted at Wardhus at the egress. It is expressly stated by Cook and Green at Otaheite that the extinction of the thread of light between Venus and the sun was gradual, and that at Otaheite the observers did not note the end at the ingress and the commencement at the egress. Now, as a practical man, I would ask Mr. Newcomb are not these appearances observational facts? They appear to me so real, that, to admit their non-reality, would be the same thing as if we were to argue that if Wales, Dymond, and Chappe had put down in their observing books the times when the limbs of Venus first appeared in contact, instead of waiting until they could not see the slightest trace of any connection between the limbs of Venus and the Sun, they would not have given earlier times than those which now appear in their journals; or that if Cook and Green at Otaheite had given the times corresponding to the last appearance of any connecting ligament at the ingress, the times given would not have been later than those which now appear in their journals. It was from these considerations, which appear clear enough, that I have treated the Otaheite observations as referring to a different phase from those of the Hudson's Bay observers, and Chappe's ingress observations. Similar remarks apply to the egress observations. You cannot talk of such appearances being simple products of insufficient optical power and bad atmospheric circumstances. The appearances presented to and described by Wales and Dymond, even at the egress, took place according to their own estimation, which is the largest, within about a second of arc. Such appearances could not be discriminated amongst with insufficient optical power, and under very bad circumstances of observing. Chappe particularly uses the phrase "très-distinctement." This point appears to have been overlooked by many who have written much upon the subject. With respect to experimental facts, I should, indeed, esteem it a great favour, and I am sure that it would be important as bearing on our preparations for the Transit of 1874, if Mr. Newcomb can refer us to any experiments bearing upon this point. It will, however, be necessary to understand clearly the positions taken up.

First, I assume the existence under sufficient illumination of irradiation. Secondly, I assume that the illumination of the sun is so great that under the ordinary circumstances of telescopic observation the optical enlargement of the sun's disc due to this phenomenon is about 3″, the exact quantity will vary under different circumstances. The data upon which these assumptions are grounded are, amongst others, the experiments of Dr. Robinson, vol. v. Mem. Royal Astron. Society, and the eclipse discussions made from observations with the great equatorial of the Greenwich Observatory. Can Mr. Newcomb refer us to any experiments which have been made with a disc sufficiently illuminated to present under the circumstances of examination an optical enlargement of 2″, and in which sufficiently powerful optical means have been employed to discriminate between the changes presented within 1″ of arc, as a small portion of the illuminated surface near the limb has been cut off by an opaque body?

I know of no such experiments. I do know that experiments were made at Paris by Wolf, in which the illumination of the disc was such that no sensible optical enlargement was exhibited. The results obtained had, therefore, no bearing on the question of irradiation; they were simply experiments on the disappearance of a small portion of a feebly illuminated disc. The results are such as any one conversant with the subject would have predicted with such a disc. The diameter of Mercury is so small that the appearance presented in a transit would not be so clearly marked as in a transit of Venus. Of the reality of the appearance of a connecting ligament in the transit of Mercury of 1868 I have no doubt, for I saw it. I would, with all due diffidence, give here a word of caution respecting discussions of these results. The phenomena under discussion, whether real or supposed, are presented only within a second of arc from the sun's limb. It is perfectly useless, therefore, to appeal to upon this question any observations which have been made with insufficient optical means to subdivide a second of arc.

The optical enlargement by irradiation is a function of the brightness, and can be made insensible by sufficiently diminishing that brightness. Unfortunately, however, when this diminution of brightness is carried to a very great extent errors in an exactly opposite direction to those of irradiation will come into play, similar, in fact, to the results of Wolf's experiments. The observations of Mercury on the sun's disc in 1868 were made with very different optical means, and some very different methods were adopted for diminishing the sun's glare. If the observations are put together without any discrimination upon these points some curious results will appear. I am afraid that some gentlemen have been much misled by want of attention to these simple points. E. J. STONE

Royal Observatory, Cape of Good Hope, July 19

## On the Age of the Earth as Determined from Tidal Retardation

CONSIDERABLE discussions have taken place in the Geological Society and elsewhere in regard to Sir Wm. Thomson's conclusion that had the earth solidified several hundred millions of years ago, when it must have been rotating at a much greater rate than at present, its form ought to be different from what it actually is. That is to say, there ought to be a much greater difference than there is between the equatorial and polar diameters. I observe that the discussion on this point has lately been renewed at the meetings of the British Association.

Although I regard all the other arguments advanced by this eminent physicist in regard to the age of the globe, so far as I have been able to follow his reasoning, as unassailable, yet I never could agree to this conclusion deduced from tidal retardation. But, so far as I remember, I have nowhere seen stated what appears to me to be the real objection to the argument. The objection is as follows:—

As the rate of rotation decreases under tidal retardation, centrifugal force must decrease also. The consequence, therefore, is that the sea must be slowly sinking at the equator and rising at the poles (see *Phil. Mag.* for May 1868, p. 382) But denudation is also lowering the level of the land at the equator. Now the whole question concentrates itself into this, viz., will denudation lower the level of the land at the equator as rapidly as the sea sinks? This question, happily, can be answered. The method lately discovered of measuring the rate of subaerial denudation enables us to determine the rate at which the land at the equator is being lowered. We are enabled from the principles of mechanics to determine the rate at which the sea is sinking at the equator. By this means it can be shown that the land is being lowered by denudation as rapidly as the sea is sinking, and that consequently, in so far as this part of the argument is concerned, we cannot infer from the present form of the earth what was its form at the time when solidification took place.

But it must be borne in mind that four years ago when Sir William read his paper on the subject before the Glasgow Geological Society, the method referred to of determining the rate of subaerial denudation was then accepted by scarcely any geologists. Taking the ideas which at that time prevailed regarding the slow rate of denudation, his conclusions were perfectly legitimate. JAMES CROLL

Edinburgh, August 21

### Neologisms

THE word *prolificness*, though not a model, is not a monster. It is a hybrid; but so is vindictive-*ness*. The chief objection lies in the fact of the *ic* being not the ordinary adjectival formative, but the *c* in the *fac* of *facio = I make*.

The true compounds of this root *change* the vowel, where, as in *satisfaction*, *malefactor*, &c., we have *no* change. Here, the original combination was no compound, but merely a pair of words in contact with each other.

Now, if we lay aside the hybrid forms, and use the word *abstract* with a certain amount of latitude, we get the following real or possible series of analogies:—

1. *Prolifaction*, like *satisfaction*.

2, 3. *Prolificacity* and *prolificacy*; the former like *capacity* from *capax*, the latter like *efficacy* from *efficax*.

4. *Prolificiality*. This implies an adjective in *alis*, from a substantive like *beneficium*, whence *beneficial*.

5. *Prolificality* suggests *prolifex*, *prolificis*, *prolificalis*; like *pontifex*, *pontificalis*.

6. *Prolificence*. Here we must look at the same time to *adjectives* like *maleficus*, and to *participles* like *sufficiens*, *-entis*, *-entia*; the rule being that, formally, the *adjectives* have no abstract of their own; but, instead of it, the participal form in *-entia*. Hence the numerous words like *benevolence*, *grandiloquence*, &c.

To this class the form under notice belongs; and it will, probably, be admitted that *prolificence*, along with its predecessor *prolificality*, is the least exceptionable, of the list.

*Prolificality* is the best abstract: *prolificence*, perhaps, the better word. None of them, however, are forms which need only be known to be adopted. There is something to demur to in all them. What this is would require a longer discussion than is here practicable.

Of the present short notice the result is that it is easier to either impugn or to excuse such a word as *prolificness* than to find a substitute for it. R. G. LATHAM

## NOTES

WE are glad to be able to state that Her Majesty's Government has been pleased to accede to the request of the British Association with respect to the proposed Eclipse Expedition. We may therefore hope for a most important series of observations along a line extending from the Neilgherry Hills in India to Cape York in Australia. The observation in India will be entrusted to Mr. Pogson, Colonel Tennant, and Captain Herschel. Mr. Lockyer has been requested to observe in Ceylon. The observing stations in Java will be occupied by the Dutch Government, and possibly also by M. Janssen, while a strong expedition has been formed from Sydney and Melbourne. The necessary instruments will be sent out to Australia by the next mail, and those for India will follow shortly. As before, the Government not only help in money but in transport, camping, and the like. The handsome way in which the Government has at once responded to this appeal justifies all we have said regarding its good intentions towards science when the requirements of science are properly represented by responsible bodies. We may add that the Government have also agreed to undertake photographic observations of the approaching Transit of Venus.

OUR thanks are due to the *Times* for the article (reprinted in another column) in which it exposed the injustice which Mr. Cardwell attempted to perpetrate in the case of Prof. Sylvester's retirement. Prof. Sylvester being only a scientific man, was, of course, fair game for a placeman, but it is none the less amusing to see how the whole pleading of "precedent" and the regulations of the service was allowed to go for nothing the moment there was a question of a hostile vote, thus showing the injustice of Mr. Cardwell's appeal to justice. An Accountant-General with a taste for income-tax, to judge from the amount of retirement awarded in a recent notorious case, is a much more valuable public servant in the present most satisfactory condition of army matters than a professor of European reputation, who is emphatically the man to infuse that scientific method into our officers of which they are so much in need.

THE introductory addresses at the winter session of the London Medical Schools, which commences on the 2nd of October, will be delivered by the following gentlemen:—At Charing Cross Hospital, by Dr. T. H. Green; Guy's Hospital, by Dr. Oldham; King's College, by Dr. Rutherford; London Hospital, by Dr. W. J. Little; the Middlesex Hospital, by Dr. John Murray; St. George's Hospital, by Dr. John Clarke; St. Mary's Hospital, by Dr. Alfred Meadows; St. Thomas's Hospital, by Mr. Le Gros Clark; Westminster Hospital, by Dr. Basham. No introductory address will be given at St. Bartholomew's Hospital, The Lecturer at University College has not yet been appointed.

THE British Archæological Association has been holding its annual sitting at Weymouth. On Monday night, after the return of the congress from their tour of inspection in the villages of Preston and Osmington, the inaugural dinner took place at the Royal Hotel, under the presidency of Sir William Medlycott, Bart. On Tuesday the members and friends of the Association visited Maiden Castle, an immense earthwork fortification three miles from Dorchester, which was described by the Rev. Mr. Barnes. At the evening meeting of the Association the following papers were read: "On the Origin and Titling of English Laws;" "Report on the Municipal Archives of Dorset;" and on "The Cerne Giant."

WE learn from a correspondent in New Zealand that footprints of the Moa have recently been detected in a new district in the province of Auckland. The locality is at the mouth of the Waikenei Creek, near the settlement of Gisborne, Poverty Bay, near the Taruheru River. The slabs in which the impressions were found were about five feet below a deposit of silt and alluvium of different kinds which had been washed away by the action of the water, leaving the stone in which the footprints were found visible, very plainly indented and following each other in succession. On either side of this track were dents here and there, as though made by the bird's short beak in picking up food as he walked—the closeness of the stride favouring this belief. Hard by this spot Mr. Worgan picked up an old stone hatchet, which, from the signs of traces it bears, is doubtless as ancient as the tracks of the Moa. Casts of these footprints have been presented to the museum of the Auckland Institute. The length of the footmark from the heel to the tip of the centre toe was seven and seven-eighths inches; from the heel to the tips of the inner and outer toes, six inches; the distance of tips of the outer and inner toes was seven inches; the length of the stride was twenty inches from heel to heel, and there were eight impressions altogether.

THE account of the whirlwind at Chilton, Buckinghamshire, on July 30, is worth careful study. The correspondent, J. B., who writes to the *Times*, sends the following facts:—"The storm began about five o'clock in the morning, accompanied by terrific thunder, large hailstones, and a most violent and terrific wind. The piece of country devastated by this wind is about a mile long, and perhaps 100 yards wide; and in that track just 300 large trees have been destroyed. In one field, thirty-six large trees were blown to pieces, trunks split down and broken off; and in another, eleven large trees lie side by side. The roof and side of a cottage were blown away. In one field four waggons were destroyed; one, loaded with nearly two tons of hay, was blown clean over a high hedge, hay and all. An old blind cart horse shared the same fate; while another waggon was blown about twenty yards, over two hedges, the four wheels being discovered in four different fields, and one-half the waggon, which was broken like fire-wood, was not found at all till Sunday evening. This is no exaggerated account, for we have seen the ruins, and the various places have been photographed."

MR. E. DICKENSON of Springfield, Massachussetts, asks us to request dealers in English birds' eggs to forward him their price lists.

A CORRESPONDENT throws out the suggestion that the tragical explosion of gun cotton at Stowmarket might have been occasioned by the fall of a meteoric stone.

M. ZALIOSKI, in an article in *Les Mondes* on the explosion of explosive compounds, asserts that the explosive properties of inflammable matter are not dependent on the normal temperature of the atmosphere, but upon its hydrometric state. Gunpowders, he adds, during a drought acquire spontaneous explosive qualities, even without any elevation of temperature, while they are also more ready to act upon and communicate the smallest spark.

THE *Revue Scientifique* appears to be starting into renewed vigour since peace and order have been restored in Paris. The last number is largely occupied with the proceedings of the British Association, including an admirable translation of the President's address.

ON the 19th June very slight earthquakes were felt at Simla at 9.40 P.M. The weather has been very sultry.

AN attempt to obtain European ice for India by the Suez Canal has failed. Out of ninety tons of Alpine ice shipped only four arrived. It is probable the parties did not know the business so well as the Americans. As it is the Alps do not at present supply the Mediterranean, many parts of which use frozen snow from Mounts Olympus and Tmolus.

THE troubles of the Indian Government about snakes are serious. The number of deaths by snake bites is great, but the number of snakes is greater, and when the experiment is tried of paying for snakes killed, the local treasuries are in danger of depletion, a fradulent trade in dead snakes springs up worse than that in sham tigers. Science seems to be the only mediator. In Bangalore rewards were paid in one month for 1,913 snakes, but Dr. Nicholson has found on examination that only 6 per cent., or 123, were really poisonous.

THE rise of the Ganges in the month of June was thirty-five feet.

DR. BARON EÖTVÖS, the son of the late eminent Minister of Public Instruction in Hungary, is now in this country inspecting our school arrangements for teaching science. We hope he may go back with a satisfactory story to tell.

A MEETING of the German Astronomical Society is fixed to be held at Stuttgard, on the 14th of September.

SUCH of our readers as were interested in the discussion carried on at the beginning of last year, by Messrs. Sylvester, Huxley, Lewes, Ingleby, Croom Robertson, and Monck, on Kant's view of Space and Time as Forms of Thought (NATURE, 9 to 15) may be glad to know that in the fourth edition of the "History of Philosophy from Thales to Comte," which has just appeared, Mr. Lewes discusses the whole question of the distinction between Sensibility and Understanding, and also that of mental forms, as understood by Kant.

THE "Birmingham Saturday Half-Holiday Guide," containing sixty-eight closely printed pages and a good map, for the price of sixpence, demands a word of notice. It is admirably arranged in every particular, the various districts being undertaken by those especially acquainted with them, and the whole brought together under careful editorship. Our attention is naturally especially directed to the portion—about one-third of the book—devoted to the natural history of the district, which is produced under the superintendence and by the members of the active Birmingham Natural History and Microscopical Society. The work is one of those peculiarly suited to such a body, and is well executed, chapters being devoted to the ornithology, lepidoptera, coleoptera, conchology, botany, and geology of the surrounding country. A more careful revision of the scientific names would have improved the appearance of the book; but, as it is, it is certainly the best, as it is the cheapest, work of the kind which has yet been issued. The suggestions of routes and indications of interesting objects are concise and yet complete. We hope that Liverpool and Manchester will not be slow to follow the example of Birmingham, and that the naturalists of the former towns will come forward as their *confrères* have done, and discharge as ably their portion of the work.

THE extreme heat experienced during the first fortnight of August is worth recording. Mr. H. Steward has published the following figures:—Monday, Aug. 7, solar maximum temperature in vacuo, 113° F.; maximum temperature in shade, 82° F.; Tuesday 8th, 113° F. and 81° F.; Wednesday 9th, 110° and 84°; Thursday 10th, 112° and 86°; Friday 11th, 119° and 89°, Saturday 12th, 115° and 89°; Sunday 13th, 125° and 91°. These high temperatures are far exceeded by those published by Mr. F. Nunes, of Chiselhurst, who, by means of standard thermometers (the one in the sun being in vacuo, and placed on the grass) has obtained the following figures:—

| | Max. in Shade. | Max. in Sun. |
|---|---|---|
| August 1 | 78·1 | 147·0 |
| ,, 2 | 83·8 | 150·0 |
| ,, 3 | 83·7 | 150·2 |
| ,, 4 | 72·1 | 139·3 |
| ,, 5 | 77·3 | 148·0 |
| ,, 6 | 80·3 | 145·5 |
| ,, 7 | 81·8 | 148·0 |
| ,, 8 | 81·0 | 147·0 |
| ,, 9 | 85·8 | 151·5 |
| ,, 10 | 89·4 | 148·0 |
| ,, 11 | 91·7 | 150·7 |
| ,, 12 | 91·0 | 146·5 |
| ,, 13 | 91·1 | 147·0 |
| ,, 14 | 84·6 | 145·0 |

Surely there must be an error somewhere. The maximum temperature of Mr. S. or Mr. N. differ by 40° and 50°! Who is to teach or correct amateur meteorologists?

FROM the report of the special course of instruction in biology to science teachers at South Kensington, we learn that Prof. Huxley's recent course was attended by thirty-nine students, of whom thirty-two were present during the whole time occupied by the lecture and demonstrations, and have made all the prescribed reports. Miss Margaret A. T. Macomish heads the prize list.

ON Sunday, July 9, a magnificent waterspout was seen off Cork harbour. It did not last long, its breaking up being followed by a heavy downpour of rain, which extended for some miles round.

DR. PETERS, of the Clinton Observatory, N.Y., has discovered another of the small planets, making the 114th of the series.

The severe storm, which one of our correspondents described in our last number, will make August 12 remembered in many parts of the South of England and South Wales. In one place in Dorsetshire, 100 trees in one orchard were completely blown down, and forty trees in another. In other places we read of cottages being unroofed, and large trees carried away; and at Cardiff, a police station is stated to have been struck by a "meteoric stone," and some damage done, whilst during the storm "a shower of small green frogs" fell.

Prof. Newton has sent us an account of the shock of earthquake experienced at Boston, U.S., on the 13th of last month. It occurred early in the morning, and though comparatively slight, was still so plainly perceptible that those who felt it had no hesitation in attributing their sensations to the proper causes. Some persons who were abroad were affected with giddiness and momentary nausea, clocks were stopped in houses, beds and windows shaken, and vessels upset.

## *THE GOVERNMENT AND PROF. SYLVESTER*

PROF. SYLVESTER is one of our best mathematicians, and enjoys a European celebrity. He was, we believe, the first of the Jewish race to compete for the highest honours in the Cambridge Mathematical Tripos, setting an example which has since been followed by several distinguished men. Although he was Second Wrangler, his religious opinions disqualified him from obtaining at Cambridge the Fellowship for which he was well fitted, and which was morally his due. He had to leave the University, and after an interval, and in an evil moment for himself, accepted at the age of forty the post of Professor of Mathematics to the Royal Military Academy at Woolwich. Woolwich Academy, as our readers know, is the training school of officers for the Artillery and Engineers. It is one of those hybrid establishments, half regiment and half college, which are managed by well-paid officers in the Army who do not teach, and taught by ill-paid civilians who have nothing to do with the management. But, inasmuch as mathematics formed the principal of the studies, the Professor of Mathematics was an important personage, and corresponded more nearly than any one else to the character of Head Master. He had a house and a salary of six or seven hundred a year. In 1869 a Royal Commission was appointed to inquire into the condition of Military Education in this country, and much evidence was given by the Professor as to the working of the Academy. He advocated extensive changes in the system, many of which were recommended by the Commissioners, and have since been adopted. But there was one among their many recommendations which was not suggested by the Professor, and which assuredly could not have been intended by the Commission to work retrospectively, and without due consideration, upon the teachers then in office. In altering the government and organisation of the Academy, and proposing a change in the educational staff, it was suggested that "the Professors, Instructors, and other officials, if military men, should be appointed for seven years, at least, with the power of re-appointment. If civilians, their tenure of office should in no case continue after the age of 55, unless an extension be specially recommended by the Governor and approved by the Secretary of State." Acting upon the Report of the Royal Commission, the War Office informed Professor Sylvester on or soon after his 55th birthday that his services would be no longer required. They did not even let him stay a few weeks to complete his fifteenth year of servitude, but bundled him off with a profusion of compliments, and the Treasury at their instance awarded him a retiring pension of 278*l.* 1*s.* a year.

Two hundred and seventy-eight pounds one shilling a year—we may mark the precision of the calculation—is thus, according to the opinion of somebody at the Treasury, the proper compensation to be given on the abolition of his office to one of the first living mathematicians, who had spent the best fifteen years of his life—or, as they say at Whitehall, with pitiless accuracy, only fourteen years, ten months, and fifteen days—in the service of the nation. He was not told, when he entered the public service in the prime of life, that the tenure of his office would be thus terminable. On the contrary, the idea is entirely novel, and had never prevailed before in our military or naval schools. We believe that Professor Sylvester is the last man in the world to set his own private claims against the public exigencies of the Royal Military Academy. But he might fairly enough object to be the victim of an experiment in organisation without being properly compensated for the consequent change in his mode of life. The Royal Commission thought that too great prominence was given at Woolwich to abstract Mathematics, and recommended the union of the two Professorships of Mathematics and Mechanics, with a view to some modifications in the system of study. But if the reformation of the Academy required the abolition of the office, surely the holder of the office deserved to be duly compensated. It is not right for the State to treat a man of approved science and elaborate education, whom it invites into its service at the mature age of forty, on exactly the same principles of superannuation as are applied to an ordinary member of the Civil Service, who becomes a clerk, from whatever unambitious reason, at the age of seventeen. There is no parity of position between the two. If Professor Sylvester had completed his fifteenth year of service, his *minimum* rate of pension, according to the rules of the Civil Service, would have been 60*l.* a year more than he was at first awarded; but the War Office and the Treasury would not even grant him this. They offered him one shilling over and above his 278*l.* a year, and bade him God speed.

Fortunately for the Professor and for the public credit, Sir Francis Goldsmid took the matter up in the House of Commons, and gave notice that he would move an Address to the Crown that the Professor might receive as his pension, "two-thirds of the salary and emoluments enjoyed by him at the time of his removal, and humbly to assure Her Majesty that this House will make good the same." More, perhaps, he could not ask; less it would be shabby to offer. When the establishment at Greenwich Hospital was abolished, the members were pensioned off with lavish generosity. Men who had been there barely a couple of years received their full pay and an equivalent for their house and allowances. When Haileybury College was abolished by the East Indian Government, its Professors and teachers carried with them into private life two-thirds of their official salaries. It seemed to be a necessary conclusion that the men who manage these matters in the Government can be liberal, and even generous, at the expense of charity funds or of distant dependencies, but have a different measure to mete with in the case of a man of science who has given his ripe intellect to the service of this wealthy nation. We rejoice at last to hear that the First Lord of the Treasury has given the case his full, though somewhat tardy, consideration, and has removed the stigma which seemed to attach to the public conscience. A country whose wealth has increased of late so enormously, as the Chancellor of the Exchequer explained to us this Session, cannot afford to be niggardly in the treatment of its distinguished men. Any mistaken attempt in that direction would be sure to recoil upon its authors, as well as to discredit the very name of economy; and Mr. Gladstone has acted wisely as well as justly in thus rectifying at the close of the Session a lamentable departmental error.—*From the Times.*

## *SUGGESTIONS TO OBSERVERS OF THE SOLAR ECLIPSE OF DECEMBER NEXT*

AN analysis of the observations made at the recent eclipse seems to show that it would be desirable that three or four observers should be stationed some 10 miles to the north or south of the limits of totality to watch for—

1. *Shadow bands* passing along the ground, and to note carefully the direction of their motion and their velocity. If possible this should be done upon any three planes at right angles to one another, say the ground, an east and west wall, and a north and south wall.

2. *Direction and velocity of the wind*, and also the direction and velocity of the drift of the clouds. This should be done by observers associated with those employed on (1).

3. *Rays or brushes of light* from the thin cusps of the sun, and whether they alter their position and intensity.

4. *Spurious red prominences* from the thin crescent, noting carefully their position and size.

The following instruments might be used with advantage for polariscopic observations:—

1. A polarimeter, consisting of a Savart's polariscope mounted behind four plates of crown glass movable on an axis, perpendicular to the plane of polarisation, which is furnished with a graduated circle and pointer, to register the position in which the depolarisation of the plates neutralises the polarisation of the object. This should be used with a telescope having a diaphragm in the common focus restricting the diameter of the field to about 10′. The attention of the observer should be confined to the centre of the field (when the bands are made to disappear by rotating the plates).

2. A plate of right and left-handed quartz attached in front of a positive eye-piece, so as to lie in the common focus of the telescope and eye-piece, with an analysing Nicol placed between the lenses of the eye-piece, which should have a field about 1° in diameter.

The bi-quartz should be fixed so that the line of junction marks the plane of polarisation, giving the two halves purple alike.

The following polariscopic observations are suggested:

1. To determine the plane and measure the amount of atmospheric polarisation at at least three points, about 8° or 10° away from the sun's place. The points to be chosen *round the sun*, say N., N.E., and E., considering the sun as a map. (It would be well if three observers could be appointed each to take one fixed spot for atmospheric polarisation and to note the changes which take place during totality—both in the plane of atmospheric polarisation and in its intensity.)

2. The intensity of polarisation should be carefully measured with the polarimeter at different points of the corona, the observer taking care to notice when the Savart's bands disappear in the *centre* of his field as before stated.

3. The dark moon and corona should be bisected by the line of junction of the bi-quartz polariscope, and the colours upon the corona should be carefully noted, not only near the line of junction, but also round the whole circumference of the dark moon. Any sudden transition from one colour to another should be especially recorded. Should any ray, or rift, or sector of colour with sharp edges be observed, it would be well to place the line of junction across such sector or rift, and note the colours upon its edges; the telescope carrying with it the line of junction might then be slowly withdrawn along the sector or rift from the limb of the moon outwards until all indications of the rift or its edges are lost—the observer, of course, noting the plane of polarisation within the rift, and whether it differs from that of the air polarisation in the neighbourhood of the sun.

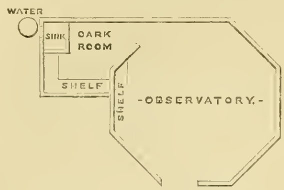
A. C. RANYARD

## *NOTES ON ECLIPSE PHOTOGRAPHY*

ECLIPSES of the sun occurred in 1860, 1868, and 1869, when photography was employed chiefly to obtain evidence as to the nature of the red prominences, and in all cases a telescope of some kind was used, the image being taken at the principal focus.

It is, however, preferable that instead of a telescope an ordinary photographic lens of *long* focus be employed. Such a lens may be a portrait combination, or single or compound lenses, adapted for landscape or copying work; the conditions are that the image shall be as large as possible, and the lens quick-acting. These requirements were found to be combined in the lens I used at Syracuse, and which was made by Mr. Dallmeyer, and lent to me for the purpose of the expedition. The lens is 4in. in aperture, and has a focal length of 30in, the image of the sun or moon being three-tenths of an inch in diameter. It is scarcely to be expected that there will be many lenses of this exceptional class available in India; but no doubt, there are many good landscape lenses of long focus which may be used, and if the images they give are not so large as those taken by the 4-inch "rapid rectilinear," the pictures obtained may have scientific value, although small. It is an interesting fact that at Oran Dr. Huggins had arranged with a local photographer to attempt some pictures with a small lens, giving an image of about $\frac{1}{8}$th of an inch in diameter.

In all cases it is strongly to be recommended that the best instruments be used. Instead of Dallmeyer's 4 inch

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lens, it is suggested that lenses of the "rapid rectilinear" make, of still larger aperture, be used, as the focal length will be increased, and consequently a larger picture will be obtained, allowance, of course, being made for the increased exposure required, if the focal length of the lens has been made greater in proportion to the aperture.

It will be convenient to assume in what I have now to say that a lens of at least four inches aperture and thirty inches focal length will in all cases be used, and that it will be corrected for the chemical rays.

Now an instrument of this kind, if used to photograph the sun's corona, will be useless if not mounted so as to follow the sun's apparent motion. The camera must, therefore, be mounted on a stand having clockwork motion. The stand of an equatorial telescope is what is required. The telescope may be removed, and the camera fixed in its Place—this for convenience only—or the camera may be fixed on the top of the telescope. It will, however, be better to remove the telescope.

In the observatory and dark room as used at Syracuse, the framework was of wood, as slight as possible consistent with stability, and was covered entirely with waterproof cloth, the dark room being lined with yellow calico, in some parts double. The floor was also covered with the waterproof cloth to keep down the dust. Instead of the cloth for the sides and the roof I should prefer very thin boards, and the roof only need be made watertight, the edges of the wood being made, if necessary, to overlap. If wood be used, one thickness of yellow calico will be sufficient. The cloth, if used alone for the sides and roof, is objectionable, owing to the possibility of wind tearing it away from the nails.

The importance of each photographic party being provided with a tent of the kind named cannot be too strongly urged. Residents in India will of course have no difficulty, but observers from England should prepare a tent at the nearest town to the place of observation, or, better still, take one with them. To unpack and erect our observatory and dark room required about a day, and to dismantle and repack it about three hours. To adjust and arrange the instruments will require about another day, and about two hours for the dismantling and packing. Much of our success at Syracuse depended on our being provided as described. The entire cost of the building, including waterproof cloth, yellow calico, and the fittings of the dark room was less than ten pounds. In the erection of a temporary construction of this kind, the shelter of a wall or building should be sought.

As these hints are for the practised photographer, and not for the tyro in the art, very little need be said about the process to be used. No dry process with which I am acquainted is adapted for the purpose. It may be suggested that the old *positive* process on glass would give good results, and I see no objection to using the negative process as well. Supposing six plates to be used, three of them could be developed as positives, and three as negatives, the exposures being timed to suit either. The negatives taken at Syracuse were not strengthened or intensified, and although they were developed as negatives they are almost as valuable as positives, as the detail is of so delicate a kind that very little of the outer corona can be seen when viewed by transmitted light. As positives, and viewed by reflected light, this detail is seen very perfectly. There is, however, detail of another kind in these negatives which can only be seen by transmitted light. The advantage of the positive process is that the picture is chiefly on the surface, but at those parts where the light has been most active, the detail would probably be visible when viewed as negatives by transmitted light.

The size of the camera will be determined by the dimensions of the plates to be used. Plates 5 by $4\frac{1}{2}$ inches will be quite large enough, and the camera will require to be slightly larger. The number of dark slides necessary will depend on the number of exposures to be made, and this again will be determined by the duration of the totality. In India, where the totality will be rather over two minutes, it will perhaps be better not to attempt more than six exposures, therefore six frames will be needed. In Australia at least twelve plates could be exposed, the totality lasting over four minutes. The single frames used at Syracuse were found to be so handy and convenient to use that I am undecided in my opinion as to whether any advantage would be gained by using double or sliding frames. This is a matter which must be left to the choice of the operator. The frames may be made in the ordinary way, but the corners of the carrier or frame itself should be fitted with silver wire or glass for the plate to rest upon; and as an extra precaution against defects likely to arise through some of the plates remaining in the frames half an hour, each corner may rest on blotting paper. In fixing on six as the number of plates to be used, I am assuming that they will all have to be prepared so as to be ready at the moment of totality; and I see no advantage to be gained by keeping an assistant in the dark room during that time; at most he could only prepare one plate, and I consider it preferable that every plate should be ready for immediate use. The preparation of these plates will require twelve minutes at least, and it is at this point and after the exposures are made that extra assistance would be valuable.

It was found at Syracuse that four baths of glass for the silver solution were sufficient; they were covered with brown paper to protect the plates from light. The dippers may be of glass, or varnished wood, and it is better to be provided with both, in case of accident. The glass should be the best patent plate, selected, carefully polished, each one being marked with a cross in one corner, and stored in plate-boxes with the marks all in one position; and in all the subsequent operations this mark should be to the left hand—the reason for this will be seen presently.

The image obtained with a camera as described is small, and it is therefore undesirable that any part of it should be disfigured by position wires. The necessity for using wires may be overcome in a very simple manner. Let the ground glass focussing screen (which should be of the finest possible kind, patent plate glass before it receives its final polish) have pencil lines crossed diagonally, and a single line horizontally across the centre of the plate. When the instrument is adjusted and stationary the image of the sun should travel parallel with this line. A plate should then be prepared, and a very small diaphragm being used in the lens, the instrument should then be moved quickly, so that the image of the sun leaves a trace which will appear after development. This will give the north point of the sun at the time of the eclipse, and serve as a key plate for all the pictures taken. The object of marking the corners of the plates will now be seen, and every plate used must be so held that in every operation the marked corner is touched by the forefinger or thumb of the left hand; then there will be no doubt about the orientation of all the pictures.

Up to this point two operators will be sufficient, one to attend to the arrangement of the instruments, and who ought to have some astronomical knowledge, so as to be able to adjust the equatorial stand, and the other for strictly photographic work. Help will of course be required in the erection of the observatory and dark room. At the time of the eclipse two other assistants will be required, one to count seconds from the clock or chronometer, and the other to hand the plate frames to the assistant at the camera. If a clock beating seconds or half seconds were used, the assistant at the camera could do the counting, but volunteers will readily be found.

The plan adopted at Syracuse could not I think have been improved. Mr. Fryer was at the instrument making the exposures, while I was at the other end of the camera changing the frames. At the word "ready" Mr. Fryer took off the cap and counted the prescribed number of seconds. At his signal "done" the plate was changed, and so on through all the exposures. Captain Speight handed to me the frames and took them from me after exposure, thus saving twenty-five seconds of the time. Sapper Gardiner counted seconds aloud. At the signal of totality given by Mr. Fryer the counting commenced. At the third second the first exposure was made, the three seconds being allowed to make sure of absolute totality. A table of the times for the exposure of each plate had been prepared by Mr. Fryer beforehand, and this was kept in view so as to avoid the possibility of mistake. The times were arranged in the following order:—3, 18, 30, 15, 8 (the 6th plate was exposed in the telescope camera, with three or four seconds to spare at the end). In India there will be time for the 6th plate to be exposed in the camera. On the day before and on the morning of the eclipse all the operators should be in their places to practise their different parts, as it will require the greatest possible care to avoid mistakes. Everything must be done deliberately and without the slightest hurry.

Previously to the last eclipse it had been supposed that the light of the Corona possessed very little actinism, and I had been strongly advised to give a full exposure. As, however, there was some doubt on the subject, it was determined to vary the exposures as stated; and unless the light of the Corona is not the same in all eclipses, I see no reason for suggesting any alteration in the time for exposing the plates to obtain different results, supposing the same kind of lens to be used. Allowance must of course be made for the altitude of the sun and the state of the weather at the various stations.

The dark frames were numbered from 1 to 6, and the plates were exposed in the order in which they had been prepared, and the development proceeded in the same order.

Much of the delicate detail of the negatives is likely to be lost by varnishing. It is therefore preferable to cover them with glass, carefully binding the edges to exclude the air; the glass cover should not touch the film. Instead of using an ordinary plate-box for the negatives, I prefer that each plate should have a slight frame similar to an ordinary "carrier," and these frames are then placed flat on a box prepared for them.

In counting seconds it is preferable that the assistant should count consecutively throughout the totality. Supposing the eclipse to last 130 seconds, by counting from 1 to 130 the operators know exactly the point arrived at after each exposure; and this is most important towards the end, as the last plate might be spoiled by the least mistake in this respect.

Artificial light of some kind will probably be required during the totality—certainly in the dark-room. We used the ordinary railway reading lamps. Boxes open on one side were provided, and in them the lamps were fixed. These boxes effectually protected the lights—without them the candles would have been extinguished by the wind.

It is better to assume that nothing will be found at the place of observation but water, but as in India there may be some difficulty in obtaining that necessary article sufficiently pure for photographic purposes, it will be better to provide a small still, which will cost about 5*s*. At Syracuse we used rain-water, which was sufficiently pure for the purpose.

Those accustomed to photographic work in India will be aware of the necessary precautions to be observed to prevent the plates drying. At Syracuse we kept our observatory and dark-room well sprinkled with water; and the glass plates, when they were in the dark frames, were covered on the backs with wet blotting-paper.

Much disappointment will be avoided if proper care be taken in packing the apparatus. All bottles and other glass articles should be placed in separate divisions and packed with cotton wool or paper cuttings. Packing-cases should be made very strong and bound with iron plates. By attention to these matters the whole of the apparatus and chemicals were found on being unpacked at Syracuse to be altogether uninjured—the packing cases bear testimony to the rough usage they have undergone.

A. BROTHERS

## *CLIFTON COLLEGE SCHOOL OF NATURAL SCIENCE*

WE have long insisted in NATURE on the extreme importance of science teaching in the higher grade schools in this country, and we are glad to find that at length its importance has begun to be recognised by the head masters themselves; so that, on the whole, the progress now being made in this direction is such that we may confidently expect that at no very distant future science instruction will be provided for in all our superior schools. Foremost, if not positively the first among the schools in which the sciences are thus taught stands Clifton College, under the able direction of the Rev. J. Percival, in which scientific study is introduced to the utmost, and keenly pursued by the boys, with the encouragement of all their masters, the latter a most important consideration, and which, we are sorry to say, we cannot assert in reference to other schools of equal pretensions. There are several points of interest about the method of teaching at Clifton, and we are glad to have the opportunity of laying before our readers a sketch of the way in which the work there is carried on, together with a sketch of the museum, which, may well become the model of all school museums. Science is much indebted to Mr. Percival for the magnificent example he has set in science education.

Natural Science at this College is not a voluntary subject, but forms a regular part of ordinary school work. The boys in the two highest classes on the classical side are allowed to choose between Science and German; throughout the rest of the school some branch of science is compulsory. In the Junior School Botany is taught, in the Upper School Chemistry and Physics. The boys on the classical side receive one lecture, those on the modern side two lectures a week on each of these latter subjects. The lectures are illustrated by experiments, accurate notes are exacted from the boys, and examinations are held every fortnight or three weeks.

The accompanying is one of these fortnightly papers:—

MAGNETISM

1. Soft iron can never be permanently magnetised, yet a piece of soft iron in contact with a magnet becomes a magnet. Why?
2. What do you understand by *coercive force*, and magnetic *saturation*?
3. How is magnetism influenced by heat?
4. Mention the substances which are attracted by a magnet in addition to iron.
5. State one or more of the methods by which steel bars may be magnetised.
6. What is the *declination* or *variation* of the magnetic needle, and the present extent of it?

CHEMISTRY

*a.* For First and Second Sets, Modern Side only.

1. Mention the oxygen compounds of phosphorus, and the action of water upon them.
2. Give an account of arsenicum and its chief characteristics.
3. What are the constituents and characteristics of arseniuretted hydrogen?
4. What is "white arsenic," and how may it be prepared?
5. You are given a liquid suspected to contain arsenic; by what means would you examine it?

*b.* For all other Forms.

1. Ammonia gas, and hydrochloric acid gas, are brought into contact: what is the resulting compound, and to what may it be compared?
2. What is *ammonium*? Describe the formation and appearance of ammonium amalgam.
3. What do you know of chloride of nitrogen?
4. What is nitric acid, and by what means may it be procured?
5. State the action of nitric acid upon metals,—copper, tin, antimony,—and the general tendency of the acid.

BOTANY

Third A. and B. only. (To be written on separate paper only).

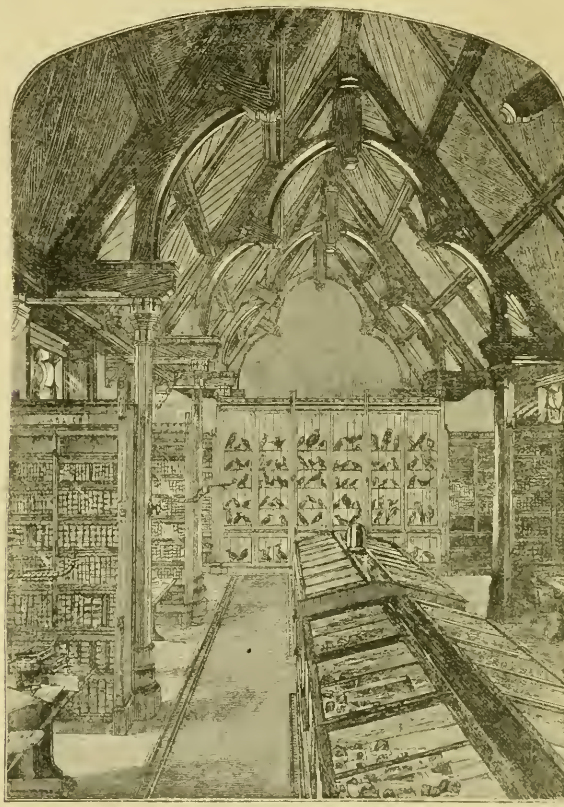
1. Describe the following forms of roots:—*tap, napiform, premorse, tubercular.*
2. How are fluids absorbed by the roots?
3. Show clearly the true nature of the various forms of the bulb.
4. What is a "rhizome" (or root-stock)? Compare it with a "corm."
5. Give an account of the structure of the stem in a common potato.
6. Why is it that plants and animals have a mutual dependence on each other for their life?

Special classes are formed for those who wish to go deeper into these subjects, or to take up others. Thus, there are special classes studying Chemistry, Physics, Zoology, Physiology, Botany, Physical Geography, and Civil Engineering.

Facilities are also afforded for learning science practically. In the Chemical Laboratory about twenty boys study analysis. A Physical Laboratory has been built and will be opened next September. It will accommodate about twenty pupils, and its arrangements will be based on those adopted at King's College, London. A large workshop fitted up with carpenters' benches, vices, and lathes has been opened this term, and is exceedingly popular. There is also a physiological laboratory, in which a few of the elder boys receive instruction in Practical Zoology and Physiology.

Marks are given for work done in all the above-mentioned classes, except in the workshop. These marks affect a boy's position in his form from week to week, and thus afford a strong incentive to careful work.

Five masters are at present engaged in teaching science. The taste for natural history is developed by means of the Natural History Society, the School Museum, and the Botanic Garden. The society consists of about seventy members. Its meetings are held once a fortnight, they are fully attended, and there is never any lack of papers to be read. The society is subdivided into sections, which hold special meetings and make excursions for the study of different branches of science. The first number of the Transactions has just been published. The sections are engaged in preparing lists of the fauna, flora, and mineralogy of the district. The whole society makes an expe-



THE MUSEUM OF CLIFTON COLLEGE.

dition annually in July to some place of interest in the neighbourhood.

A conversazione was given last month to celebrate the opening of the Museum and Botanic Garden. The former shares with the library the new room recently added to the school buildings by the head-master. Space being, of course, limited, it has been decided to make the collections essentially British; the local series to be as far as possible complete, and the general collections typical. Through the liberality of friends, considerable progress has been made already. Thus the museum already contains a fine series of British plants, another fine series of typical British fossils, nearly 1,000 specimens of minerals, the same number of British Lepidoptera, many British birds and their eggs, and a good typical collection of shells. The completion of the local series is left in the hands of the different sections.

The Botanic Garden is large, and is laid out in long narrow beds with grass walks between. Over 1,000 hardy herbaceous plants are here arranged according to the natural system; and new additions are being constantly made. There are also specimens of all the ornamental trees and shrubs commonly cultivated in England; and a rockery has been built for Alpine plants.

## THE BRITISH ASSOCIATION MEETING AT EDINBURGH

### SECTION A.

*On Temperative Equilibrium of an Enclosure in which there is a Body in Visible Motion*, by Prof. Balfour Stewart, F.R.S.—It is now several years since Prof. Tait and the author of this paper came jointly to entertain the belief that there is some transmutation of energy, the exact nature of which is unknown, when large bodies approach and recede from one another. It is desirable to vindicate an idea of this nature, both from the theoretical and the practical point of view—that is to say, we ought, if possible, to exhibit it as a possible deduction from those laws of nature with which we are already acquainted; and, on the other hand, it ought to be supported by observations and experiments of a new kind. In our case the experiments and observations have been of a difficult nature, and are yet in progress, it is therefore premature to bring them before the notice of this section. A theoretical vindication of the idea has been obtained by Prof. Tait, and more recently one has occurred to the author of these remarks, which he now ventures to bring before the section. Men of science are now sufficiently well acquainted with Prevost's theory of exchanges and its recent extension. We know that in an enclosure, the walls of which are kept at a constant temperature, every substance will ultimately attain the very same temperature as these walls, and we also know that this temperature-equilibrium can only be brought about by the absorption of every particle being exactly equal to its radiation, an equality which must separately hold for every individual kind of heat which the enclosure radiates. This theoretical conclusion is supported by numerous experiments, and one of its most important applications has been the analysis of the heavenly bodies by means of the spectroscope. Let us now suppose that in such an enclosure we have a body in visible motion, its temperature, however, being precisely the same as that of the walls of the enclosure. Had the body been at rest, we know from the theory of exchanges that there would have been a perfect equilibrium of temperature between the enclosure and the body; but there is reason to believe that this state of temperature-equilibrium is broken by the motion of the body. For we know both from theory and experiment that if a body, such for instance as a star, be either rapidly approaching the eye of an observer or receding from it, the rays of the body which strike the eye will no longer be precisely the same as would have struck it had the body been at the same temperature and at rest; just as the whistle of a railway engine rapidly approaching an observer will have to him a different note from that which it would have had if the engine had been at rest. The body in motion in the enclosure is not therefore giving the enclosure those precise rays which it would have given it had it been at the same temperature and at rest; on the other hand, the rays which are leaving the enclosure are unaltered. The enclosure is therefore receiving one set of rays and giving out another, the consequence of which will be a want of temperature-equilibrium in the enclosure—in other words, all the various particles of the enclosure will not be of the same temperature. Now, what is the consequence of this? The consequence will be that we can use these particles of different temperature so as to transmute part of their heat into the energy of visible motion, just as we do in a steam engine; and if it is allowable to suppose that during this process the moving body has retained all its energy of motion, the result will be an increase of the amount of visible energy within the enclosure, all the particles of which were originally of the same temperature. But Sir W. Thomson has shown us that this is impossible; in other words, we cannot imagine an increase of the visible energy of such an enclosure unless we acknowledge the possibility of a perpetual motion. It is not, therefore, allowable to suppose that in such an enclosure the moving body continues to retain all its energy of motion, and consequently such a body will have its energy of motion gradually stopped. Evidently in this argument the use of the enclosure has been to enable us to deduce one proof from the known laws of heat and energy, and we may alter the shape of the body without affecting the result; in other words, we should expect some loss of visible energy in the case of cosmical bodies approaching or receding from one another.

*Observations on Water in Frost rising against Gravity, rather than Freezing in the Pores of Moist Earth*, by Prof. James Thomson, LL.D., of Belfast. In this paper Prof. Thomson, in continuation of a subject which he had brought before the British Association at the Cambridge Meeting in 1862 on the Disintegration of Stones exposed to Atmospheric Influences, adduced some remarkable instances which he had since carefully observed. In one of these observed by him in February 1864, he showed that water from a pond in a garden had in time of frost raised itself to heights of from four to six inches above the water surface level of the pond by permeating the earth bank, formed of decomposed granite, which it kept thoroughly wet, and out of the upper surface of which it was made to ascend by the frost, so as to freeze as continuous columns of transparent ice rather than that it would freeze in the earth pores. From day to day during the frost the earth remained unfrozen, while a thick slab of columnar ice formed itself by new water coming up from the pond, and insinuating itself forcibly under the bases of the ice columns so as to freeze there, pushing them up, not by hydraulic pressure, but on principles which, while seeming to have been previously not noticed, appear to involve considerations of scientific interest, and to afford scope for further experimental and theoretical researches.

### SECTION B.

A REPORT *On the Publication of Abstracts of Chemical Papers* was read by the secretary (Dr. Thorpe). The committee, which consisted of Profs. Williamson, Roscoe, and Frankland, having charge of the matter, said they were glad to be able to announce that regular monthly reports of the progress of chemistry have been published since April last by the Chemical Society. These reports have been rendered as far as possible complete, by giving abstracts, more or less full, of all papers of scientific interest, and of the more important papers relating to applied chemistry. The abstracts have been made by chemists, most of whom are members of the Society, whose zeal for science has induced them to undertake the work for the small honorarium which the Council has been able to offer. A numerous Committee of Publication has been formed, whose members gratuitously undertake the revision of proofs, and a comparison of abstracts with the original papers. The committee feel that their thanks are due to those gentlemen engaged in the work for having already so far succeeded in accomplishing a task of such difficulty and importance, and they confidently hope that their continued exertions will still further perfect the details of the scheme, so as gradually to increase the usefulness of the report. It is right to state that the funds of the Chemical Society, available for the purpose of the report, although so opportunely aided by a grant of 100*l.* from the British Association, were insufficient to defray the necessary expenses, and that voluntary contributions to the amount of upwards of 200*l.* have been received towards the cost of publication for the first year up to April 1872. There is good reason to believe that the expectations entertained of the usefulness of these reports will be fully realised by their continuance on the present system; and that they will be found largely to conduce to the progress of the science wherever the English language is spoken.

Prof. Williamson said it had long been felt in England that some equivalent was needed for those admirable annual reports which have long been published in Germany, and of which the value was so very great to workers in chemistry. To meet that want was the object the committee had in view.

A vote of thanks was given Prof. Williamson for his exertions in connection with the matter.

Dr. Thorpe read a paper *On Phosphorus Chlorides*. He said he had attempted to prepare the missing oxichlorides analogous to those obtained from vanadium by Roscoe, but without success. When the phosphoryl trichloride is heated with zinc in a sealed tube, the oxygen is withdrawn and phosphoric chloride is obtained. He had also prepared sulpho-chloride of phosphorus by the action of sulphide of phosphorus on the penta-chloride of phosphorus.

Mr. Pattison Muir made a communication *On an Antimony Ore from the Thames, New Zealand*. The specimen analysed was beautifully crystallised and almost chemically pure antimony sulphide, containing only traces of arsenic and antimony. Mr. John Dalzell communicated a paper *On Suphur Dichloride*. He has repeated Hübner's experiments, and finds that the compound actually exists at low temperatures. Dr. Wright gave a *résumé* of his researches *On the Derivatives from Codeia*. An account of these investigations has already appeared in our columns. Mr. Tichborne read a paper *On the Dissociation of Molecules by Heat*, and showed some very pretty lecture experiments on the subject. Mr. J. G. Buchanan read a paper illustrated by diagrams *On the rate of Action of Caustic Soda on a Watery Solution of*

*Chloracetic Acid.* He has determined the rate at which chloracetic acid suffers decomposition, when heated simply with water or with caustic soda in a sealed tube. The following papers were also read :—Prof. Apjohn, *Some Remarks on the Proximate Analysis of Saccharine Matters* ; Dr. Gladstone, *On Crystals of Silver* ; Mr. Braham, *On the Crystallisation of Metals by Electricity* ; Mr. J. S. Holden, *On the Aluminous Iron Ores of Co. Antrim* : Prof. Maskelyne, *On Dufrenite and a New Mineral from Cornwall, and on Localities of Dioptase* ; Rev. Mr. Highton, *On a Method of Preserving Food by Muriatic Acid* ; Mr. Wanklyn, *On the Constitution of Salts* ; Mr. Harkness, *On a Method of Testing Wood Naphtha.*

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## SECTION C.

MR. CARRUTHERS, F.R.S., read a paper by Mr. Grieve *On the Position of Organic Remains near Burntisland*, and also a paper by himself *On the Vegetable Contents of Masses of Limestone occurring in Trappean Rocks in Fifeshire*, and the conditions under which they are preserved. Large masses of plants which formed the coal had been enclosed in the trappean ash, and subsequently calcified by the large amount of lime contained in the rocks. Mr. Carruthers considered that these fragments were enclosed in a peaty condition, because the mass was penetrated in every direction by roots, showing the existence of vegetation on this soil. The attention of Mr. Grieve was first directed to the specimens by observing on the shore large masses of limestone which had been polished by the drifting sand. The action of this sand was well shown in the neighbourhood, even the hard basaltic rocks having been polished by it. Mr. T. M'K. Hughes said that after what had been brought out in regard to the action of the drifting sand, they must take care not to attribute the polishing of rocks in every instance to glacier action.

The second meeting of Section C. was opened by Mr. Pengelly, who read the *Seventh Report on the Kent's Cavern Explorations.* His clear and lively lecture drew together a good audience. Commencing with some general remarks on the history and working of the Cavern, in order to make the subject clear, he pointed out the usual section to be, in descending order :—1. Black mould, containing many objects of recent date, and some of Romano-British times; also remains of animals still living, or which lived in historic times. 2. Granular stalagmite, containing remains of extinct animals, and also a human jaw. 3. Cave earth, yielding a harvest of extinct remains, also flint implements. 4. Crystalline Stalagmitic floor, and Breccia formed of rocks from distant hills; bears only have been obtained from these. He then described the work done during the past twelve months, showing what new passages had been opened, and the number of species which had been obtained. They included hyæna, horse, rhinoceros, Irish elk, ox, deer, badger, elephant, bear, fox, lion, reindeer, rabbit, bat, wolf, dog, &c. Many of the bones were gnawed by hyæna, others were marked by rootlets encircling them. Altogether, about 2,200 teeth and bones and 366 flint implements and flakes had been obtained since the last year's Report was read.

*The Contents of a Hyæna's Den on the Great Doward, Whitchurch, Ross, Herefordshire*, were pointed out by the Rev. W. S. Symonds, F.G.S. He remarked that the section of the deposits was—1. Superficial soil and stalactitic matter with Roman (?) pottery and human bones. 2. Thin band of stalactitic matter. 3. Cave earth, containing flint flakes and chips, stone instruments, teeth and bones of numerous mammals either extinct or not now inhabiting the district, as the cave lion, cave bear, hyæna, mammoth, long-haired rhinoceros, fossil horse, &c. 4. Stratified sand and silt, with rolled pebbles. 5. Thick floor of stalagmite, and cave earth separated every few feet by layers of stalagmite, containing flint flakes.

Mr. Vivian, referring to the length of time during which man had existed on the earth, thought he might have existed for about a million years. Prof. Hull remarked that there was no evidence as yet to bring back man to the Glacial epoch, and therefore opinions about the high antiquity of man should be reserved. Mr. Prestwich concurred with Mr. Hull, but said there was no doubt that man followed very closely upon the Glacial period.

Mr. L. C. Miall read a paper *On Some Further Experiment and Remarks on the Contortion of Rocks*, describing results obtained by subjecting limestone, flagstone, slate, and plaster of Paris to forces of low intensity but of long continuance. Mountain and magnesian limestone proved to be indefinitely plastic ; slate slightly elastic, but almost incapable of permanent deflection. Remarks on some cases of superficial and modern contortions were appended to the paper.

Prof. Hull and Mr. W. A. Traill, B.A., of the Geological Survey of Ireland, read a paper *On the Relative Ages of the Granite, Plutonic, and Volcanic Rocks of the Mourne Mountains, Down, Ireland.* They first pointed out the presence of two varieties of granite, differing, as Prof. Haughton had shown, both in composition and origin ; the soda granite of Slieve Croob (consisting of quartz, orthoclase, albite, and mica) being of metamorphic origin, and the potash granite of Mourne (consisting of quartz, orthoclase, albite, and mica) being eruptive. The relative, and as far as possible, the actual ages of these granites, remained to be determined, which the authors considered might be determined by a consideration of the basaltic and felstone-porphyry dykes, by which the district had on several occasions been invaded. The conclusions thus derived were that the granite of Mourne was newer than that of Slieve Croob by a long interval, and that while the former was probably Mesozoic, the latter was of Palæozoic age.

The third meeting of the Geological Section was held on Saturday August 5. The first paper read was by the Rev. Dr. Hume *On the Coal Beds of Panama*, in reference mainly to their economic importance. The author drew attention to the discovery of a series of seams in the Isthmus of Panama. Analysis proved the coal to contain about 75 per cent. of carbonaceous matter, the remaining portion being water and ash ; it had a fair heating and a large illuminating power. There are four points where the coal reaches the surface of the thickness of 9ft., 12ft., and with intervening streaks of shale and clay 25ft. ; it, however, improves in value at greater depths. He pointed out the great importance of this coal, in the event of a canal being made through the Isthmus.

The relation of health to certain geological formations was treated of by Dr. Moffatt. He remarked that the district in which he lived consisted geologically of the Carboniferous and of the New Red Sandstone system ; that the inhabitants of the former were engaged in mining and agriculture, and those of the latter in agriculture chiefly. Anæmia, with goitre, was very prevalent among those persons living on the Carboniferous system, while it was almost unknown among those on the New Red Sandstone ; and phthisis was also more prevalent among the former than the latter. He then gave some statistics as to the diseases prevalent among the counties of Chester, Flint, and Denbigh, and stated that the practical deductions to be drawn from the inquiry were, that all young persons living on a Carboniferous formation having symptoms of incipient goitre and anæmia, ought to be moved to a soil upon red sandstone, and persons of strumous habit ought to reside upon sandstone at an elevation of at least 800 or 1,000 feet above the sea. In the discussion which followed the reading of this paper Mr. G. A. Labour mentioned a Carboniferous district in Northumberland containing a thin bed of limestone where the people suffered from goitre. Sir Richard Griffith remarked that goitre was unknown in Ireland, although they had plenty of Carboniferous rocks. Professor Hall agreed with Dr. Moffat respecting the healthful character of the New Red Sandstone.

A paper was then read by the Rev. J. F. Blake *On the Yorkshire Lias and the Distribution of its Ammonites.*

Some relics of the Carboniferous and other old land surfaces were described by Mr. Henry Woodward.

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## SECTION D.

THE Committee for the Close Time for Birds, reported by the Rev. Canon Tristram, LL.D., that it had gone on year after year endeavouring, as well as it could, to influence public opinion on the question of the preservation of indigenous life in this country. At the time of its appointment there was no protection whatever for any creature not coming under the Game-laws. Anything not game was treated by law as vermin. A curious case had arisen in regard to Pallas's sand grouse. That bird made its appearance on the east coast of England, and if it had been allowed to breed on the sand-pits of Durham, Yorkshire, and Lincolnshire, no doubt it might have become an indigenous bird. He (Dr. Tristram) summoned some people for shooting it out of season in the spring of the year ; but it was decided that, being sand grouse and not Scotch grouse, it was beyond the benefit of the laws. The committee had to congratulate the Association two years ago on having succeeded with very little difficulty in steering a Bill safely through all the perils of Select Committees of both Houses of Parliament. That Bill, however, was shorn of its fair proportions; and although it went into the House a Bill for the Protection of Indigenous Animals, it came out an Act for the Preservation of Sea-Fowl. The sea-fowl had borne their testimony to the success of the Act so far, and it was something to have to say that within the last year the numbers of sea-fowl that had bred on the Yorkshire coast were, at least, three times as many as they were two years ago. That success was a great benefit, at the same time, to those who made their living by sea-fowl, because purveyors of feathers and eggs had found that the Sea-Fowl Act had actually very largely increased not only their profits, but their supply, in the same way as the improvement of the Salmon Acts had restored the salmon to rivers from which it had been almost extirpated. The committee, therefore, finding there was a unanimous verdict in favour of the Act regarding sea-fowl, strongly recommended the Association to endeavour to extend the Act in two ways. This they proposed to do next session by introducing amending clauses. One object to be aimed at was to extend the Act to all wading birds and all web-footed birds good for human food. It was desirable to protect the sandpipers, the plovers, the lapwings, and the whole of the duck tribe, which were being rapidly exterminated. Having succeeded in that, the committee should next endeavour to have British law on the subject assimilated to the sternly restrictive laws of every other civilised country, except Holland, Greece, and Turkey—those three being the only countries in the world professing to be civilised which had not a close-time for all creatures.

ORNITHOLOGY.—Prof. Duns, D.D., New College, Edinburgh, read a paper *On the Rarer Raptorial Birds of Scotland;* the four following propositions were stated:—1. That species occur in pairs, often at long intervals, in localities where they have long since ceased to breed, but where they have been at one time not uncommon. 2. The geographical range of stragglers seems to widen with the lapse of time. 3. Certain species have greatly increased in recent times over wide districts where they were comparatively rare. 4. Year by year the raptorial birds of Scotland are becoming fewer. These positions were all treated of in the paper, which, not giving specific characteristics or descriptive details, yet pointed out all the chief sources of information and enumerated all the localities. R. Sibbald's list in "Scotia Illustrata," 1684, and the many that intervened between it and the author's own lists collected during the last thirty years, were all referred to, and the conclusion come to was that most of the larger raptorial birds were rapidly disappearing from Scotland, and that even the smaller forms which were very common in the southern and central districts were yearly becoming rarer. The author also expressed his belief that both the farmer and the game preserver would lose much when between them they succeeded in destroying all the hawks and owls.

ICTHYOLOGY.—A paper was communicated by Colonel Playfair, H.B.M. Consul-General at Algiers, *On the Hydrographical System and the Fresh Water Fish of Algeria.* After describing certain interesting features in the physical configuration of the country, the paper went on to state that in the rivers flowing to the Mediterranean there were sixteen species of fish, only three of which were common to the whole region, one being the common eel. There were eleven species peculiar to the littoral of Algeria, among which was a small trout. The common gold fish, which was very common, was not a native of Algeria, but was supposed to have been introduced by the caprice of a certain Sultan many centuries ago. It was now, however, universal in the streams. The plateau had only afforded seven species, one of them being the same as a South African species. In the Sahara there were some peculiar species. The upper part afforded two species, one being the common eel, and in the lower region two species were found in the salt lakes, and had been frequently ejected by the Artesian wells. It had been concluded that these latter species inhabited a vast subterranean sea occupying the bottom of the Sahara depression. The question had been asked why they were not destitute of eyes, but it was to be remembered that their underground life was simply an episode in the voyages they made between one well and another. When they reached a well they were either forced up or by instinct came to the surface. Owing to the shortness of the rivers and their being extremely rapid in their upper portion, the physical conditions were not such as would admit of the introduction into them of the true salmon with any prospect of success.

Mr. C. W. Peach exhibited some apparently tailless trout which had been sent to him by Mr. Colin Hay, distiller, of Ardbeg, Islay. They were taken in Lochmaorichen, in Islay. That loch was about 1,000 feet above the level of the sea, and not above one acre in extent. It was so shallow that a man could wade through it, and had a stony bottom, with a few weeds. Although it was surrounded by other lochs, these tailless trout were found only in it. The whole of them were "docked," and Mr. Mackay, a keen sportsman, who has fished it often for thirty years, never caught one with a perfect tail. They are in excellent condition, being fed on the small crustaceans which are abundant in the loch. Mr. Peach further stated that Mr. Hay was about to add to his kindness by procuring a further supply of fish, if possible, from the fry to the adult state. He also intended to transport some of the "docked" trout to a loch at a short distance, in which trout had never been taken, and try to rear a stock from them, and see whether they would all remain "tailless.

Dr. Grierson said that, at the mines of Wanloch-Head, Dumfriesshire, and Leadhills, Lanarkshire, there were streams coming from the shafts in which trout without tails were frequently got, as also trout with deficient fins. The fish referred to were, moreover, frequently blind. Specimens of these fish were to be forwarded to Professors Turner, Traquair, and Dr. Günther for examination.

Mr. A. G. More exhibited some brown trout taken in salt water. It was not, he thought, generally known that the common or brown trout of fresh-water streams was an occasional visitant to the salt water. The salmon and the sea-trout, and the sewin or Welsh sea-trout, descended regularly to the sea after they had finished breeding in fresh water; but the common brown trout had seldom been observed under the same circumstances. In Scotland Mr. Peach, who had an extensive experience and knowledge of marine zoology, assured him that no instance of the kind had come under his notice, save once, when he found a river-trout in the stomach of a cod-fish. Possibly that trout was captured in salt water, but it might have been dropped by a cormorant, or have been swept down the river in a flood either weak or possibly already dead. In the west of Ireland—in the counties of Donegal, Sligo, Limerick, and Kerry, Mr. More had ascertained, partly through others and partly from his own observation, that the river-trout spontaneously frequented the salt water at the mouths of the rivers. The brown trout captured in salt water differed from their usual condition in having brighter and more silvery scales, something like those of the young salmon in the smolt condition. Mr. More would like it to be ascertained if these trout were brown trout "pure and simple," or hybrids.

Prof. Duns exhibited a specimen of the spiny shark, *Echinorhinus spinosus*, Blain, which had been taken at Earlsferry, near Elie, Fifeshire, in the February of this year. He also mentioned that a specimen had also been taken in January 1867 near Boness, Linlithgowshire.

Dr. C. Lütken described a new genus of fish belonging to the family of the sea-devils, allied to, and, in fact, almost intermediate between the curious genus Melanocetus discovered some years since by Mr. Johnson at Madeira and the monstrous Ceratias, which, until the discovery of Mr. Johnson, was the best known example of the Apodal Lophioids. Of the third genus of the almost blind apodal deep sea Lophioids, it was strange that the Greenland seas should have already possessed a species, *O. himantolophius*, described many years ago by the senior Reinhardt from a mutilated specimen, but which description had been almost forgotten by recent icthyologists. Among the characters distinguishing this genus Oneirodes, there is one both peculiar and suggestive, viz., the curious development of the head of the first dorsal fin-ray, which, with its tentacles, pigmental spots, &c., gave the impression of, as it were, a mimicry of the head, say, of a Nereis. It would not be very wonderful if it were really intended to allure other rapacious fishes, and if the old stories of the angling propensities of the "fishing frog" were found to contain more truth than is generally believed. The new species *O. eschrichtii* was taken at Greenland.

ENTOMOLOGY.—Mr. Roland Trimen, F.L.S., F.Z.S., read a note on a curious South African grasshopper,* *Trachypetra bufo*,

* Methuen's "Wanderings in the Wilderness," 2nd edition, 1848, App. p. 372, pl. II., fig. 3.

White, which mimics with much precision the appearance of the stones among which it lives.

He commenced by observing that some tendency existed to separate too widely those cases of mimicry where one animal imitated another from those in which an animal closely resembled either some part of a plant or some inorganic object; and expressed the opinion that these two sets of cases were wholly one in kind, the evident object in all being the protection of the imitator.

Describing a visit paid to the vicinity of Grahamstown in search of this insect, he observed that it was a work of considerable difficulty to distinguish the grasshoppers from the stones, and he was engaged for half an hour in careful search over a known station of the species before discovering an example. He noted the further most interesting fact, that, in certain spots (often only a few square yards in extent) where the stones lying on the ground were darker, lighter, or more mottled than those generally prevalent, the Trachypetra found among such stones varied similarly from the ordinary dull ferruginous-brown colouring in imitation of them.

It was pointed out that the close imitation of the stones was mainly effected by the modification of the dorsal shield of the prothorax, which is, with the whole thorax, much flattened and widened, and is further much produced posteriorly, and has its surface roughened or granulated in close resemblance to the surface of the stones.

In conclusion, he called attention to the bearing of the case of this insect on the question of the origin of species; and in putting the alternative whether the peculiar station of the Trachypetra had been specially prepared for it immediately before or simultaneously with the creation of the insect, or whether, on the contrary, the insect had been very gradually modified by natural selection in imitation of the stones for the purpose of concealment, he expressed his decided opinion in favour of the latter hypothesis.

Specimens of the insect were exhibited in association with some of the stones among which they were captured, and the very close resemblance between stones and insects excited general remark. Mr. Trimen observed that in nature the mimicry was more effective, the colours of the dead insects having faded considerably, and the shrinking of the abdomen having caused the hind-legs to be much more apparent than was the case in living examples.

*Echinoderms.*—Prof. Wyville Thomson read a paper *On the Structure of the Crinoids*, to which it would be impossible to do justice in a brief summary. He proposed to make as primary divisions of the family the Astomata and Peristomata. Dr. Lütken of Copenhagen remarked on the great interest of the paper, and referred to Prof. Wyville Thomson's earlier and excellent memoirs on the development of a species belonging to this family. In a paper *On the Palæontological Relations of the Fauna of the North Atlantic*, Prof. Wyville Thomson exhibited and described a remarkable new genus possibly related to the Diademidæ, in the corona of which the plates overlapped, and which, when taken out of the dredge, rolled about like a soft egg; this was called *Calveria hystrix*. The Pedicellariæ were most beautiful objects, and the species is one of the most remarkable of all living Echinoids. A beautiful recent species called *Purpuratus* of the genus *Porocidaris* was also exhibited, as also specimens of *Brissinga*, *Pourtalesia*, and *Rhizocrinus*. A choicer assemblage of rare and remarkable forms was probably never before exhibited to Section D, and it is not possible to refrain from mentioning that most of them will be described and figured in an early number of the Proceedings of the Zoological Society of London.

*Cœlenterates.*—Dr. Charles Lütken of Copenhagen, in introducing to the notice of the department a recent addition to the fauna of the Arctic region, said they would know that the progress of modern science had given an increased interest and importance to the knowledge of Arctic forms. Naturalists were now busily engaged in looking for the remains of the vegetable and animal kingdoms left in the sedimentary deposits from the glacial epoch in which an immense ice-field had covered a great part of the earth. One of the latest discoveries in Scandinavia was that of a fresh-water deposit at the bottom of a great bog, containing the relics of a truly Siberian vegetation. On the other hand, recent investigations, for which they were in part indebted to the British Government and British naturalists, had shown that many of the lower animals, hitherto thought only to inhabit the Arctic Seas, had a very great geographical distribution. For a long time the seas of Greenland had been one of the principal sources of our knowledge of Arctic life. It was about the only country, with the exception of the most northern part of Norway, within the Arctic Zone, where there was established a regular colony with a staff of officials, among whom there was always to be found one or more who were anxious to make their situation profitable to science, and the directors and officers of the Museum at Copenhagen always encouraged these efforts with the view of collecting at Copenhagen as ample material as possible for the study of Arctic life. These efforts have been in later times rivalled by those of the Swedish Government, but their own efforts were greatly promoted by the circumcumstance that the profits of the colonisation of Greenland were derived almost solely from the revenues got from the rich animal life, and that the Esquimaux were very acute observers of that nature from which they also derived their whole sustenance. He now submitted to the notice of the department a new species of Antipathes (*A. arctica*) found lately in the stomach of a Greenland shark; it belonged to a tribe of corals hitherto believed to be exclusively inhabitants of the warmer seas, not being previously found north of the Mediterranean or South Carolina. He was now informed by Prof. Wyville Thomson that species of that genus did come to the surface during his late dredging expedition in the North Atlantic. This discovery, in addition to that of the Lophioid fish described above, indicated that the treasures of the Arctic Seas were not yet exhausted, and ought to stimulate further attention to them. Prof. Wyville Thomson and others took this occasion to state their admiration at the perfect order and care with which the Scandinavian Museums were kept, and their estimation of the great kindness shown by the officers of these Museums to naturalists in this country in sending over for examination complete series of different forms of Arctic life.

*Dredging.*—Mr. W. Saville Kent sent an account of the zoological results of the 1870 dredging expedition of the yacht *Norna* off the coasts of Spain and Portugal.

Rev. R. B. Watson gave a very graphic account of the trials and troubles he had encountered in dredging at Madeira, and appended to his paper a list of the mollusca met with by him in Madeira.

Mr. A. G. More also submitted to the department some account of a recent dredging expedition which he had made to Bantry and Kenmare Bays.

### SUB-SECTION.—ANTHROPOLOGY.

In the anthropological department on Monday, August 7, Prof. Turner presided, and there was again crowded attendance throughout the day. The first paper was read by Mr. J. S. Phené *On the Manners and Customs of the Early Inhabitants of Britain, deduced from the remains of their Towns and Villages.* He drew attention to two prominent points, the universality of the circle, curve, or oval, in all the earliest British remains; and the similarity of the physics of the various localities where British remains are still traceable, arguing that though divided into tribes, yet the inhabitants at the time were one people. In alluding to the physical features of their settlements, he pointed out that a conical hill towards the east, with a stream between it and the settlement, seemed an indispensable condition in selecting a place of abode, and where hills did not naturally exist they had been formed with great labour, as the Castle Hill at Cambridge. He assumed the object of proximity of the hill was for facility of worship, and the separation by the stream was indicative of purity of sacred separation. He believed that our great cities had been founded on these places, chosen by our so-called barbarian ancestors, and quoted Edinburgh, with Arthur's Seat as the place of worship, and Holyrood as the site of habitation, in illustration of his views.

Mr. Phené also read a paper *On an Expedition for the special Investigation of the Hebrides and West Highlands in search for Evidences of ancient Serpent Worship*, and assigning to this worship the shape of many mounds he had examined in Scotland. This paper caused an animated discussion, in which Mr. Boyd Dawkins remarked that there was no invariable relation between the sites of ancient habitation and the neighbouring hills, such as Mr. Phené had inferred. The dwellers in Britain, before the arrival of the Normans, lived in hut circles, placed sometimes on the tops of hills and at others in the bottom of valleys, but in all cases they chose a soil though which the rainwater could easily pass. This was obviously the result of their not wishing to be flooded by the rains of winter. We know next to nothing, he said, of their habits and modes of life, but the remains of the animals round their habitations proved that they lived on their flocks and herds as well as by the chase. The presence of querns, also, showed that they were pastoral. Besides the ox and horned sheep and the pig, they ate fox, wild cat, and horse, and even the dog, and, to speak in general terms, any other animal they could get hold of. About their religion or symbolism nothing was known.

Dr. Archibald Campbell said he had seen a great deal of serpent worship in India, and on returning to his native Highlands, he had made numerous inquiries as to the traces of serpent worship there, but none of the people he had asked could give any clue.

Dr. Grierson remarked that he did not consider Mr. Phené had brought forward any evidence to prove there had ever been serpent worship in Scotland.

Colonel Lane Fox observed that Mr. Phené had undertaken his expedition in regard to serpent worship with a foregone conclusion, and the result had been that he had rather disproved his case than otherwise.

The third paper was given by Mr. C. Wake *On Man and the Ape.* He opened his communication by referring to the physical agreement of structure between man and ape, and argued that the latter animal equally possessed the power of reasoning, and affirmed that man had no mental faculty other than the ape possessed. This paper also led to a hot discussion in which Canon Tristram, the Rev. Mr. Brodie, Rev. Mr. Goodsir, and others joined. Mr. Conway thought that Mr. Wake had been accused of using words such as "nature" and "evolution," which were incapable of definition, but, on the other hand, the department had heard bandied about such words as "creation," equally incapable of definition. The idea that something was produced out of nothing was just as vague an idea, he contended, as that of "nature" or "evolution."

Prof. Struthers, as a person accustomed to dissect men and quadrupeds, said that apes were very like ourselves. He had always regarded this man and monkey question as a very small one, he meant to say, it was only part of a much larger question. If similarity of structure was to prove origin, they must take in a very large portion of the animal kingdom, all made on the same general plan. He looked upon the theory of evolution simply as an hypothesis. He did not think that facts would at present warrant a belief one way or the other, though at the same time there were parts in the human body which we could not understand on the theory of man having been an independent and original creation. We had within our bodies structures which have no function, and which cannot be explained without going down to the lower animals. He did not say they had sprung from them, but he affirmed the question was not one to be bundled out of doors in the way desired by some reverend friends. He should like to say to his theological friends that scientific men did not, in the examination of these laws, shut the Creator out, it was only the *modus operandi*, the mode of proceeding, which was the subject of inquiry.

Mr. G. Harris read a paper *On the Hereditary Transmission of Endowments and Qualities.*

Dr. Charnock and Dr. Carter Blake contributed a paper *On the Physical and Philological Characteristics of the Wallons*, showing that the ordinary Wallons stood in a similar relation to Belgium to that which the Irish peasant did to the "Sassenach" of England. As evidence of their peculiar character, a Wallon would drag a pig from Namur to Ghent, or even to Bruges or Antwerp, in order to gain a few more sous than he could in his own district. The Spanish armies in the Pay-Bas were made up of Wallons. A special mental and moral character might be predicted of the Wallons of each district. The language was a spoken, not a written one, the pronunciation differing in different localities.

Mr. G. Petrie read a paper *On Ancient Modes of Sepulture in the Orkneys.* He said sepulchral mounds were there very frequent, generally on elevations. The skeletons were often discovered in a sitting posture. Mr. Flower remarked that the sitting posture of the skeleton was an interesting discovery, as it had been observed in every country in Europe, as well as in Peru, India, and Africa. Herodotus, in his account of the *Autocthones*, a people inhabiting what is now the province of Tunis, shows that they always placed their dying friends in a sitting posture to await their last hour, and it seems that they so buried their dead, as they were now found in the old African sepulchres in the same position.

The next paper was a communication received from Mr. J. Wolfe Murray, *On a Cross traced upon a Hill near Peebles.*

## SECTION E.

Most of the papers in this section were purely geographical, having but little reference to Natural Science. Among the most interesting read on the first day, August 3, was one by Mr. Clements Markham on *The Somali Coast*, contributed by Captain Miles. The paper contained some interesting information in reference to the trade in gum and aromatic spices, as it has been carried on by the natives from ancient times. Mr. D. Hanbury, alluding to a statement in Captain Miles's paper, that in ancient times frankincense was held to have come from Arabia, and from the adjacent coast of Africa, said that, while this was the case, they were taught in all the books that had appeared on the subject in the latter part of the last century, and in the whole of the present till within the last few years, to believe that frankincense was a product of India. It was very desirable to have information on this highly interesting subject. With regard to the different species of gum trees, their information was very poor, and as to myrrh it was even more so. Much had been written as to cinnamon, early authors holding that it was a production of Africa and Arabia. It was a very interesting question, and one which required elucidation, whether the cinnamon mentioned in Holy Writ was the production of Africa and Arabia, or whether it was merely carried thither from India, or from the still remoter regions of Siam and China by way of commerce, and whether in that way the idea was promulgated that it was produced in the land and districts from which it was shipped, by way of the Red Sea, to Europe.

Mr. Clements Markham also read a paper contributed by Captain Elton, on *The Limpono Expedition.* Captain Elton, who was formerly an officer on Lord Strathnairn's staff in India, undertook the expedition for the purpose of discovering whether the river was navigable to the sea—a point of great importance, on account of the discovery of gold on the banks of the Tati, one of the upper tributaries. Capton Elton's canoe was wrecked, and his journey, amounting to upwards of 900 miles—had to be completed on foot.

One of the most valuable papers in this section was one by Dr. J. D Hooker, descriptive of the botanical features of *The Atlas Range*, the main features of which we have already chronicled. Dr. Cleghorn stated in the discussion which followed, that, like everything else done by Dr. Joseph Hooker, this investigation had been carefully and thoroughly carried out, and a great desideratum of botanical knowledge had been obtained. The absence of primroses, gentian, and anemones was most remarkable. The observation on the exhausted condition of the forest was also noteworthy.

Commander A. Dundas Taylor, late of the Indian Navy, contributed a paper on *The Propesed Ship Canal between Ceylon and India.* With the Alderney and other British Parliamentary harbour discussions before the eyes of their understanding, permission, he thought, might perhaps be readily accorded to a student of thirty years in Indian hydrography to bring before the Association his views concerning the proposed scheme. After giving a sketch of the various projects that had been put forward for making a navigable passage between Ceylon and the Indian continent he proceeded to say that the project for deepening the Paumben Passage for large ships had been set aside by its own advocates in favour of the Port Lorne scheme, which had such remarkable advantages as to claim the attention of the Governments and mercantile communities of Bengal, Madras, Bombay, and Ceylon. An interesting discussion followed, in which the President and Sir E. Belcher joined.

The next morning the first paper was one by Mr. E. H. Palmer *On the Geography of Moab* A grant of 100*l.* was made by the Association last year, on the recommendation of this section, to promote the exploration of Moab, and though that grant had not been sufficient, and in consequence the exploration had been deferred, Mr. Palmer's paper explained what was already known of Moab, and what had been previously done in its exploration.

Captain H. R. Palmer, R.E., contributed a paper *On an Acoustic Phenomenon at Jabal Nagus*, in the Peninsula of Mount Sinai; and Dr. Ginsburg made a verbal communication in reference to a treatise *On Further Disclosures of the Moabite Stone.* This treatise referred chiefly to the history of the stone.

On Saturday, August 5, a communication was read by Staff-Commander George, R.N., *On a New Artificial Horizon.* The old artificial horizon, with its roof, trough, and bottle of quicksilver, was bulky, heavy, and often very inconvenient to carry; while the more convenient one with a folding roof is still open to the same objections as regards bulk, weight, and inconvenience. Yet an artificial horizon is an absolutely essential part of a traveller's equipment, so that any improvement in its construction is sure to be welcomed. Captain George's instrument is stated to combine all the advantages of the larger and more cumbrous horizon now in use, together with the additional property of securing observations at very low altitudes. The improvements are not confined to its reduced size and weight, but extend to its mechanical arrangements, form, and moderate price. The new horizon weighs $1\frac{1}{4}$ lb., while that now in use weighs $6\frac{3}{4}$ lbs. The self-replenishing horizon consists of two circular disc-like reservoirs, about $2\frac{1}{2}$ inches in diameter, and three-quarters of an inch in depth, made of iron in one casting. One contains the mercury, and the other is the trough, fitted with a glass cover for observing. The discs are connected at their circumferences by a narrow neck, with a hole drilled through it, by which the mercury passes from one reservoir to the other, communication being opened or cut off by a stop-cock, without removing the glass cover, or running the risk of losing any of the mercury.

A paper by Major Basevi, *On the Minicoy Island*, was next read. Major Basevi, who is connected with the Great Trigonometrical Survey of India, visited the Island of Minicoy, with the object of comparing the intensity of gravity on an island station with that at inland stations in the same latitude. Minicoy is a small coral island, in shape somewhat resembling a crescent, and about $6\frac{1}{2}$ miles long. The whole of the island is covered with cocoa plants, which are the chief source of wealth to the inhabitants, all of whom have their own trees—the rich as many as 2,000. The village of Minicoy is situated nearly in the middle of the island on the west side. It is half a mile long, and contains about 300 houses, built of coral rock, cemented with lime and thatched with palm leaves. The result of Major Basevi's observations on the Island of Minicoy was the conclusion that gravity on the coast is greater than inland, and at an ocean station like Minicoy greater than on the coast. It was already known that at inland stations gravity appeared to be in defect of that observed at coast stations in similar latitudes; and, by including the ocean station of Minicoy in Major Basevi's series, a confirmation of the law has been obtained.

Captain A. Pullan contributed some notes *On British Gurhwal*, where he had been employed for four years on the Trigonometrical Survey; and Mr. Samuel Mossman a paper *On the Inundation and Subsidence of the Yangtze River*.

Mr. Clements Markham read a report *On Badokshan*, by Pandit Manphul; and a description of a journey from Yassin to Yarkhand, by Ibrahim Khan. The most interesting feature in connection with these papers was that they confirmed the surveys of the country made in 1838-40 by Captain Wood of the Royal Navy. Captain Wood, who is a native of Edinburgh, discovered the river Oxus, and for doing so was awarded one of the gold medals of the Royal Geographical Society. His surveys were ignored by Prussian and Russian geographers, but were now confirmed by the native travellers who have devoted their attention to the parts of the country in question.

The Rev. F. O. Morris contributed a paper *On Encroachments of the Sea on the East Coast of Yorkshire*. It was stated that on the average there had been a loss of land of from two to three yards every year—probably about $2\frac{1}{4}$ to $2\frac{3}{4}$ yards per annum. If looked at in round numbers, the waste of land, at three yards in each year, would be found to be about thirty-nine acres between Spurn Point and Flamborough Head alone, or in a hundred years of 3,900 acres, which, at a value per acre of 30*l.* or 50*l.*, would represent a serious money loss of grain or other crops; or, taking the waste, as had been calculated, at one mile since the date of the Conquest (1066), the money value in that interval, at 30*l.* per acre, would be equal to 691,200*l.*, or at 50*l* an acre no less that 1,152,000*l.* Mr. Morris concluded by saying there was no doubt whatever that a sea-wall of roughly-hewn, or even unhewn, stones, laid on an angle of about thirty-five degrees, would for ever protect the land from encroachment.

## SECTION F.

The papers and discussion in Section F are scarcely of a nature to come within the range of a report in NATURE. Occasionally, however, they may well find a place, as when on the first day Sir John Bowring read the *Report of the Metric Committee* of the British Association. The Committee were much gratified at the large amount of information the Royal Commissioners had collected in regard to the metric system, but regretted that the Commissioners had not recommended a bolder course than the permissive legislation of its use. The Commissioners assumed there was no immediate cause requiring a general change in the existing system of legal weights and measures of the country for the purposes of external trade, but they had not sufficiently taken into account the bearings of the question on education and scientific workmanship, and the general economics of the nation. The committee admit that the full realisation of the advantages of the system must be the work of time, but all the more necessary is it to make provision for the same by inserting in any measure on the subject clauses fixing a time when the use of the new system will become binding. Pending the final settlement of this important question, the committee are gratified in finding that the Educational Code of this year for the first time prescribes that in all schools the children in Standards V. and VI. in arithmetic should know the principles of the metric system. The committee are convinced that the school is the proper place for initiating this useful reform, and urge that teachers should at once commence to introduce this subject in the schools. The committee have represented to the London School Board the desirability of introducing the metric system into its schools, and will correspond in a similar manner with other school boards. In order to diffuse information on the subject, the committee suggest that they should be re-appointed, with a grant of at least 75*l.* from the funds of the Association. After some discussion, the report was accepted—it being understood that no opinion was expressed on the compulsory question. On the same day the *Report of the Committee for the Tabulation of the Census* was read by Mr. Fellowes. It stated that various suggestions had been made to Mr. Bruce, with the view of having the returns from the various parts of the kingdom tabulated in one uniform method, and the committee had reason to believe that the recommendation in their memorial would ultimately, to a considerable extent, be adopted by Her Majesty's Government.

After the reading of Mr. Fellowes's paper *On a Proposed Doomsday Book*, giving the value of the Governmental property, as a basis for a sound system of national finance and accounts, Mr. T. J. Boyd, master of the Merchant Company, read a paper *On Educational Hospital Reform; the Scheme of the Edinburgh Merchant Company*. The object of this paper was to illustrate, from what had been done by the Merchant Company in recent years, the manner in which similar foundations might increase their usefulness and extend the benefits contemplated by the founder.

On the following day Col. Sir J. E. Alexander read a paper *On Sanitary Measures for Scottish Villages*. Among the evils pointed out as existing in these villages were the overcrowding of cottages, the system of "box-beds," in which father, mother, and children might often be found huddled together, the built-in windows quite incapable of being opened, the general want of air and ventilation, and the proximity of cow-sheds and pig-sties. The writer showed how ministers, surgeons, schoolmasters, and employers might promote the welfare of the people by inculcating the laws of health, and promoting a taste for pure and innocent recreations.

One of the most interesting episodes in this section occurred on Saturday, when the reading of a paper by Mr. George Smith, *On Indian Statistics and Official Reports*, gave occasion to the following remarks on Indian education by a native Hindoo, Mr. A. Jyram Row. A great element in the success of the schemes for the better education of the Indian population was the nature of the education which must in future be given to the natives of India. At present it was certainly of a character calculated to do a great deal of good, but at the same time it was restricted to English literature and mathematics. Now, the mere reading of Shakespeare, and the mere cramming of a few propositions from Euclid, would never enable people to embrace large questions of speculative and scientific interest, which alone could be expected in the end to lead to any practical result. Without such an education these statistical schemes would seem at first sight to have nothing to do with anything that was practical, unless it were (as some people supposed) that they merely had reference to the imposition of a poll-tax or some such thing. They could not see (and it was not to be expected that people unaccustomed to scientific questions and the bearings of each department of science upon the solution of problems entirely unconnected with the department could see) that such schemes would be of the highest consequence towards the material welfare and progress, not only of Hindostan, but of every nation on the globe. Therefore, he was of opinion that such an influential body as the British Association would do well to exert its influence in obtaining for the natives of India a more thorough scientific education.

The time of this section on Monday, August 7, was chiefly occupied by debating the administration of the Poor-law Reformatories and kindred subjects not suited to our columns. In a paper on the *Scientific Aspects of Children's Hospitals*, Dr. William Stephenson endeavoured to show how far their general management tended to promote the twofold object for which they were called into existence—namely, the relief of the children of the poor and scientific instruction in the diseases of children—and what external causes were at work to check the full development of the influences they exerted. He pointed out the importance of such institutions as the Sick Children's Hospital, in the way of extending the knowledge of the diseases of children among students of medicine, and also in the way of training nurses both for the hospital and for the family.

On Tuesday, the paper which excited the greatest interest and most animated discussion was by Miss Lydia Becker, *On some Maxims of Political Economy as applied to the Employment of Women and the Education of Girls*; and this was followed by one on *Naval Efficiency and Dockyard Economy*, by Mr. Charles Lamport, and by others on *Land Tenure and the Assessment of the Poor*, concluding the business of this section.

### SECTION G.

On the opening day of the Association, Mr. Thomas Stevenson, C.E., in introducing the subject of a *Proposed Automatic Gauge for the Discharge over Waste Weirs*, said that in order to ascertain the amount of available rainfall, which was so important in questions of water supply, it was necessary to gauge the quantity of water which escaped at the waste weirs of reservoirs. Observations made only once or twice a day could not supply the information. He proposed to place a tube perforated vertically with small holes, the lowest of which was on a level with the top of the waste weir, so that whenever water passed over the weir, it also passed through the holes in the tube. The water was collected in a tank capable of holding the discharge for a certain number of hours. The quantity so collected was a known submultiple of what passed over the weir. The discharge through the holes was ascertained by experiment.

In the discussion which followed, different views were expressed as to the practical value of Mr. Stevenson's proposal, which was, however, favourably regarded by Prof. Rankine.

A paper on *A New Form of Salmon Ladders for Reservoirs* was read by Mr. Alexander Leslie, C.E. The new form of salmon ladders for reservoirs of varying level, a model of which was exhibited, contemplates that on all occasions the whole outflow required to run down the stream should be through only one sluice at a time, and over the top of that sluice, which would open by lowering, and shut by being raised, except in extreme floods, when, for the sake of keeping down the level of the lake, so as to avoid flooding the adjoining lands, or when from any other exceptional reason, such as an accumulation of ice, it may be necessary to provide a lower outlet or the means for a more rapid discharge. Assuming the rise of the lake to be 12 feet, and that it is full, or up to the level of the waste weir, the uppermost sluice is opened, so that the water may flow over it to the depth of, say, from 9 to 12 inches, and then run down the inclined plane of, say, 10 feet in width, composed of a series of pools formed by stops reaching quite across from wall to wall, the fall from surface to surface of those stops being 18 inches, and the depth of each pool being not less than 3 feet. The fish may then easily leap over the successive falls, seven in number, after which they must take the last leap over the sluice into the lake, the last leap being at first like all the rest, 18 inches, but diminishing in height as the level of the lake is lowered, till at last it is nothing, when the level of the lake comes to be the same as that of the pool. The paper went on to describe the process of the working of the machine when the lake gets too low to give the requisite supply of water over the top, and concluded by stating that it would be preferable not to make the ladder above 18 inches. On that point, however, the author did not offer any decided opinion, but left it an open question.

The next paper was by Mr. R. A. Peacock, C.E., Jersey, on *A Chain Cable Testing and proposed New Link*. The paper proposed to provide a new testing link, which, it was believed, would be found useful in various ways. The following is a description for a cable of which the metal would be one inch in diameter:—Let the cable manufacturer provide himself with a number of plates of rolled iron, of the same quality as the cylindrical bars of iron of which the ordinary links are made. The thickness of each plate is to be equal to the diameter of the bar from which it is provided. Eight links will have to be punched out of the plates by means of a steam punch. One new link, when filed half round, will be placed longitudinally at each extremity of the cable, with which it will be connected. A new link, after being filed as aforesaid, will be inserted longitudinally at every fifteen fathoms in each cable, so as to form a part or parts of the cable; and each cable being about 150 fathoms long, will require eight new links.

A paper *On Road Steamers*, by Mr. R. W. Thomson, Edinburgh, was read by Mr. Miall. In the outset, the paper alluded to the importance of road steamers and the difficulties which had been encountered in arriving at the present stage of perfection with these machines. A uniformity in the working of the engine having been reached, a thick carpet of india-rubber for the tires of the wheels was introduced, which much improved the running on roads. These india-rubber tires not only completely prevented hard shocks to the machinery, but saved the road from the grinding action of the iron wheels which was so injurious to by-ways. There had been serious objections made to the use of these engines by people interested in the roads, but the author could assure them that the india-rubber tires actually improved the roads. The paper went on to refer to rigid tires as used for road steamers, and stated that the amount of adhesion obtained by this tire was much less than by the india-rubber kind. The latter kind took a firm hold of the road, whatever might be the nature of the surface. The only ground upon which india-rubber tires did not work well was where the soil was extremely wet or of a very soft nature. For farm work the wheels of the engine required a much thicker coat of india-rubber.

Mr. Robert Fairlie read a paper *On the Gauge of Railways*. The author argues for the narrower gauge, and says:—Experience has shown that 3ft. 6in. can be made a highly economical and efficient width, but it does not by any means follow that it is the most serviceable and most efficient, any more than it follows that the accidental 4ft. 8½in. was all that could be desired, even though an Act of Parliament had made it an article of belief. On the contrary, as our knowledge and experience increase, we are enabled to approach more and more nearly to that happy mean on either side of which is error. While, on the one hand, there is every necessity for obtaining such a gauge as will afford a good and useful width of vehicles, on the other it is necessary to avoid such narrow limits as would necessitate the introduction of too great overhang on each side of the rails. The 3ft. gauge appears to me to comply with all the necessary conditions better than any other, and it is from no mere theorising that I lend all the influence I have towards its adoption. There is a certain amount of saving in first cost as compared with the 3ft. 6in., not a large amount, but worth considering. This, however, I leave out of the discussion for the present. The all-important matters are to place upon the rails a thoroughly efficient stock that shall possess a maximum of capacity and a minimum of weight, and to supply engine-power under the most economical circumstances, and I hold it to be easier to accomplish these objects on the 3ft. gauge than upon any other. I am led to this conclusion both by a comparison of the actual work done on the railways of the 3ft. 6in. gauge, with that which can be accomplished with the 3ft. gauge, and because, having in view the practical requirements of goods traffic, I find that I can obtain an ample floor area with less dead weight than can be secured by any other width; on a wider gauge the dead-weight increases, on a narrower one the capacity diminishes. He quoted figures to show that to carry 50 tons of goods on the Norwegian or Queensland 3ft. 6in. gauge, the proportion of one ton per waggon being preserved, 92 per cent. of the weight of rolling stock used on the 4ft. 8½in. would be required; as against only 43 per cent. on a 3ft. gauge, showing a saving of 47 per cent. on the latter as compared with the 3ft. 6in. Of course, if the waggons were loaded up to full capacity, these percentages would be very much changed. It is to this point especially that I wish to direct your attention, as upon it the economy of the 3ft. gauge rests. Whatever saving may be effected in first cost may be lost sight of, the great advantage lying in the saving effected in working expenses. Every ton of dead weight saved goes towards securing the prosperity of the line, and if we can obtain the ample platform which the 3ft. gauge gives, combined with so much saving in weight, nothing is left to be desired.

A paper *On a New System of Warming and Ventilation* by Mr. J. D. Morrison, was read. The main features of the system consist in so circulating fresh air through a warming chamber into the room, and foul air through the fire into the chimney, that all local currents are resolved into one, which forms an upper warmer current from the fire to the opposite wall, and an under colder current from the wall back again to the fire, when, after supporting combustion, the products escape up the chimney. The vacuum thus produced by the warmer current through the chimney creates the now colder current from the atmosphere, which, passing through the heating chamber, supports the respiration of any number of persons.

On Friday, August 8, Mr. A. E. Fletcher, F.C.S., read a paper *On the Rhysimeter*, an instrument for indicating the velocity of flowing liquids, and for measuring the speed of ships through the water. The principle on which it is constructed resembles that of the anemometer, recently brought into notice by Mr. Fletcher, by which he is able to measure the speed of hot air, flame, and smoke, contaminated with dust or corrosive vapours, as met with in furnace flues and factory chimneys. Both in the anemometer and in the rhysimeter, the impact force of the current, and also its tendency to induce a current parallel with itself, are measured and made to become indicators of the force and velocity of the stream. The apparatus is very simple. A compound tube with two orifices at the bottom, one of which faces the source of the current, while the other faces the opposite direction, is held in the stream, and communicates by tubes with the indicator where the pressure is measured by columns of ether, water, or mercury, according to the circumstances of the case. When used to measure the velocity of a brook or open stream of water, the speed at any depth or at any portion of its surface can be separately estimated. For taking the speed of water in pipes it is only necessary that there should be suitable cocks screwed into the pipes at the required places; through these the "speed-tube" of the rhysimeter passes without allowing any escape of water, whatever may be the pressure. A still more important application of the instrument is to measuring the speed of ships. Here the speed-tube pierces the bottom or side of the ship, and projects a few inches into the water outside. The indicator may be in the captain's cabin. It resembles in size and appearance a barometer. In it a column of mercury indicates continually the speed of the ship. The full effect of the velocity is imparted to the mercury, without loss by friction or otherwise, so that the indication must always be absolutely correct. The instrument may be made self-registering, showing by a dial the total number of knots the ship has run since she left port, and marking on a sheet of paper the speed attained at every portion of the time. This permanent register may, in many cases, be of the greatest value. The paper was illustrated by diagrams, and by tables showing the velocities in knots per hour, or in feet per second, for the various heights of the columns of water or mercury.

Admiral Sir Edward Belcher said the principle was very valuable, but he did not see the necessity of passing the tube down so far below the water. He thought one or two inches would suffice.

Prof. Rankine said the principle of the instrument was an old one, and the author, he believed, admitted this. Mr. Fletcher had overcome a series of inconvenient and difficult details, and had produced an instrument which had actually been applied to practice with satisfactory results. He belived that the instrument would be a good substitute for the old log system of ascertaining the speed of a ship.

This section did not sit on Saturday.

## SCIENTIFIC SERIALS

THE article in the *Quarterly Journal of Science* for July which will be most read, is by the editor, Mr. Crookes, "Experimental Investigation of a New Force," on which we have already commented. "The Dawn of Light Printing" gives a sketch of the early discoveries in photography of Niepce, Fox Talbot, and Daguerre. Mr. F. C. Danvers gives an account of the present condition of inventions for Pneumatic Transmission, with mathematical formulæ for the power obtained. Under the title "Where are the bones of the Men who made the unpolished Flint Implements?" Mr. Pengelly argues that we know so little about the effect of various climatic and atmospheric conditions on the bones of man and the lower animals, that it is rash to conclude, because human remains are not, as a rule, found associated with flint implements and animal remains in the bone caves, that therefore they cannot have been originally deposited along with them. He also cites a number of unquestioned instances in which the bones of man have been found in such situations, to all appearance contemporaneous with the animal remains. Even were such evidence entirely wanting, Mr. Pengelly considers the flint implements themselves absolutely conclusive proof of the contemporaneity of man with the mammoth and the extinct cave-animals. One of the most valuable and interesting articles in the number, though a short one, is entitled "A New Mechanical Agent: A Jet of Sand." Mr. B. C. Tilghman, of Philadelphia, appears to have solved the problem of cutting or carving, mechanically, hard substances, such as stone, glass, or hard metals, in an expeditious, accurate, and economical manner. He has shown that a jet of quartz sand thrown against a block of solid corundun will bore a hole through it one and a half inches in diameter and one and a half inches deep in twenty-five minutes, and this with a velocity obtainable by the use of steam as a propelling power at a pressure of 300lbs. per square inch. The apparatus used for grinding or cutting glass or stone is described in detail. By covering parts of the glass surface by a stencil or pattern of any tough or elastic material, such as paper, lace, caoutchouc, or oil paint, designs of any kind may be engraved upon it. In his abstracts of the Progress of Science, the editor now confines himself entirely to the physical branches.

THE *American Naturalist* for August contains no one very striking paper, though several of considerable interest. Dr. J. S. Billings contributes a mycological paper on the "Study of Minute Fungi," and Mr. A. S. Ritchie one, entitled "The Toad as an Entomologist," showing the very large number of insects which that animal destroys. On one occasion the writer found thirteen perfect insects in the stomach of a toad belonging to nine species, besides one elytron each of two others, and other vestiges of legs and wings. He concludes that the toad is of great service to agriculturists.—Prof. Lesquereux has an article on the "Mode of Preservation of Vegetable Remains in the American Coal Measures," an important article on vegetable palæontology; and Alexander Agassiz a short paper on "Systematic Zoology and Nomenclature," indicating the great importance of a correct system of nomenclature as an item in the history of zoology.

THE *Western Chronicle of Science* for July 1871. Edited by J. H. Collins, F.G.S. Nos. 1—7. Falmouth, W. Tregaskis.—We have much pleasure in noticing the first seven numbers of this local scientific periodical, and sincerely hope it will not be allowed to drop from want of subscribers, of which the editor complains. It should be encouraged by all lovers of scientific inquiry, not only in the western district but throughout the country. Its low price, only twopence, puts it within the reach of all, while at the same time a large circulation is required to make it pay. The seventh number contains an interesting paper, valuable both to architects and geologists, on the ornamental rocks of Devon and Cornwall, counties abounding in beds of vari-coloured limestone sufficiently hard to receive the polish of marble. The second is a most sensible and judicious paper on the duties of local societies. If the suggestions here made were carried out in all societies, an interest in physical science would soon become universal. Besides other matters, the number contains the results of the May examinations in science, so far as these concern the classes in the West of Cornwall. A large proportion seem to have passed in the various subjects, the total number of successful candidates being 69.

SINCE the commencement of the *Revue Scientifique*, it has continued much the same course as its predecessor the *Revue des Cours Scientifiques*. Seven numbers are now before us, containing among others, the following articles, besides reports of lectures or extracts from the proceedings of various learned societies at home and abroad:—Van Beneden on Commensalism in the animal kingdom, Ancient Churches by M. Ch. Contejean, Geographical distribution of the Balænæ by Van Beneden, Physico-chemical researches on Aquatic Articulates by M. Felix Plateau, M. Chauveau's Report on Science and Legislation in relation to the Cattle-plague in France, M. Claude Bernard on the Influence of External Heat on Animals, Accounts of the Life and Writings of M. Claparède and Prof. Payen, M. Pasteur's address, "Why France did not find superior men in the moment of peril," the addresses delivered at the Liverpool meetings of the British Association by Huxley, Tyndall, and Rankine, and reports of some of the sectional proceedings.

## *SOCIETIES AND ACADEMIES*

### Bengal

Asiatic Society, June 7.—"Memorandum on the Total Eclipse of December 11 and 12, 1871," by Lieut.-Col. J. F. Tennant, R.E., F.R.S. In December of this year we have a Total Eclipse visible in Southern India. The duration is short, but in some respects the circumstances are very favourable, as the Line of central Eclipse passes over the Nilgherry Hills, where, I understand, fine weather may be confidentle.expected. In order to be prepared, I have computed carefully the Central Line across India, and have added the extent to which errors of the Tabular place of the moon may be expected to shift it. I hope to have before the Eclipse a knowledge of what errors may be anticipated in the Tables, and thus be in a position to choose a central spot, if it is worth making a change. The figures, however, show that this is not probable, the principal result of an error in Right Ascension being to shift the centre of the shadow along its path, the deviation from which would be corrected by a small error in the declination which could hardly be foreseen. The duration of the Eclipse will be small. At the Nilgherries it will be about two minutes, but this cannot, so far as I know, be as yet accurately predicted, from uncertainty as to the real diameters of the sun and moon, when free from the enlargement by irradiation. If the value of the moon's diameter deduced by Oudemans from Eclipses, be used with that of the sun obtained in the Greenwich Transit Circle, then I find the duration in the Nilgherries just two minutes. The data of the Nautical Almanac give two minutes seven seconds, and if I may judge from the result I got in 1868 the real duration will fall between these. Short as this time is, it is enough with an adequate preparation to produce some results of value. It is long enough to allow photographs to be taken of the Corona, as to whose structure there is more to be discovered. There seems now no sort of doubt that the Corona is not only a solar appendage, but is, as I stated in my report on the Eclipse of 1868, the comparatively cold atmosphere of the sun. This should be further spectroscopically examined. Observers have differed about the number and position of the faint bright lines they have seen, but it does not seem that any one has connected the variations with the position of the part examined. To do this appears urgently necessary, and there have been additions made to the spectroscope which will allow more than one portion of the Corona to be examined, and its lines recorded during the short time it is visible. There is another subject, too, of spectroscopic examination. Kirchhoff, in his theory of the solar constitution, supposed it surrounded by an extensive atmosphere consisting of metallic and other vapours, as well as gases, by the absorption of which the dark Fraunhöfer lines were produced It has long been clear that there was no such extensive atmosphere, and some physicists have been satisfied that there is none such. Mr. Lockyer and his collaborateurs, though they have detected a great number of bright lines at the bases of the prominences, have never approached, so far as I know, the number of even the conspicuous dark lines, whose origin has, therefore, not been satisfactorily made out. At the Eclipse of December 22, 1870, however, Prof. Young, at the moment of obscuration, and for one or two seconds later, saw, as far as he could judge, every atmospheric line reversed, and this was confirmed by Mr. Pye. I have but the scant information of this point given in the Royal Astronomical Society's Council Report, but it is sufficient to show me why this has not been seen before by observers looking out for it, and also to make me feel the importance of verifying the observation. To understand why it has not been seen before, it must be considered that the image of a bright object in the focus of a telescope when relieved against comparative darkness is enlarged by a phenomenon known as irradiation; the light encroaches on the darkness. The sun thus appears larger and the moon smaller than the real size. This continues till the real contact of the limbs internally; at this moment the thread of light, which previously had considerable width, appears suddenly broken and vanishes in a total eclipse; while in the transit of a planet or annular eclipse there appears the "black drop" of the observers of the Transit of Venus in 1769. At page 16, vol. xxix, of the monthly notices of the Astronomical Society will be found some figures illustrating this phenomenon in a planetary transit. When we are dealing with so thin a stratum surrounding the true photosphere, we cannot see it in sunshine, as it is lost in the irradiation (it *may* be partly visible in very large telescopes where the irradiation is very small), and we are very apt to lose it at the moment when the sun disappears, for it is found only between the places where a moment before the sun and moon's limb appeared, so that the observer following either of them might well miss it. In the search for and verification of this important observation, the duration of total phase can matter little. I have been in communication with the Home Secretary on the subject of observations of this eclipse, and my views, I may say, have been most cordially receivd. I am not yet in a position to submit a proposition officially, but I have great hopes of being able to do so in a few days.* I may just mention that in plotting the shadow track on a map it is necessary to allow for the error of its zero of longitude, a precaution often forgotten. The longitudes of the G. T. Survey require a correction of 3′-2·7″, and those of the Atlas of India one of 4′-11″ to adjust them to the accepted longitude of Madras.

The President was very glad to learn from Colonel Tennant that the Government is likely to sanction a scientific expedition to the Nilgherries on the occasion of the total eclipse in December next. The objects to which Colonel Tennant proposed to direct observation were, he need hardly say, of very great scientific interest and importance. The spectroscopic analysis of the Corona, so far as it had yet been effected, had been productive of no very certain results. The matter could not, however, be in better hands than those of Colonel Tennant. He only wished to suggest that those members of the Society, who might have the requisite leisure and opportunity, should, even with the unaided eye, endeavour to observe as carefully as possible the exact apparent shape and characteristics of the Corona. He believed that data of very considerable value might be thus obtained by persons who knew how to observe. Later in the evening Colonel Tennant kindly consented to draw up some short directions which might serve as a guide to members of the Society who might visit localities of the total eclipse.

### Paris

Academie des Sciences, Aug 7.—M. Faye in the chair. Notice was given of the death of M. Lecocq, a correspondent living in Clermont Ferrand, the author of valuable pamphlets and papers on the geology of Central France. M. Lecocq was, however, a very active and clever physicist, and started many theories of his own. He was a Professor in the University, and his loss will be very deeply felt by his friends.—Two different papers were sent describing a bolide which was seen on the 15th of August, and which is most extraordinary, as it was visible during twenty minutes by Marseilles observers. The course was most irregular and zig-zag. Leverrier supposed that two different bolides might have been seen at Marseilles and at the other stations, as the descriptions do not agree. The fact of remaining visible during so long a time at Marseilles is astonishing, and M. Leverrier is at a loss to account for it. The phenomenon will be more fully investigated. This is also the case with a paper sent by M. W. de Fonvielle, describing the fall of a thunder-bolt on August 3, $3^h$ $19^m$, on the kitchen of a convent situated in Paris, at 250 yards from the National Observatory, where the astronomers felt a great shock. A gas-burner was lit under very curious circumstances. The explosion was very long and very strong, and it is supposed the lightning was shaped like a sphere falling from the clouds. M. Dumas showed the interest of elucidating a phenomenon of so much importance for public safety, as ignition of gas may be the secret cause of many fires. The committee is composed of M. Dumas and M. Jamin, professor to the Sorbonne. Special experiments and inquiries will be made at the expense of the Academy. M. Fonvielle will be an auxiliary member of the committee.—M. Delaunay read a paper on the Observatory during his administration, and showing that observations of small planets will be made with greater zeal than on former years.—A letter was read from M. Angström, the Swedish physicist, maintaining that each gas has its own spectrum in spite of the differences exhibited by previous experiments. The learned physicist shows that in each case where differences were found, it is possible to explain it by extraneous matters, mixed with the substance submitted to the experiment. The importance of this memoir is obvious.—M. Bert, who was formerly the Prefect of the North during the investment of Paris, sent a paper on the death of fishes living in fresh water when immersed in sea water. These fishes are literally suffocated by a singular effect of desiccation, the exosmose is very active, principally when their skin is clothed with large scales. The phenomenon is quite extraordinary when observed on frogs, which lose the greater part of their weight, and are almost as much dried up as if they had been salted alive.

* This has since been done.

M. Bert will examine the action of fresh water on sea-fish, which is not so rapid. These sea-fish are too heavy for fresh running water, and are found generally to remain at the bottom of the water. On the contrary, fresh water fish always swim at the top of salt water.

### New York

Lyceum of Natural History, Oct. 24, 1870.—In a paper read at this sitting the author observed:—In the sequence of events included in our Drift period there is a marked break, a middle period, during which, over most of the north-western states, no Drift deposits were made, and when most of this area was covered with a forest growth and sustained many and large animals. At a subsequent period, all parts of this area, less than 500 feet above the highest of our present great lakes, was submerged, and most portions of it covered to greater or less depth, with new Drift deposits, clays, sands, gravel and boulders, a large part of northern and remote origin. Nearly all the large boulders of the Drift belonging to this later epoch are sometimes of great size (100 tons) and have been *floated* to their present positions, as they overlie undisturbed stratified sands and clays, which would have been broken up and carried away by glaciers or currents of water, moving with sufficient velocity to transport these blocks. Hence they must have been floated from the Canadian highlands, the place of origin of most of them, by *icebergs*. This epoch of the Drift period I have therefore termed the Iceberg Epoch. During this epoch the submergence of the land in the interior of the continent, was greater than in the epoch of the deposition of the Champlain and Erie clays, and all the area north of the Ohio was covered with water up to a height of over 500 feet above Lake Erie, or 1,100 feet above the ocean level. The highlands of south eastern Ohio, and most of the country south of the Ohio river, were not covered by this flood, and now bear no drift deposit of any kind. Tracing out the line of ancient water-surface, we find that the depression was greater towards the north, so that the Alleghanies and their foot-hills, and also a wide area of comparatively low country in the Southern states, formed not only a shore, but a continental limit to the great interior iceberg-ridden sea of the later Drift Epoch. In the western reaches of this sea, which was of fresh water, in the later centuries of its existence, was deposited the Löes or "Bluff" which I have elsewhere designated as the later lacustrine, non-glacial drift. During the deposition of the Löes the interior sea was already narrowing and growing shallower by the cutting down of its outlets, or by continental elevation, or both. The descent of the water-level and decrease of water-surface have been going on perhaps constantly, but not uniformly, to the present time, when the area of the great lakes is the insignificant 85,000 square miles it now is. In the descent of the water-level, retarded at certain periods, terraces and beach lines were formed at various places by the shore waves. With these history ends. This then is the classification I would suggest of the drift deposits as they occur in the valley of the Mississippi, premising that here, as in other geological periods, the column is nowhere absolutely complete:—

| Period. | Epochs. | Strata. | Notes. |
|---|---|---|---|
| Quaternary. | Terrace. | Terraces, Beaches, Löes. | Sand and gravel beaches with logs, leaves, and fresh-water shells. Löes with fresh-water and sand shells. |
| | | Iceberg Drift, Löes. | Boulders, gravel, sand, and clay, drifted logs, elephant and mastodon teeth and bones. |
| | | Forest Bed. | Soil-peat with mosses, leaves, logs, stumps, branches, and standing trees, mostly red cedar. Elephas, mastodon, castoroides, &c. |
| | Glacial. | Erie Clays. | Laminated clays with sheets of gravel, occasional rounded and scratched northern boulders, many angular pieces of underlying rocks. |
| | | Glacial Drift. | Local beds of boulders and rarely boulder clay resting on the glaciated surface. |

From the above table it will be seen that the remains of elephant, mastodon, and the gigantic beaver, occur in the forest-bed and in all the succeeding drift deposits. It should also be said that they are found in still greater abundance in peat-bogs and alluvial deposits which belong to the present epoch. We have seen that the submergence of the later drift epoch, though so wide-spread, left a large part of the area lying between the Mississippi and Atlantic uncovered. This area the elephant, mastodon, great beaver, &c., inhabited during the continuance of the flood that covered the forest bed. From this retreat they issued with the subsidence of the water, following the retreating shore-line, till they occupied all the region now exposed about the great lakes. By what influence they finally became extinct, we cannot yet say. It has been claimed that they continued to exist down to the advent of man, and that he was an agent in their destruction. This statement may be true, but requires further proof before it can be accepted with confidence. The vegetation of the forest bed indicates a cold climate, thus confirming what we had otherwise learned of the habits of the extinct elephant. He was clothed with long hair and wool, was capable of enduring, and probably preferred a sub-arctic climate, and was associated in this country as in Europe, with the musk ox and the reindeer. We may therefore infer that a progressive increase in the annual temperature, drove most of the animals of the Forest-bed northward, and caused to gather on the shores of the Arctic sea, the herds of elephants whose remains so much impress all travellers who visit that region. This was probably the scene of the last vigorous and abundant life, and of the death of the species; an event consequent, perhaps, on the action of local causes, which we shall comprehend when we have opportunities of studying the record. One remarkable statement in regard to the Forest-bed requires notice. In more than one instance, parties digging wells in South-Western Ohio, have reported not only that they found a black soil and logs, but that "some of these logs bore marks of the axe, and were surrounded with chips." These stories I formerly rejected as pure fabrications; but in the light of recent observations, they seem to me to be in part true, and not difficult of explanation.

## BOOKS RECEIVED

Foreign.—(Through Williams and Norgate)—Skandinaviens Coleoptera Synoptiskt Bearbetade, vol. x: C. G. Thomson.—Medicinische Abhandlungen: E. Reich.

## PAMPHLETS RECEIVED

English.—Journal of the Chemical Society, second series, vol. ix.—The Seat of the Soul Discovered: (J. Gillingham (F. Pitman).—Notes on the Antechamber of the Great Pyramid: Capt Tracy, R.A.—Proceedings of the Essex Institute, vols. i to vi.—Bulletin of the Essex Institute from the commencement to August 17, 1870.—Instructions for the Prompt Treatment of Accidents, &c.: A. Smee.—Accident Insurance Company, a Year's Claims, 1870.—Journal of the Iron and Steel Institute, No. 3, vol. ii.—The Manufacture of Russian Sheet Iron: Dr J. Percy (John Murray).

American and Colonial.—Transactions of the Entomological Society of New South Wales pt. 2, vol. ii.—The American Gaslight Journal.—Transactions of the Academy of Natural Sciences of Philadelphia, parts 9 and 10.—Proceedings of the Albany Institute, vol. i., part 1.—Memoirs of the Boston Society of Natural History, 1868-69.—Memoirs of the Peabody Academy of Science, vol. i., No. 2.

Foreign.—Les Mondes, Nos 14 and 16.—Journal de Medicine et de Chirurgie, Nos. 3 to 6, 1871.—Giornale di Sicilia, No. 173.—Rendiconti, vol. iv., No. 14. Astronomische Nachtichten, No. 1856.—L'Institut, No. 1920.—Annals de Chimie et de Physique, vol. xxii., Jan. 1871.—Bulletin Hebdomadaire, 192. La Revue Scientifique, No. 8.—Allgemeine Bibliographie, &c., No. 32.—Sitzungsberichte der Gesellschaft der Wissenschaften in Prag, for 1870.—Zu Anatomie der Elephanten Schilderkrote: Dr. A. Fritsch.—Uber die Anzietung: Dr. A. von Waltenhofen

## CONTENTS

THURSDAY, AUGUST 31, 1871

## ON THE VARIOUS TINTS OF FOLIAGE

ALTHOUGH we cannot yet say—

Far, far o'er hill and dale green woods are changing,
Autumn her many hues slowly arranging,

still it may be interesting to put together certain facts with reference to the tints of foliage which have recently been acquired to science.

Up to the present time I have been able to distinguish several dozen colouring matters in the leaves of different plants, and far more in the petals and fruits, and no doubt further inquiry will very greatly increase this number. The subject would, therefore, be quite unmanageable, if we could not divide them into well-marked groups by means of their optical characters. This is still more important when, as on the present occasion, it is desirable to give a condensed summary of the leading facts. I shall, therefore, not attempt to describe the individual colouring matters, or to explain how they may be distinguished or identified by means of their spectra, either in their natural state or after being acted on by re-agents; but merely point out the general relations of the various groups, and refer to my published papers for illustrations of the methods employed in the inquiry.* The colours of these groups are not only related to one another optically and chemically, but also have a very similar connection with the growth of the plants, and thus it is possible to give a general explanation of the very various tints of foliage, without entering into technical details. For a more complete account, I beg to refer to a paper on this subject, just published in the July number of the *Quarterly Journal of Microscopical Science.*

One of the chief difficulties in studying the colours met with in plants is, that they are often mixtures of quite distinct colouring matters. Sometimes these may be easily separated, for one may be soluble, and the other insoluble, in such reagents as water, alcohol, ether, or bisulphide of carbon; but in many cases they are so closely related, that anything like a complete separation is perhaps impossible; even then, however, it may be possible so to effect a partial separation, that the presence of two different substances may be recognised, and with proper care a very good opinion may be formed as to their general properties. Nature, also, herself often assists us in this inquiry, for different plants, or the same in different states, may furnish particular colouring matters comparatively pure, or so variably mixed that the character of the mixture may be recognised.

For the purposes of the subject before us, I have found it desirable to divide the different colouring matters into the following groups:—

1. The *Chlorophyll* group is distinguished by being insoluble in water, but soluble in alcohol and in bisulphide of carbon. There are three or four species, giving well-marked spectra, with several narrow, dark, absorption-bands, one or more of which occur at the red end. The mixed chlorophyll of ordinary green leaves may be obtained in a tolerably satisfactory state by heating in alcohol dark green holly leaves, previously crushed so as to insure rapid solution, and then, when cold, agitating in a test tube with bisulphide of carbon. This sinks to the bottom, holding nearly the whole of the dark green chlorophyll in solution, whilst nearly all the xanthophyll remains dissolved in the alcohol. Leaves having an acid juice must not be used, for that would change the normal chlorophyll into another modification, nor should the solution be left long in contact with them, for then the separation is much less perfect.

2. The *Xanthophyll* group also contains several distinct species, but only two are common in leaves, one being more, and the other less, orange. They are characterised by being insoluble in water, but soluble in alcohol and in bisulphide of carbon; and when dissolved in this latter their spectra show two not very distinct absorption-bands at the blue end; but the red, yellow, and yellow-green rays are freely transmitted. They may be obtained from yellow leaves, by the use of alcohol and bisulphide of carbon.

3. The *Erythrophyll* group comprises a number of colours soluble in water, in alcohol, and in ether, but insoluble in bisulphide of carbon. Those met with in leaves are more or less purple, made bluer by alkalis, and redder by acids; and thus sometimes plants containing the same kind may vary more in tint, owing to a variation in the amount of free acid, than others coloured by entirely different kinds. The erythrophyll may be obtained, free from chlorophyll and xanthophyll, by heating the leaves in alcohol, evaporating to dryness, redissolving in water, filtering, and evaporating at a gentle heat; but, on the whole, it is better to digest the leaves for a day or so in sufficient cold ether to dissolve all the contained water, and then to agitate with water, which subsides to the bottom, with nearly all the erythrophyll in solution, but mixed with more or less of the colours of the following group. There are many species of erythrophyll, some of which have very interesting botanical relationships, being so far found only in particular classes of plants.

4. The *Chrysotannin* group contains a considerable number of yellow colours, some so pale as to be nearly colourless, and others of a fine, dark, golden yellow. They are soluble in water, in alcohol, and in ether, but not in bisulphide of carbon. Their spectra show a variable amount of absorption at the blue end, usually with no bands when in their natural state, but sometimes with one or more sufficiently distinct when they are oxidised. They may be obtained free from chlorophyll and xanthophyll by processes similar to those made use of in the case of erythrophyll, and leaves should always be selected which are as free as possible from colours of that group. Some of the chrysotannin colours strike a dark colour with ferric salts, and constitute the tannic acid sub-group, of which there are at least six different kinds, whereas others do not give any such reaction, and constitute the chrysophyll sub-group. In both sub-groups the intensity of colour is usually greatly increased by partial oxidisation, and they are thus altered into colours of the following group.

5. The *Phaiophyll* group comprises a number of more

* Proceedings of the Royal Society, vol. xv., p. 433 (*Philosophical Magazine*, vol. xxxiv., 1867, p. 144); *Quarterly Journal of Microscopical Science*, vol. ix., 1869, pp. 43 and 358; *Monthly Microscopical Journal*, vol. iii., 1870, p. 229; *Quarterly Journal of Science*, new ser., vol. i., 1870, p. 64.

or less brown colours, insoluble in bisulphide of carbon, and of very variable solubility in water or alcohol. The spectra show strong absorption at the blue end, extending over the green, often the red is very dull, and sometimes there are definite absorption bands, when the solution is acid, neutral, or alkaline. On the whole they are in that state of oxidisation which has a maximum intensity of colour, and are simply decolourised by further oxidisation.

The very numerous tints of foliage depend almost entirely on the relative and absolute amount of the various colours of these different groups, but much remains to be learned before we can explain all their relationships. The colour of green leaves is mainly due to a mixture of chlorophyll and xanthophyll and the variation in the relative and absolute amount of these easily accounts for the darker and brighter greens. The tints are also much modified by the pressure of colours of the erythrophyll group, which, according to circumstances, may give rise to lighter or darker browns, approaching to black, or to reds. Healthy unchanged leaves also contain various substances belonging to the chrysotannin group, but in many cases when these belong to the more typical kinds of tannic acid, their colour is so faint that they have little or no influence on the general appearance of the leaves.

The relation of these groups to one another is still somewhat obscure. There are facts which seem to indicate that chlorophyll may in some cases pass into xanthophyll by oxidisation, and xanthophyll into chlorophyll by deoxidisation, but neither point can be considered to be established. There is manifestly some connection between the formation of chlorophyll and erythrophyll; and those conditions which are favourable to the production of one are unfavourable to the development of the other. In the present state of our knowledge it seems most probable that chlorophyll is formed when the vital functions of the leaves are very active, and erythrophyll when they are less active but not destroyed. Exposure to light also appears to be necessary, and we often see rough natural photographs of superjacent leaves produced in this manner. As I have already said there are several different kinds of erythrophyll, giving very different spectra, but the most prevalent are two which are related to each other in an interesting manner. One of these is more especially found in very young leaves, and when slightly oxidised artificially it passes into the other. This more oxidised kind is that found in the greater number of leaves which are red in autumn. Both are completely decolourised by further oxidisation, and most probably this occurs in leaves themselves when their red colour is lost. Since many contain erythrophyll in early spring and lose it as the season advances, whilst it still continues to be present in the leaf-stalks, I am much inclined to believe that its disappearance is due to the ozonised oxygen given off from the chlorophyll, which is developed to so much greater an extent in the leaves than in the stalks, and that its reappearance in autumn in many leaves is characteristic of the period when they are not dead but have more or less ceased to give off ozone.

On the approach of autumn, before the leaves have withered, we have thus in the foliage of different plants an exceedingly variable mixture of chlorophyll, xanthophyll, and erythrophyll, with the different members of the chrysotannin group; and it is to the changes which occur in some or all of these substances that the very variable tints of autumn are due. The most striking of these depend on the alteration of the chlorophyll. So long as it remains green the production of bright reds and yellows is impossible, but when it disappears the yellow colour of the xanthophyll is made apparent; and, if much erythrophyll is present, or contemporaneously developed, its colour, combined with this yellow, gives rise to scarlet or red. In many cases, however, the chlorophyll does not disappear, but is changed into the dark olive modification, easily prepared artificially by the action of acids on the more green, and when this is present, only dull and unattractive tints can be produced. We may thus easily understand why the special tints of early autumn are yellows and reds, or dull and dark greens. In these changes the various pale yellow substances of the chrysotannin group remain comparatively unaltered, and even sometimes increase in quantity; but they soon pass into the much darker red-browns of the phaiophyll group, whilst the erythrophyll fades; and thus later in the autumn the most striking tints are the brighter or duller browns, characteristic of the different kinds of plants or trees.

As already named there are many different species of colouring matters belonging to the chrysotannin group, both of those which are, and of those which are not, closely related to the more typical kinds of tannic acid. So far I have not been able to ascertain whether there is any one particular artificial oxidising process which will in each case give rise to the exact products naturally formed in the leaves themselves; but on the whole there is such a close correspondence between them that we cannot hesitate in concluding that the rich brown tints of autumn are mainly due to the oxidisation of the previously-existing more or less pale yellow colour of the chrysotannin group—a conclusion fully borne out by various independent facts. The difference in kind of tannic acid, and the absence or presence of any considerable amount of a chrysophyll substance, explains in a very satisfactory manner the difference in the tint of the leaves of different trees. Thus, for example, the quinotannic acid found in a comparatively pure state in the yellow leaves of the beech is changed by oxidisation into the fine red-brown colour of those leaves at a later period. This kind of tannic acid also occurs in the elm, but is there mixed with more or less of a chrysophyll, which turns to a duller brown; and thus we find the leaves of different elm-trees vary in tint, and are often of very dull brown colour. The leaves of the oak and Spanish chesnut contain gallo-tannic acid, and this, when oxidised, gives rise to a dull tint, like that seen in the faded leaves of those trees; and similar principles hold good in other cases.

As far as we are able to judge from the various facts described above, we must look upon the more characteristic tints of the foliage of early spring as evidence of the not yet matured vital powers of the plant. In summer the deeper and clearer greens are evidence of full vigour and high vitality, which not only resists but also actually overcomes the powerful affinity of oxygen. Later on the vital powers are diminished, and partial changes occur, but the affinity of the oxygen of the atmosphere is nearly balanced by the weakened but not destroyed vitality. At this stage the beautiful red and yellow tints are developed, which produce such a fine effect in certain kinds of scenery. Then comes more complete death, when the affinity of oxygen acts without any opposition, and the various brown tints of later autumn make their appearance, due to changes which we can imitate in our experiments with dead compounds. This may not be a pleasing way of viewing an otherwise charming subject, but I think we must all admit that it is substantially true.

H. C. SORBY

## *HUMAN ANATOMY AND PHYSIOLOGY*

*The Physiological Anatomy and Physiology of Man.* By Robert B. Todd, William Bowman, and Lionel S. Beale. A new edition by the last-named author. Part 2 of Vol. i. (Longmans and Co., 1871.)

THIS part corresponds to the third, fourth, and fifth chapters of the last edition; it is now divided into four chapters, one of which is devoted to a general consideration of the properties of tissue, and the others contain detailed accounts of the connective, cartilaginous, osseous, and adipose varieties. Dr. Beale seems to have spared no time or trouble upon the present part, which has been carefully revised throughout; a considerable amount of new matter has been added, and many parts, especially those relating to the development of the different tissues, have been entirely re-written.

The chapter on the forms of connective tissue is very full and complete, and compares very favourably with that in the last edition; descriptions of several well-marked varieties, which were before omitted, being now introduced, such as those occurring in the Whartonian jelly, the vitreous humour, and the cornea. With respect to yellow elastic tissue, Dr. Beale states that the fibres, usually considered to belong to it, which are found in tendons, and resist the action of acetic acid, are not of elastic nature at all, but are merely imperfectly-formed white fibrous tissue; and in his account of areolar tissue he strongly contests one of the most generally-received pathological doctrines of the day, that which supposes in many cases of degeneration that the interstitial areolar tissue of the organ is the active agent, becoming hypertrophied, and then contracting and compressing the structures in its meshes. Dr. Beale considers, on the contrary, that in most cases the areolar tissue is quite passive, and that the phenomena ascribed to it are really produced by the rapid multiplication of parts of white blood corpuscles which have passed through the walls of the blood vessels.

In his account of cartilage Dr. Beale dissents from the opinion held by some, that the capsule of a cartilage cell differs from the matrix in its origin and nature; he points out that in some cases there is no matrix, in others no cell-wall can be demonstrated as distinct from the matrix; and again, in others the capsule passes gradually into the matrix; and maintains that the matrix when present is entirely formed of old capsules, and is *not* developed independently of the cells. Fibro-cartilage and elastic cartilage are both well described; no mention at all of the latter form was made in the previous editions.

The chapter on bone contains a good account of its histological structure, but is chiefly interesting from the views put forward as to the mode of origin of the canaliculi. Virchow states that they are formed by the deposition of calcareous matter round processes radiating from corpuscles contained in the lacuna, while Kölliker thinks they are formed by resorption after the lacuna has been entirely surrounded by calcareous matter. Dr. Beale differs from both—he says the bone corpuscles of the lacuna have frequently no processes, and that when processes are present they are always much shorter and much less numerous than the canaliculi, and he points out that the formation of these little channels begins at their distal end, not at the end next the lacuna, as has been supposed; his own view is that in an early stage of the development of bone, it is all permeable to nutrient fluids, but that as calcareous matter is deposited this permeability gets restricted to constantly narrowing channels, which ultimately remain as the canaliculi, and are at first filled with soft matter (cartilage or membrane), which in fully formed bone dries and shrivels up, leaving the canaliculi as true tubes.

The concluding chapter, that on adipose tissue, is on the whole good, but in the account of its histological structure the impression is conveyed that an adult fat cell consists merely of an envelope containing oily matter—no mention being made of the fact that by proper treatment a nucleus also can be almost always demonstrated. Dr. Beale considers that the fatty matter contained in the cell is formed by the degeneration of the mass of "bioplasm," or "germinal matter," of which it was once entirely composed.

The part is illustrated by a large number of very good figures, and several full-page plates.

## *OUR BOOK SHELF*

*Elementary Treatise on Natural Philosophy.* By A. Privat Deschanel. Translated and edited by Prof. Everett, M.A., D.C.L., &c., Professor of Natural Philosophy, Queen's College, Belfast. In Four Parts. Part 2.—Heat. (London: Blackie and Son.)

THIS work is intended to be an elementary treatise on the science of Heat. The remarkably fine engravings that embellish it throughout, give it an air of reality which, unfortunately, is not generally possessed by English scientific books. Still, some of the original engravings might have been improved; for example, figs. 223, 240, 245, and 264 are peculiar, and do not represent what is likely to be seen in the laboratory. Having said this much in favour of Prof. Everett's translation, we cannot avoid making some unfavourable criticisms. We decidedly object to the numerous formulæ and equations which may almost be said to disfigure many of the pages; they are not sufficiently explained for a popular work, and might have been more compressed if intended for advanced scientific students. And seeing that formulæ and explanations usually vary inversely as each other within the same volume, we should have been pleased—indeed, we expected—to find as many of the former eliminated as possible. This expectation was occasioned by the translator himself, who complained that oftentimes we are confronted with "unexplained formulæ, which burden the memory without cultivating the understanding." Can Prof. Everett assert that he has explained the formula on page 362 of Part 2? Has he not rather fallen into the very error which he so ably deplores in his preface; failing to see amid V, KT, and other algebraic mystifications, that his H and h are not directly comparable: that the mercury in Gay-Lussac's tube is hot, while a barometer is generally cool? A student of Nature will scarcely be taught much that is satisfactory concerning Gay-Lussac's beautiful method of determining vapour densities, by being led away at once into intricate formulæ "which burden the memory, without cultivating the understanding." This one example will sufficiently indicate the fault which runs through the whole volume before us. Much new and valuable matter, albeit besprinkled with formulæ, has been added by the translator; and various passages in the original have been modified or otherwise corrected. But, though we have no hesitation in saying that the original has been thereby improved, yet the final result is neither remarkable for its novelty, nor edifying from its simplicity.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Thickness of the Earth's Crust

I TAKE in NATURE in parts. Your part for May last has just reached me in Calcutta, and is somewhat rich on the question of the thickness of the earth's crust; for three of its four numbers contain letters on that subject. First, my letter to you, p. 28; two critiques upon it from "A. J. M." and Mr. Green, p. 45; and a third from Mr. David Forbes, p. 65.

Reply to "A. J. M." I do not think it safe to draw inferences from a comparison the members of which differ so materially; a plate of lamp oil, probably a quarter of an inch deep, and the solid crust of the earth resting on a fluid mass 3,856 miles deep, supposing the crust, according to the thin-crust theory, to be 100 miles thick. To be sure, the motion in the experiment is very much greater than precession; but the depth of fluid is also very different, and the cases are not parallel. Moreover, in the one case, the oil rests on the plate, weighed down by gravity, and is easily carried round bodily with the plate. This is different to the hard crust rubbing over a depth of fluid. I see the writer, like M. Delaunay, relies much on the extreme slowness of the motion. More on this soon. We shall have to beware how we adopt the phrase "a rope of sand," for if we use the rope *slowly* enough, it may become in our hands a "rod of iron."

Reply to Mr. Green. By "pushes" I did not mean mechanical knocks, but merely geometrical movements. The pole of the earth goes round the pole of the ecliptic in a circle in about 26,000 years. I divided this circle into a multitude of little pieces. Nor was my description meant to represent this motion as being by fits and starts, but only in portions, to assist the mind in a popular explanation, not of precession, but of the thing I wanted to be understood, the slipping of the hard crust over the fluid. I think it is very likely my own fault that my description has been misunderstood.

Reply to Mr. David Forbes. Mr. David Forbes glides out of the discussion on the plea, that after all I make out that Mr. Hopkins' calculations were based only on "an idea." But so is M. Delaunay's opinion based on an idea. We must start with an idea. Ideas are of two kinds, sound and unsound; and if the idea we rest upon is sound, we cannot possibly do better than build upon it. M. Delaunay thinks his idea is supported by an experiment. I have tried to find a description of the experiment, but without success. I altogether doubt whether any experiment with models can be devised to lead to trustworthy results, regarding such a huge mass as the earth affected by such slight motions as precession. Mr. David Forbes says he went to the Royal Society to hear a paper of mine read on the constitution of the solid crust of the earth. It was natural that he should suppose from the title, that it might bear upon this question of the thickness of the crust. But this is not the case at all, as he would find. M. Delaunay's strictures on Mr. Hopkins' investigation seemed to me so important, that I did *drag in*, I may say, an allusion to them in a note. It is very likely, that, as my paper was almost entirely a calculation of mathematical formulæ and their numerical application to the pendulum observations lately made in India, the paper itself was not read aloud, and that conversation turned on the incidental note.

I propose now to view the subject in a new light, and to begin *de novo*; and I feel confident that your readers will see that there is more to be said for Mr. Hopkins' method than they have by this time been led to think.

The precessional motion is no doubt extremely slow: and that because the precessional force is extremely small. But the particles of the earth's mass have a good deal more to do in the matter than to partake of this small motion. Every twenty-four hours they have to undergo a strain, first this way and then that, such that I believe no fluid, however viscous, could sustain. Though the precessional force is so minute, it is the resultant or residuum of an almost infinite number of other disturbing forces nearly balanced; as I proceed to show.

Let G be any point in the earth's mass; $b$ its distance from the centre; $a$ the radius of the earth; $c$ the distance of the sun; $\theta$ the angle between $b$ and $c$; S the sun's mass. Then, considering the sun's action by itself, its attraction on G I resolve parallel and at right angles to $c$, and from the former position subtract the sun's attraction on the *disturbing* forces on G which are wanted. These are, neglecting the smallest quantities,

$$\frac{4Sb}{c^3}\cos\theta \text{ and } \frac{Sb}{c^3}\sin\theta.$$

For the sake of a name I will call the plane through the earth's centre, at right angles to the line joining it and the sun, the Boundary Plane, as it intersects the surface almost exactly in the boundary line between sunlight and darkness. It will be observed that at this plane the first of these disturbing forces equals zero, and is positive on the sun side of the plane, and negative on the opposite side. The amount of this force is the same at all points lying in any plane parallel to the boundary plane, and the aggregate of the positive forces on the one side, and of the negative on the other, would be in each case a force acting through the earth's and sun's centres, but for the slight deviation of the earth's figure from a sphere, and the consequent arrangement of its mass. Suppose, for argument's sake, that the particles of the earth's mass are held invariably together as a rigid body; then the disturbing forces parallel to $c$ will amount to an aggregate positive force, and an aggregate negative force, tending to separate the two parts of the earth formed by the boundary plane. These forces are equal to each other, and pass nearly through the earth's centre, at opposite and equal distances from it. They form, in fact, a mechanical "couple," and twist the earth round some diameter lying in the boundary plane. The arm of this couple is a minute quantity depending upon the ellipticity of the earth's figure. Hence the movement of the couple, from which precession and nutation arises, is a minute quantity, while the *force* of the couple (which is the tension of the earth's mass perpendicular to the boundary plane) is not of that minuteness. The other disturbing forces, represented by the second formula, all tend towards $c$, and compress the mass. They would all balance each other, were the earth spherical. The resultant of these forces on the sun side will be a minute quantity of the order of the ellipticity, and will act at a certain distance from the centre; and the resultant on the opposite side will be an equal force, acting at an equal distance from the centre, but in an opposite direction; so that again there will be a couple of minute power, assisting with the other couple to produce the combined motion of precession and nutation. In this case, although the forces nearly balance each other on each side the boundary plane, parallel to it, and but a small resultant follows, yet the particles of the mass have to undergo the compression from opposite sides, as in the other case they have to sustain the tension caused by the opposing forces tending to separate the two halves of the earth.

The moon will produce forces precisely similar to these, and a little more than twice their amount. The forces of the sun and moon do not come to their maxima and minima at the same time, except at new and full moon. At other times they partly conspire or counteract each other according to their position. Sun and moon together will produce most irregular cross-strains through the mass, if the particles are held together by any degree of rigidity.

It will be seen, then, that as within twenty-four hours *every* particle of the mass in its rotation is carried through the boundary plane *some* days in the year, and most of the particles every day, every part of the mass will be periodically subject to the maximum strain I have described as taking place at that strain, and most parts twice a day, as well as the compressing force at right angles to $c$.

The same will be the case, and at different times, except at new moon and full moon, at the boundary plane appertaining to the moon. The result is that all parts of the mass are perpetually undergoing considerable cross-strains, for the forces of tension and compression will not relieve each other, as they act at right angles to one another.

If the mass of the earth were not rigid, but sufficiently elastic, like, for instance, a globe of india-rubber, the particles would yield to their small disturbing forces; and the result would be that each particle, as it arrived by rotation at each point of its circuit, would move in proportion to the force acting on it, one way or the other. In this case there would be nothing to cause the axis to move; the earth would steadily revolve, and no precession or nutation would occur. The whole mass would, as it were, breathe, heaving its surface and drawing it in again in a complicated undulation.

But observation shows that the earth *has* precession and nutation; and therefore the mass cannot be thus elastic.

If it be only partially rigid, then there would be a corresponding degree of yielding; but precession and nutation would still be produced, and a strain, in a somewhat diminished degree, affect the mass

Now, mathematical calculation made on the hypothesis of the earth's mass being absolutely rigid, and that throughout, shows that the annual precession would be 51·3566″.* Astronomical observation shows that the precession is actually 50·1″. The remarkable nearness of these results is sufficient proof that the earth's mass is not the limp thing some take it to be; all viscous-fluid from only 100 miles down to the centre, moving so slowly, that it gives inertia to the hard crust (supposed thin) as if it were all solid! It is more like the highly rigid mass which Sir William Thomson has shown it to be from other considerations.

JOHN H. PRATT

Calcutta, July 15

* See this worked out in my "Mechanical Philosophy," 1st edit. p. 562, 2nd edit. p. 540.

### Meteorology in South Australia

As it may be interesting to some of your English readers to hear something of natural phenomena in such an out-of-the-way part of the world as South Australia, I forward a description of three very fine meteors which have lately been seen here, as well as a splendid display of Aurora Australis.

On January 5th, 1871, at about half-past nine P.M., I observed a splendid meteor. It appeared at first like a fixed star about three times as large as Jupiter, or say six or seven inches in diameter, and was probably about 15° above the horizon, or nearly of the same apparent height as a large star which was just below the planet Jupiter, a little to the west of him and within half an hour of the meridian. The meteor, which was very brilliant, somewhat of the appearance of Jupiter, remained apparently stationary for, at least, five seconds; it then gradually began to move from a due north position to a direction about S.S.E., and in a horizontal line; it then burst into several smaller meteors and went out, having lasted fully twenty seconds altogether. The moon was shining brightly at the time, being a few days off the full.

On the same night, and at about the same hour, a large meteor was seen by a survey party at Hookina, a place about 400 miles north of Adelaide. The surveyor in charge of the party (Mr. Hamilton) from whom I obtained the particulars of this meteor, says he was facing the east when he observed it going from N.E. to S.E., describing a large arc at an apparent elevation of 20°. He describes its colour as greenish, and so bright that it almost overpowered the light of the moon. It ultimately burst with a loud explosion into a number of fragments of red and blue colours, and the earth was felt to tremble as though a shock of an earthquake had occurred.

On the 25th March last, at about twenty minutes to three o'clock in the afternoon, I observed a meteor in the full blaze of the sun. It appeared like a bright brass-coloured ball of fire, shooting through the sky like a rocket; it seemed to have a green and blue light round a central brass-coloured nucleus. The meteor appeared about three inches in diameter; it had a whitish comet-like tail, about three feet long, and it came from the N.N.E. and travelled downwards towards the S.S.W., so that as I was looking south it appeared to come over my left shoulder. It lasted about ten seconds, then burst without noise, and became dissipated. This meteor seemed to fall at about 15° from the horizon.

In the S. A. "Register," a few days after seeing the last described meteor, it was stated that a surprising sound was heard at Point Macleay, and other places about fifty or sixty miles to the south-east of Adelaide as of the firing of cannon, and the correspondent of that paper at Mannum (a place on the River Murray, about sixty miles east of Adelaide) writes as follows:—

"On Saturday last (25th March) at exactly 2.45 P.M., I was looking down the Murray River, when suddenly my attention was attracted by a large ball of fire falling from the heavens in almost a perpendicular course. The lakes are from here in the direction which it indicated—almost due south—so that I have no doubt the extraordinary phenomenon mentioned as having occurred on the shores of Lake Alexandrina, may have arisen from one of the causes assigned, viz., a falling meteor or an aerolite. What I saw was evidently the explosion immediately preceding the fall, and it presented the appearance of a luminous meteor." . . . .

The display of "aurora australis" which I observed on the 23rd March last, commenced at about eight o'clock P.M. It increased in brightness till eleven o'clock, when it gradually faded away. At about two o'clock A.M., while at a ball, I came out on the balcony and observed the whole southern sky lighted up by a most gorgeous display of aurora. It occupied about 70° or 80° of the horizon, extending from about S.S.E to S.W., and reached to a height of say 60° above the horizon. It was of a splendid red rose colour and streaked with beams of white light at various distances apart—say two bands of white in every 10°. These white bands appeared about two feet to five feet wide, which would answer to say 5° observed by the eye alone. The display was so bright that by placing my hand with the fingers apart at about two feet from a lady's white dress, I could distinctly see the shadow of each finger. This aurora was also seen in Victoria and New South Wales.

I may mention that Adelaide is situated in south latitude about 35°, and longitude 138° 40′

M. M. FINNISS

Adelaide, June 19

### The Solar Aurora Theory

IN the very interesting lecture of Mr. Lockyer upon the recent solar eclipse which has just appeared in NATURE, he says, speaking of the green line layer above the hydrogen, "Here obviously we have, I think, merely an indication of another substance thinning out, in spite of the extraordinary suggestion which was put forward that the corona was nothing but a *permanent solar aurora.*"

I agree entirely in this view except as to what would seem to be implied by the expression *in spite of.* I fail to see any inconsistency between the idea of a substance "thinning out" and a permanent solar aurora.

What I intended in adopting and endorsing this auroral hypothesis was simply this: to express the belief (which I still hold, though with no great tenacity) that the substance which composes this green-line layer is also found in the upper regions of our atmosphere in a state of almost inconceivable tenuity, and at an elevation of certainly more than one hundred miles; and, further, that the peculiar filamentary and radiant structure of the corona, and very possibly its luminosity to some extent, are due to solar forces closely analogous to those which produce our terrestrial auroras.

Or in other words, that an observer, at the planet Mars for instance, looking at the earth during a period of auroral activity would see its poles capped by a corona in substance, structure, and to some extent in origin, closely analogous to that which is permanent around the sun.

And if we grant the identity of the 1474 line with that which is, to say the least, so closely coincident with it in the auroral spectrum, it is difficult to see why the hypothesis should be considered "extraordinary," or *per se* improbable.

That the enormous chemical, thermal, luminous, and magnetic activity of the solar surface should be unaccompanied by manifestations of what we call electric energy seems far more unlikely than the contrary; and if such energy operates we should naturally look for phenomena, the counterparts of those by which it shows itself here, but on the *solar* scale of course.

As to the identity of these lines, however, there may fairly remain some doubt. This line in the spectrum of the aurora is so rarely seen, so faint, and so difficult of observation, that, although the few observations thus far obtained show even a surprising agreement with each other and with this idea, it is safer to maintain a cautious reserve.

C. A. YOUNG

Dartmouth College, U.S.A., August 16

### Lecture Experiments on Colour

FOR some time I have been taking an active interest in the phenomena of colour, and have read with much pleasure the papers of Mr. Strutt and other gentlemen, and the abstract of the interesting lecture recently delivered at the Royal Institution by Prof. Maxwell. I have repeated many of the experiments of these observers, and have successfully exhibited them in public in a modified form, and in a way which can be readily repeated by other lecturers without the aid of the elaborate contrivance used by Prof. Maxwell. The following experiments make no pretension to rigid accuracy, but are merely described as striking lecture-table demonstrations of well-ascertained but little known scientific facts. I use the lime light for their exhibition, as it suits my convenience better than the electric light, though many lecturers prefer the latter.

A beam of light from the lantern is passed through a slit, focussed by a lens, refracted by a disulphide of carbon prism, and the spectrum exhibited in the usual way. A flat cell containing a solution of potassium permanganate is next placed in front of the slit. With a weak solution and narrow slit a series of black bands are produced in the green part of the spectrum, but with a stronger solution the green and yellow are completely cut out, allowing only the red and deep blue lights to pass. On widening the slit these bands of coloured light of course increase in width also, gradually approaching each other until they overlap, producing a fine purple by their admixture.

If the experiment be repeated, substituting for the permanganate an alkaline mixture of litmus and potassium chromate in certain proportions, only the red and green light are transmitted, the blue, and especially the *yellow*, being completely absorbed. On widening the slit as before, the red and green bands overlap, and produce by their union a very fine compound yellow, while the constituent red and green are still visible on each side. The effect is most striking when by the widening of the slit a round hole is exposed in its place, when there appear on the screen two circles, respectively green and red, producing bright yellow by their admixture. This experiment is the more striking as it immediately follows the process of absorbing the simple yellow. The mixture above described (suggested by Mr. Strutt) answers better than a solution of chromic chloride.

Of course, it is a well-known fact that all natural yellows give a spectrum of red, yellow, and green, and a common effect illustrating the compound nature of yellow is noticed when exhibiting a continuous spectrum on a screen. When the slit is narrow the green is very fully developed, and only separated from the red by a very narrow strip of yellow, while on gradually increasing the width of the slit the red and green are seen to overlap, producing the brilliant yellow we generally notice. Thus the purer the spectrum the less yellow is observed.

If the continuous spectrum be produced with a quartz prism, a little management and adjustment of the distance of the screen will cause the two spectra to overlap, so that the red of one may be made to coincide with the green, blue, or any desired tint of the other. The same result is obtained by employing two slits at the same time, the distance between which can be adjusted. By this means two spectra are obtained simultaneously, any portions of which can be made to coincide.

I have not tried to use a double refracting Nicol's prism, as is suggested by Mr Strutt in the number of NATURE for June 22.

A saturated solution of potassium chromate absorbs all rays more refrangible than the green, while a solution of ammonio-sulphate of copper stops all *but* the blue and green. These statements may be proved by placing flat cells containing the liquids in front of the slit of the lantern, and on placing one cell in front of the other in the same position the green light only is transmitted. This experiment serves to explain the reason that the mixture of yellow and blue generally results in green, all other rays being absorbed by one or other of the constituents.

By placing the two cells in front of separate lanterns, and throwing discs of light on the screen, a beautifully pure white is produced where the blue and yellow overlap.

I employ one lantern only for this experiment, using *two* focussing lenses side by side to produce the overlapping circles of light.

I also employ a cell with three compartments, containing solutions of aniline red, ammonio-sulphate of copper, and a mixture of potassium chromate with the last solution, and projecting images on the screen by means of three lenses fitted on the same stand but capable of separate adjustment. I can thus exhibit overlapping circles of brilliant red, blue, and green light, which produce a perfect white by their admixture, while at the same time there is seen the compound yellow produced by the union of red and green, the purple arising from the red and blue, and a colour varying from grass green to sky blue produced by the combination of the green and blue light. This experiment has the advantage of exhibiting *at the same time* the three primary colours—red, *green*, and blue—the compound colours produced by their mixture, their complementary tints, and the synthesis of white light.

The flat cells mentioned are made by cutting thin pieces of board to the desired shape, and cementing pieces of window glass on each side by means of pitch.

Sheffield, June 26 ALFRED H. ALLEN

### Mr. Stone and Prof. Newcomb

I AM sorry, indeed, that anything in my answer to Prof. Newcomb should be unsatisfactory to Mr. Stone. It will certainly be hard if after drawing upon myself Prof. Newcomb's indignation by advocating Mr. Stone's claims, I should find that I have unwittingly offended Mr. Stone also.

It is the misfortune of a writer on science that he has often to deal with overlapping claims; and when he adheres unflinchingly (as I have always done) to what he regards as the strict line of truth, he cuts off a little from the claims on either side, and so offends both claimants. I have found myself in the same difficulty as respects the work of Dr. De la Rue and Fr. Secchi, in 1860, and I fear the result may have been the same in that case also.

In another case, that of Mr. Lockyer and his fellow-workers in spectroscopic solar researches, I freely admit that what I regarded as the line of truth when I wrote "The Sun," I now no longer regard as strictly such, evidence having been produced which has satisfied me to that effect. Even in this case, however, I have in the first place very little to correct, and in the second I am by no means certain that I shall be able to satisfy all or any of those concerned.

Fortunately or unfortunately, the writer who cannot please all proves equally his desire to do justice to all by leaving all dissatisfied. This is commonly the fate of the true neutral. I must confess, however, that I cannot see what reason Mr. Stone has for being dissatisfied, since I have ascribed to him, much to Prof. Newcomb's dissatisfaction, the final and complete solution of a problem which both have dealt with very ably. I am still waiting to hear the nature of Prof. Newcomb's objections. Whatever they may be, I am assured of *this*—that in defending (if I can defend) my own work, I shall be advocating Mr. Stone's claims. I hope that in so doing I shall not very grievously offend that gentleman, towards whom I entertain the most friendly feelings.

RICHARD. A. PROCTOR

### Saturn's Rings

THE reviewer of Lieut. Davies's work on Meteors has somewhat misunderstood the extent to which I have been indebted (in preparing my treatise on Saturn) to Prof. Maxwell's excellent "Essay on the Saturnian Ring-System." I have quoted in all two and a half lines from that essay, with proper reference to it, and I have devoted one-third of a page to summarising the most important section of the essay. All the rest of my chapter on the Nature of the Rings was written before I had seen Prof. Maxwell's contribution to the meteoric theory of the ring-system. I may add that every result in Saturn, which is not distinctly referred to authority, or else obviously common property, has been worked out by myself, as my note-books will abundantly testify. RICHARD A. PROCTOR

Brighton, August 28

### A Rare Phenomenon

SUNDAY, the 13th August, and several days before, having been very hot and dry, a great deal of dust was suspended in the atmosphere, which caused without doubt the intense red colour of the setting sun, and might contribute to the phenomenon I am about to describe. This phenomenon may easily be understood by means of a globe bisected by a meridian plane, one half of it representing the celestial vault. Beginning at the eastern end of the equator, the space between the 40th and 50th degree of longitude may be tinged with reddish grey; then the space between the 60th and 75th degree, further, that between the

85th and 105th, and finally that between the 115th and 140th degrees. But the intensity of colour must vary inversely to the breadth of the stripes, and the three stripes left between the red ones be filled with a pretty vivid blue. This hemisphere placed upon a table with its southern pole pointing towards sunset will afford a tolerable portrait of the aspect of the sky as it appeared immediately after sunset, and continued unchanged for more than a quarter of an hour. The stripes were not visible near the horizon, but were very distinct at an altitude of about fifteen degrees, and almost disappeared about the zenith. No cloud was seen during the occurrence of the phenomenon.

These stripes were certainly parallel in reality, and their apparent divergence may be accounted for by perspective. The reddish stripes may owe their colour to sunlight reflected back from the particles scattered in the atmosphere. But why did the celestial vault show so distinct a blue colour in the intervening bands? Yet, probably, this phenomenon is more easily to be explained than the infinite variation of evening colourings that want a valid explanation to this day.

Magdeburg, August 19 A. SPRUNG

## *THE AURORA*

THERE was a very fine display of aurora here on the night of the 21st. It commenced to be visible about 9.30 P.M., reached its maximum about 11, and faded suddenly away about 11.30. In appearance it was of a silvery white, without a trace of that rose colour which characterised the three great displays of last autumn. The main portion of the light was in the north-western quarter of the heavens, and it was sufficiently strong to see large print by. Extending from the north-west and reaching the north-eastern horizon arose three luminous arches concentric with each other, the 1st about 15° altitude, the 2nd about 25° altitude, and the 3rd about 40° altitude. These were connected by radial tongues of light which were ever changing their height. There was another marked and isolated nucleus about and around *α* Lyræ.

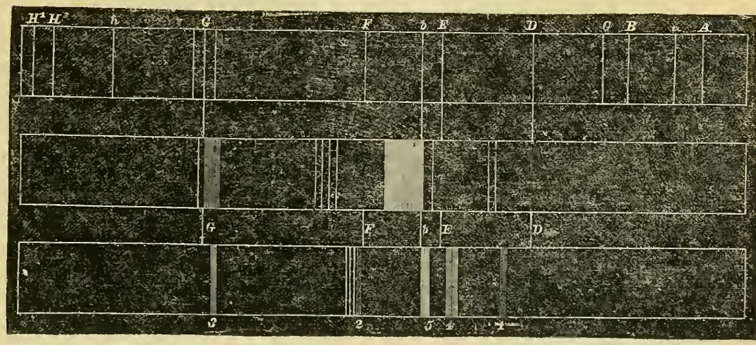
At about 10.45 P.M. there were most curious rays shot up from the arches in the north, and concentric with them These shooting arches, if I may call them so, had at the horizon an apparent angle of about 150° to 180°, but as they approached the nucleus in Lyra, they contracted and lost themselves in sheets of white light. On applying the spectroscope I found one bright line visible all over the heavens excepting on the south horizon for an altitude of about 25°. The spectrum obtained on the north-west gave five bright lines, of which I send a drawing.

From want of convenient measuring apparatus I had resource to the method of superposed spectra. The light I chose for comparison was that of a tallow candle, from which I got the bright lines of sodium and carburetted hydrogen. The instrument I used was one of Browning's direct vision spectroscopes—an instrument that gives the best results with the minimum amount of light. Of the bright lines, two were strong, one was medium, two were very faint. In the accompanying map I have put the solar spectrum at the top and carried the chief lines down for comparison. In putting numbers to the lines I have been directed by their degrees of intensity.

SUN . . .

CANDLE . . .

AURORA . . .




COMPARISON SPECTRA OF SUN, AURORA, AND CANDLE

No. 1 is a sharp, well-formed line, visible with a very narrow slit.

No. 2. A line very slightly more refrangible than F. The side towards D is sharp and well-defined, while on the other side it is nebulous.

No. 3. Slightly less refrangible than G, is a broad ill-defined band only seen with a wide slit.

No. 4. A line near E, woolly at the edges, but rather sharp in the centre. This should be at or near the position of the line 1474 of the solar corona.

No. 5. A faint band coincident with *b*, extending equally on both sides of it.

The barometer stood at 29·574 in.; the thermometer at 61°·3. A gentle wind was blowing from the south-west, and the sky was free from clouds.

Observatory, Dun Echt, Aberdeen LINDSAY

## *FRUIT CLASSIFICATION* *

DR. DICKSON referred to the confessedly unsatisfactory state of fruit-classification, and to the very unnecessary extent of the existing terminology, which is further complicated by a considerable amount of variance among botanists as to the precise application of several of the terms employed. He was of the opinion, which he believed to be a growing one among botanists, that the most convenient method of classification was, in the first place, rigorously to restrict the definition of a "fruit" to the mature or ripe pistil, excluding from that definition the modifications of accessory parts or organs, which, in many cases, are correlative therewith; and, secondly, to base the primary classification upon the general character of the modification undergone by the parts of the pistil in ripening, treating as of minor importance the characters involved in the description of the flower, such as the superior or inferior position of the ovary, &c.

The classification which Dr. Dickson suggests for the consideration of botanists approaches most nearly to that indicated by Schacht in his "Grundriss," of which, indeed, it may be viewed as a modification and expansion. Schacht grouped fruits under three heads—(1) Capsular fruits which dehisce to allow the seeds to escape; (2) splitting fruits or Schizocarps, which

* "Suggestions on Fruit Classification." By Alex. Dickson, M.D., Regius Professor of Botany in the University of Glasgow. Read before the British Association, 1871.

break into pieces which do not allow the escape of the seeds; and (3) fruits which neither dehisce nor fall into indehiscent pieces, including Berries, Drupes, and Achenes. As this last group is very heterogeneous, Dr. Dickson prefers to consider Berries, Drupes, and Achenes severally, as forms of equal value with Capsules or Schizocarps, and therefore would divide fruits into five groups, viz., "Capsules," "Schizocarps," "Achenes," "Berries," and "Drupes," as will be seen in the following table :—

| | | |
|---|---|---|
| I. CAPSULE — Dry, dehiscing, to allow the seeds to escape. | Simple. (Probably the two forms included under this head should be embraced by a single term.) | 1. *Follicle.*—Dehiscing by one suture, usually the ventral: *e.g.*, Aquilegia, Caltha, Magnolia. |
| | | 2. *Legume.*—Dehiscing by both sutures: *e.g.*, Cytisus, Vicia, &c. |
| | Compound. | 3. (Name wanted) Seeds escaping by longitudinal rupture of the wall of the capsule (dehiscence by valves, teeth, or pores): *e.g.*, Brassica, Viola, Rhododendron, Iris, Lychnis, Papaver, Campanula, &c. |
| | | 4. *Pyxidium.*— Seeds escaping by transverse rupture of the wall of the capsule (dehiscence circumcissile): *e.g.*, from superior ovary, Anagallis, Plantago, Hyoscyamus, &c.; from inferior, Bertholletia, &c. |
| | | 5. *Regma.*—Seeds escaping by rupture along the inner angles of the lobes into which the fruit separates: *e.g.*, Geranium, Euphorbia, &c. |
| II. SCHIZOCARP.—Dry, breaking up into indehiscent pieces. | Breaking longitudinally into indehiscent cocci. | 6. *Carcerulus.*—Lobes not hanging from forked "Carpophore:" *e.g.*, Tropæolum, Borago, &c. |
| | | 7. *Cremocarp.* — Lobes separating from below, and, for a time, hanging from extremities of forked "carpophore:" *e.g.*, (from superior ovary) Acer, and (from inferior ovary) *Umbelliferæ.* |
| | Breaking transversely into one-seeded joints. | 8. *Lomentum.*—*e.g.*, Ornithopus, &c. |
| | Breaking first longitudinally, then transversely | 9. (Name wanted) *e.g.*, Platystemon. |
| III. ACHENE. — Dry, indehiscent, not breaking up. (Probably the names applied to the different forms should be abolished and the term Achene applied to all.) | Superior. | 10. *Achene* (in restricted sense.—Pericarp not adherent to seed: *e.g.*, Ranunculus, Rumex, Ulmus, Fraxinus, &c. |
| | | 11. *Caryopsis.*—Pericarp adhering to seed: *e.g.*, Gramineæ, &c. |
| | Inferior. | 12. *Cypsela.*—Pericarp not much indurated: *e.g.*, *Compositæ, Valerianaceæ*, &c. |
| | | 13. *Glans.*—Pericarp hard: *e.g.*, Quercus, Castanea, Fagus, Corylus, &c. |
| IV. BERRY. — Seeds imbedded in pulp. As a rule indehiscent. | Outer portion of pericarp delicate (thin-skinned). | 14. *Uva* —Superior: *e.g.*, Vitis, Solanum, &c. |
| | | 15. *Bacca* (in restricted sense).—Inferior: *e.g.*, Ribes, Vaccinium, &c. |
| | Outer portion of pericarp firm, leathery, or hard (thick-skinned). | 16. *Amphisarca.* — Superior: *e.g.*, Adansonia, Passiflora, &c. (Citrus should be included here.) |
| | | 17. *Pepo.*—Inferior: *e.g.*, Cucurbita, Cucumis, &c. (Punica should be included here.) |
| V. DRUPE. — Endocarp distinctly defined & more or less indurated. Outer portion of pericarp of variable consistence— fleshy, leathery, or fibrous. As a rule, indehiscent. | One-stoned. (Probably the two forms included under this head should be embraced by a single term.) | 18. *Drupe* (in restricted sense).—Superior: *e.g.*, Prunus, Cocos, &c. |
| | | 19. *Tryma* —Inferior: *e.g.*, Juglans, Viburnum, &c. |
| | Two or more stoned. (Probably the two forms included under this head should be embraced by a single term.) | 20. (Name wanted) Superior: *e.g.*, Ilex, Empetrum. |
| | | 21. *Pome.*—Inferior: *e.g.*, Pyrus, Cratægus, Sambucus, &c. |
| | With one plurilocular stone. | 22. (Name wanted) *e.g.*, Cornus. |

As the modifications undergone by the fruit in ripening stand in direct relation to the dispersion of the parts by which the plant is disseminated, probably the most philosophical method of classifying fruits would be according to the nature of the parts disseminated. To carry out this principle rigorously, however, would lead to practical difficulties, far outweighing any advantage gained. At the same time, it is evident that the foregoing classification satisfies, in a general way, the conditions of such a method; thus, in capsules and berries, the *seeds*, as a rule, are the ultimate parts disseminated; in Drupes, the *stones*; in Schizocarps, the *mericarps* or *joints*; and in Achenes, the *fruits as wholes.* As refractory exceptions, however, may be mentioned, those cases where the seed *minus* its testa is the part ultimately disseminated, for example, in *Oxalis*, where, on dehiscence of the capsule, the elastic testa becomes ruptured, violently expelling the body of the seed with the tegmen; or in the so-called drupaceous seeds (*e.g.* in *Punica*) which are doubtless devoured by birds, and, after digestion of the pulpy testa, the body of the seed with the hard tegmen is evacuated, and dissemination occurs. Or, again, in such a drupe as the apple, where the induration of the endocarp is slight, we have the fruit behaving as a berry, and dissemination taking place by means of the seeds.

Some botanists may perhaps be surprised to note the omission of the terms *siliqua* and *silicula*, so universally employed to designate the fruits of *cruciferæ*. A little reflection, however, is sufficient to make it evident that, if distinctions so trifling in character, as those which separate these fruits from other valvular capsules, were consistently carried out in practice, the terminology would become altogether intolerable. A similar argument may be adduced in favour of the suggestion made in the foregoing table, as to the propriety of devising some common term which will supersede those of *follicle* and *legume.*

## NOTES

WE are happy to say that the Eclipse Committee has been perfectly successful in its attempt to send a complete set of instruments to Australia; and a code of instructions is being drawn up in order to ensure similar observations being made at all stations.

IT is now announced that the Swedish Government has abandoned the intention of establishing a colony in Spitzbergen for permanent scientific observation, mainly, it appears, in consequence of jealousies on the part of the Russian Government.

THE autumn meetings of the Iron and Steel Institute were commenced at Dudley on Tuesday morning, under the presidency of Mr. Henry Bessemer. About 250 members of the Institute were present, and during the course of the proceedings, the secretary announced that forty-seven new members had been elected, amongst whom were the Earl of Dudley, and Sir Antonio Brady, of London. The President, in opening the meeting, described the locality in which it was assembled as one of the most interesting districts this country presented to the iron manufacturers—a district, indeed, in which they might say that the great iron industry took its rise; its very cradle and birthplace. Mr. H. Johnson, mining engineer, read a paper "On the Geological Features of the South Staffordshire Coalfield, in Special Reference to the Future Development of its Mineral Resources." The South Staffordshire coalfield, one of the oldest in Great Britain, he said, was remarkably rich in coal, ironstone, and limestone. The secretary then read a paper by Mr. John Giers, Middlesboro', "On the Ayresome Ironworks, Middlesboro', with Remarks upon the Alteration in the Size of Cleveland Furnaces during the last Ten Years." A paper was read by Mr. Thomas Whitwell, Thornaby Ironworks, Stockton, "On further Results from the Use of Hot Blast Fire, brick Stoves." Mr. T. W. Plum, Shifnal, Salop, read a paper "On the Advantages of increased Height of the Blast Furnaces in the Midland District." The last paper was read by Mr. J. Lowthian Bell, Newcastle, "On Mr. Ferries' Self-coking Furnace." A large party then proceeded by train to Tipton, where the ironworks between that town and Wolverhampton were visited, and a pleasant afternoon was spent in investigating the mechanical processes in some of the largest works of South Staffordshire. The meetings conclude on Friday.

In his last weekly return, the Registrar-General refers to the westward advance of Asiatic cholera as investing with more than ordinary interest Dr. Frankland's usual monthly report upon the quality of the metropolitan water supply. The water supplied by the New River and Kent Companies is again reported freest from organic impurity, that of the East London and Chelsea Companies the most impure. With reference to the advantage which would be derived from a general application of Dr. Clark's softening process to the London water supply, the Registrar-General adduces the following valuable facts, which have been communicated by Mr. Robert Rawlinson, C.B., C.E. :—"The average daily water supply to the metropolis was 111,292,104 gallons in June, and 112,107,697 gallons in July. Now in each million gallons of these waters there is about one ton of bi-carbonate of lime, or $111\frac{1}{4}$ tons in June and 112 in July. About two-thirds of this weight of lime or chalk would be removed by Dr. Clark's softening process—that is, in June 74 tons, and in July about 75 tons. In each year about 25,000 tons of useless lime would be removed from the metropolitan waters by the simple and easy process now in use at Canterbury." The Registrar adds: "This riddance of the foreign matter, which deprives water of some of its cleansing properties, is in itself an advantage; but, besides this, the fine precipitate of chalk carries down with it suspended impurities and probably frees it from choleraic and other contagions. It is a most effective filtration." It is a comfort to know that the working classes are beginning to feel their strength. When that is put forward it is certain to be in the direction of sanitary and educational measures. The Registrar-General appends to his report a concise sketch of the steps proper to be taken in view of the threatened epidemic.

A correspondent at St. Andrews informs us that Professors Helmholtz, Huxley, Sylvester, Peters, Tait, Wyville Thomson, and Crum Brown, with several other *savans*, are now in that city, where there appears to be a sort of after-glow of the British Association. Our correspondent remarks on some points of interest in connection with the recent meeting of the Association. There was an unprecedented number of senior and second wranglers and Smith's prizemen in attendance in Section A. Prof. Helmholtz and Baron Liebig, M. Dumas, and Profs. Poggendorf, Bunsen, and Hofmann, all expressed great regret that they were unable to attend. Among other distinguished men present under circumstances, in some cases, of great difficulty, were Prof. Delffs, of Heidelberg, Dr. Baunhauer, of Haarlem, Drs. Anderson, Stenhouse, and Apjohn, Prof. Williamson, and others.

A correspondent of the *Times* writes that on Monday, at 3.45 A.M., while at Worthing, he felt a distinct shock of earthquake. "My first impression," he says, "was as if some person in the room above had fallen heavily on the floor, again and again. A second followed in about a minute and a half, but of less violence; each was accompanied by a low murmur as of a distant waggon. Again last (Tuesday) night, shortly before midnight, another but slighter shock was felt. On the first occasion many people were much alarmed, and, until able to compare notes, attributed the unusual sounds to a variety of causes. One gentleman drew a sword and searched his house for burglars. A family of unprotected females found relief in hysterics; but, almost to a minute, all were agreed in their statement of the time of the disturbance."

Late advices from Captain Hall's expedition in the *Polaris* state that the party reached Newfoundland in good condition, and was received with the utmost attention by the authorities. Captain Buddington, the sailing and ice-master of the expedition, has resigned, and will probably be replaced at Disco by Captain Richard Tyson. The United States frigate *Congress* has left New York with additional supplies of provisions, coal, &c., for the *Polaris*, and will proceed by the most direct route to the depôt agreed upon in Greenland. A number of persons accompany this vessel, among them Mr. Bryan, the astronomer of the expedition, and several gentlemen who will return in the *Congress*.

The total expenses of the Mont Cenis Tunnel amount to 65,000,000f., of which 20,000,000f. are to be contributed by the Railway of Northern Italy, and more than 25,000,000f. by the French Government. The masonry of the tunnel is reported to be excellent throughout, and no inconvenience whatever from smoke, steam, or mephitic air is apprehended.

Sir R. Murchison has received a letter from Dr. Kirk, British Consul at Zanzibar, in which he states that Dr. Livingstone is moving slowly but safely, evidently feeling his way, and "determined to leave little doubts behind him this time."

An association for the promotion of scientific instruction among the working classes, in connection with the Government Science and Art Department, has been formed at West Bromwich, and on Monday evening the movement was formally inaugurated at a meeting held in the schools in Bratt Street. The classes will open shortly for a winter session.

Co-operation on the part of the Dominion of Canada in the storm-signal observations of the United States commenced on the 15th of last month. Telegraphic reports and communications will be made from a number of stations in the Province of Quebec, and published from Washington with the regular series, the observations from the United States being telegraphed back in return. Dr. Smallwood, the well-known meteorologist of Montreal, is in charge of a central office in that city, where the local reports are to be concentrated, and whence they are to be communicated to Washington, and to whom the returns are to be transmitted.

A natural science demyship, of the annual value of 95*l.* and tenable for five years, will be awarded at Magdalen College, Oxford, in October next. The examination will commence on October 3, and candidates must call on the president on the day previous. This examination will be held in common with Merton College, where a Postmastership, of the annual value of 80*l.* for five years, will be awarded at the same time and with the same papers. Each candidate will be considered as standing in the first instance at the college at which he has put down his name, and, unless he has given notice to the contrary, will be regarded as standing at the other college also.

The Zoology Exhibition, value 40*l.*, for two years, at the preliminary Scientific and 1st B.Sc. examinations of London University has this year been obtained by Mr. J. C. Saunders, of Downing College, Cambridge.

Dr. H. Alleyne Nicholson has resigned his Lectureship on Natural History in the Extra-Academical Medical School of Edinburgh, and is now in Toronto.

An expedition for botanical purposes is in course of formation to the summit of Mount Bellendenker and the Endeavour River in Queensland. This mountain is situated on the east coast, near Cape Grafton, and nearly opposite the head of the gulf of Carpentaria, and the Endeavour River is a little farther north. It is the highest mountain on the Queensland coast. It is very probable that many new plants will be found in the course of this expedition, as the country is almost virtually unexplored. The last explorer was Kennedy, who was killed twenty years ago. It was in the Endeavour River that Captain Cook landed and cleaned his vessel after the discovery of the Torres Straits.

Now that the so-much-dreaded cholera is rapidly approaching our shores, it behoves everyone to be able to recognise the preliminary symptoms of the disease and to guard against them. We would therefore most strongly recommend all who can to read a most valuable and instructive paper by Mr. John Murray, the Inspector-General of Indian hospitals, on "Cholera: its Symptoms and Early Treatment," which was read at the recent meeting of the British Medical Association at Plymouth. It would be a very great boon to society, and probably the saving of many lives, if this paper could be published as a penny pamphlet.

THREE exhibitions, giving free education, and tenable in the department of General Literature and Science, or in that of Engineering and Technical Science, will be open to new students at the Hartley Institution, Southampton, at the commencement of the autumn term next month.

AN earthquake took place in Chiriqui in the State of Panama on the 26th June, at 7.50 P.M. It was rather severe, but no damage was done.

THE U.S. sloop of war *Jamestown* sailed from Valparaiso on the 3rd June, to determine the position of certain reefs and islands reported to have been discovered between the Equator and 24° N.

THE district round Wagga-Wagga, in Australia, was disturbed on June 8 by a somewhat violent earthquake shock; and, owing to the rarity of the occurrence of such phenomena, it has caused much interest. The shock consisted of a succession of sharp but continuous vibrations, lasting altogether for about twenty seconds, the motion appearing to be from the N.W. to the S.E. There was felt, at 16 minutes to 3 P.M. (local time), a slighter second shock, preceded like the first, by a dull rumbling sound.

IN a letter to the *American Journal of Science and Art*, Dr. B. A. Gould reports satisfactory progress with respect to the Cordova Observatory. Although the enterprise has met with an exceptional amount of obstacles, Dr. Gould, who writes on the 26th of April last, expected to begin the mounting of the instruments in the course of a few days. We shall probably recur to his interesting communication.

## *OBSERVATIONS OF LUMINOUS METEORS IN THE YEARS* 1870-71 *

THE object of the Committee was, as last year, to present a condensed report of the observations which they have received, and to indicate the progress of Meteoric Astronomy during the interval that has elapsed since the last report. A valuable list of communications on the appearances of luminous meteors has been forwarded to the Committee in the course of the year, as well as regular observations of star showers. The heights and velocities of thirteen shooting-stars obtained by the co-operation of Mr. Glaisher's staff of observers at the Royal Observatory, Greenwich, during the watch for meteors on the nights of the 5th to the 12th of August last, are sufficiently accordant with the velocity of the Perseids, as previously determined by similar means in the year 1863, to afford a satisfactory conclusion that the results of direct observation are in very close agreement with those derived from the Astronomical Theory of the August Meteor Stream. On the mornings of the 13th to the 15th of November last, a satisfactory series of observations of the November star shower (as far as its return could be identified), recorded at the Royal Observatory, Greenwich, and at several other British Association stations, concurs with very similar descriptions of its appearance in the United States of America, in showing the rapid decrease of intensity of this display, since the period of greatest brightness which it attained in the years 1866 and 1867.

Notices of the appearance of more than twenty fire-balls and small bolides have, during the past year, been received by the Committee; fourteen of the former were compared to the apparent size and brightness of the moon, and the latter include three detonating meteors of the largest class. Descriptions of some of the largest of these meteors are given at length in the report. No notice of the fall of an aërolite during the past year has been received, although the occurrence of large meteors during the Autumn and Spring months was unusually frequent. The locality of one of these, which appeared with unusual brightness in the South of England, on the evening of the 13th of February can be determined at least approximately, as also the elevation of its path.

A table of the height of sixteen shooting stars doubly observed in England during the meteoric shower of August 1870 (independently of the observations made at the Royal Observatory, Greenwich), appeared in the last volume of the British Association Reports. A comparison of the observations made at the Royal Observatory, Greenwich, on that occasion, with those recorded at the other stations, enables the paths of thirteen meteors, seen by Mr. Glaisher's staff of observers (ten of which are new to the former list), to be determined; and the heights and velocities of the meteors thus identified are entered in the Report. The results are as follows: The average height of 16 meteors contained in the last report was 74 miles at appearance and 48 miles at disappearance; of 13 meteors (given in the present list), 72 miles at appearance and 54 miles at disappearance; of 20 meteors (observed in August, 1863), at appearance 82 miles, at disappearance 58 miles. The present average heights are thus somewhat less than those observed in 1863, but they agree more closely with the general average height at first appearance, viz., 70 miles, and that at disappearance, viz., 54 miles. The average velocity of the Perseids (relatively to the earth) observed in the year 1863 was thirty-four miles per second, and that of three Perseids in the present list was thirty-seven miles per second; while the velocity on the astronomical theory, as calculated by Prof. Schiaparelli, was thirty-eight miles per second.

A considerable shower of shooting-stars was also noted on the night of April 20 last, of which preparations were made to record the progress, with satisfactory results.

The report, which was full and elaborate, contained a description of the new meteor-showers noted during the few last years by Prof. Schiaparelli, agreeing in many points with previous determinations by the Committee from the observations contributed to the British Association, and suggesting considerations of novel and important interest in relation to the probable explanation of certain facts regarding the radiant points of shooting-stars. These are in some cases (more or less exactly) simple, double, or multiple points; and in other cases present a wide central space or region of "diffuse radiation." On the other hand, distinct radiant points of ordinary shooting stars, observed on several closely adjacent nights, although apparently exhibiting no other connection with each other by meteors observed on the intervening dates, sometimes including many days, are yet so nearly identical in their positions as to make it almost certain that they belong to distinct families or systems of meteor-streams. Prof. Schiaparelli shows, in a preliminary discussion of these results, that if the particles of a small meteor-cloud, entering from extraplanetary space the region of the sun's attraction, is deflected from its primitive course by the attraction of one of the larger planets into an elliptic orbit round the sun, the velocities of its particles, in their elliptic orbits, will, in general, differ slightly among themselves; and the meteor-group will, in consequence, extend itself into a continuous stream of gradually increasing length along the orbit of the group. Although the continuity of the group will be preserved along its whole length during this extension, yet the stream will only form a continuous meteor-ring (when the foremost particle overtakes the hindmost one in its course) if, while gaining one complete revolution upon the latter, this and the foremost particle of the stream continue to describe the same orbit round the sun, or an orbit which undergoes the same perturbations by the planets. But since the two ends of the stream, during its extension, occupy very different positions in space, the orbits of the extreme particles are, in general, very differently affected by the attractions of the planets; and, when the particles in advance have gained one entire revolution upon those in the rear, the group will not, in general, form a closed ring; but an open, spiral curve, the ends of which, instead of exactly meeting, will generally overlap each other. When the first particle has gained a second revolution in advance upon the last, a second convolution of the coil will generally be added to the spiral curve; and no perfect meteor-annulus, for the same reason as before, will generally be formed by this circuit, or by any succeeding circuits of the meteor-stream, until its length and the number of its circuits are indefinitely increased. Since the thickness and

* Report of Committee, British Association, 1871.

density of the stream diminish as its length increases, its interlacing wreaths will give rise to a group of meteor-showers, more and more difficult to distinguish from each other, as their number becomes greater, until at last the condition of a meteor-belt so formed becomes that of innumerable meteor-particles revolving in orbits apparently independent of each other, and intersecting each other in all possible directions within the general boundaries of the elliptic ring. The appearance presented by a meteor-group of this description, during its first encounters with the earth, will be a periodic star-shower (like that of the November meteors), diverging, whenever it is visible, from a nearly exact and single radiant point. At the end of a certain number of cyclical returns, the star shower will be annually visible on a particular date, diverging from the same, or nearly from the same, radiant-point, but much less abundantly than at first; and a twin meteor-shower with a time of maximum, and a radiant point closely adjacent to the former ones will, at intervals, make its appearance with the original shower. This also, like the latter, after an equal lapse of time, will become annual; and both diminishing together will present the appearance of a double meteor-shower, appearing simultaneously, or very nearly together, with a double or twin radiant-point; while at intervals, a third meteor-shower, of the same general features as the previous two, will begin to be added to the group. Proceeding in this manner, as the antiquity of the meteor-ring increases, the star-shower will resolve itself into a more or less well-defined group of slender streams, producing alternate short lulls, and flights of meteors from a great multiplicity of radiant points, contained within a limited region of diffuse or multiple radiation. The ordinary appearance of the star-shower on the nights of the 9th and 11th of August, answering very closely to the description of a meteor-stream in an already far-advanced stage of its development, the much higher antiquity of the August than that of the November star-shower, already shown by its regular annual return, and by the ancient times in which it appears to have been recorded, must now also be regarded as satisfactorily confirmed by the frequently-recorded multiple, and more commonly observed diffuse character of its radiation. Among the star-showers of less ancient date, of which the November meteors appear to present a conspicuous example, Prof. Schiaparelli includes a meteor-shower observed by Zezioli on October 12, and two others on November 10, 1868; one star-shower on each of these dates radiating very exactly from points in the neighbourhood of the constellation Taurus, as well as the star-shower of October 18, and 20, 1864 and 1865, the radiant point of which was very exactly marked in those years in Orion.

Continued observations of the best-known star-showers being calculated to afford such important information on the present conditions, and on the probable antiquity of their connection with the solar system, the committee propose to re-examine the principal meteor-showers during the coming year, with suitable means for registering the meteors observed on each of the following dates, viz., August 9 to 11, October 18 to 21, November 13 to 15 (A.M.), December 11 to 13, 1871, and January 1 to 3, and April 19 to 21, 1872, and to determine, as exactly as possible, the moments of maximum frequency, the rates of appearance, and the principal points of radiation of the meteors visible on those days.

## THE LATE REV. W. V. HARCOURT'S RESEARCHES ON GLASS *

THE subject of the preparation and optical properties of glasses of a great variety of chemical positions, formed, for nearly forty years, a favourite study with the late Mr. Harcourt. As stated in a report published in the British Association Reports for 1844, some experiments on the subject were commenced in 1834, which he was encouraged to pursue further by a request published in the fourth volume of the Transactions of the Association. A report on a gas furnace, the construction of which formed a preliminary inquiry, was published in the reports, but the results of the actual experiments on glass have never yet been published.

My own connection with these experiments commenced at the meeting of the Association at Cambridge in 1862, when Mr. Harcourt placed in my hands some prisms formed of the glasses which he had prepared, to enable me to determine their character as to fluorescence. I was led incidentally to observe the fixed lines of the spectra formed by them; and as I used sunlight which he had not found it convenient to employ, I was enabled to see further into the red and violet than he had done, which was favourable to a more accurate determination of the dispersive powers. This inquiry being in furtherance of the original object of the experiments, seemed far more important than that as to fluorescence, and the increased definiteness caused Mr. Harcourt to resume his experiments with the liveliest interest, an interest which he kept up to the last. Indeed, it was only a few days before his death that his last experiment was made. To show the extent of the inquiry I may mention that at least 166 masses of glass were formed, and cut into prisms for measurement, each mass doubtless involving in many cases several preliminary experiments, besides discs and masses for other purposes.

It is well known how difficult it is, in working on a small scale, to make glass which is free from striæ and imperfections of the kind. Of the first group of prisms, 28 in number, 10 only showed a few of the principal dark lines of the solar spectrum; the rest had to be examined by the bright lines in artificial sources of light. These prisms seemed to have been cut at random by the optician from the mass of glass furnished to him. Theory and observation alike showed that striæ interfere comparatively little with an accurate determination of refractive indices when they lie in planes perpendicular to the edge of the prism. Accordingly, in the rest of the research the prisms were formed from the glass mass that came out of the crucible by cutting two planes passing through the same horizontal line a little behind the surface, and inclined $22\frac{1}{2}°$ right and left of the vertical, and polishing the enclosed wedge of 45°. In the central portion of the mass the striæ have a tendency to arrange themselves in nearly vertical lines by the operation of currents of convection, and by cutting in the manner described the most favourable direction of the striæ is secured for a good part of the prism. This attention to the direction of cutting, combined no doubt with increased experience in the preparation of glass, was attended with such good results that now it was quite the exception for a prism not to show the principal dark lines. Some of the latest prisms were almost equal to prisms of good optical glass.

On account of the difficulty of working with silicates, arising from difficult fusibility and the pasty character of the glasses, Mr. Harcourt's experiments were carried on with phosphates, combined in many cases with fluorides and sometimes with borates, tungstates, molybdates, and titanates. The glasses formed involved the elements potassium, sodium, lithium, barium, strontium, calcium, glucinium, aluminium, magnesium, manganese, zinc, cadmium, tin, lead, thallium, nickel, chromium, uranium, bismuth, antimony, tungsten, molybdenum, titanium, vanadium, phosphorus, fluorine, boron, and sulphur. A very interesting subject of inquiry presented itself collaterally with the original object, namely, to ascertain whether glasses could be formed which would achromatise each other so as to exhibit no secondary spectrum, or a single glass which would form with crown and flint a combination achromatic in that sense. This inquiry presented considerable difficulties. The dispersion of a medium is small compared with its refraction, and if the dispersion be regarded as a small quantity of the first order, the irrationality between the two media may be regarded as depending on small quantities of the second order. If striæ and imperfections of the kind present an obstacle to a very accurate determination of dispersive power, it will readily be understood that the errors of observation thus occasioned go far to swallow up the small quantities, in the observation of which the determination of irrationality depends. Accordingly little success attended the attempt to draw satisfactory conclusions as to irrationality from the direct observation of refractive indices; but by a particular mode of compensation, in which the experimental prism was achromatised by a prism built up of a combination of slender prisms of crown and flint, I was enabled to draw trustworthy conclusions as to the character, in this respect, of these prisms, which were good enough to show a few of the principal dark lines of the solar spectrum.

Theoretically any three different kinds of glass may be made to form a combination which shall be achromatic as to secondary as well as primary spectra; but for a long time little hope of a practical solution seemed to present itself. A prism containing molybdic acid was the first to give fair hopes of success. Mr. Harcourt warmly entered into the subject, which he prosecuted with unwearied zeal. The earlier molybdic glasses prepared were many of them rather deeply coloured, and most of them of a perishable nature. At last, after numerous experiments, molybdic glasses were obtained nearly free from colour, and permanent. Titanium had not yet been tried, and about this time a glass

* Paper read by Prof. Stokes in Section A, British Association, 1871.

containing titanium was prepared. Titanic acid proved to be equal or superior to molybdic in its power of extending the blue end of the spectrum more than corresponds to the dispersive power of the glass; while in every other respect—freedom from colour, permanence of the glass, greater abundance of the element—it had a decided advantage; and a great number of titanic glasses were prepared, cut into prisms, and measured. Some of these led to the suspicion that boracic acid had an opposite effect to titanic, to test which Mr. Harcourt formed some simple borates of lead, with very varying proportions of boracic acid. These fully bore out the expectation; the terborate, for instance, which in dispersive power nearly agrees with flint glass, agrees on the other hand in the relative extension of the blue and red ends of the spectrum with a combination of about one part (by volume) of flint glass with two of crown.

By combining a negative (or concave) lens of terborate of lead with positive lenses of crown and flint, or else a positive lens of titanic glass with negatives of crown and flint, or a positive of crown and a negative of low flint, achromatic triple combinations free from secondary dispersion might be formed, without encountering formidable curvatures, and by substituting at the same time a borate of lead for flint glass, and a titanic glass for crown, the curvatures might be a little further reduced.

There is no advantage in using three different kinds of glass rather than two, to form a fully achromatic combination, except that the latter course might require the two kinds of glass to be made to order, whereas with three we may employ for two of them the crown and flint of commerce. It is probable that enough titanium might be introduced into a glass to allow the glass to be properly achromatised by Chance's "light-flint."

In a triple combination of lenses the middle lens may be made to fit both the others, and be cemented. Terborate of lead, which is somewhat liable to tarnish, might thus be protected by being placed in the middle. Even if two kinds only of glass be used it is desirable to divide the concave lens into two for the sake of diminishing the curvatures. On calculating the curvatures so as to destroy spherical as well as achromatic aberration, and at the same time, to make the adjacent surfaces fit, very suitable forms were obtained with the data furnished by Mr. Harcourt's glasses.

After encountering great difficulties from striæ, Mr. Harcourt at last succeeded in preparing discs of terborate of lead and of a titanic glass, of about 3in. diameter, almost homogeneous, and with which it is intended to attempt the construction of an actual object glass, which shall give images free from secondary colour.

This notice extends to a greater length than I had intended, but still it gives only a meagre account of a research extending over so many years. It is my intention to draw up a full account for presentation to the scientific world in another way. I need but say that the small grant made to Mr. Harcourt for these researches has been expended over and over again, but it was his wish, in recognition of that grant, that the first notice of the results he obtained should be made to the British Association.

## *THE BRITISH ASSOCIATION MEETING AT EDINBURGH*

### SECTION A.

*On the Recent and Coming Solar Eclipses*, by J. N. Lockyer, F.R.S. The substance of this has already appeared in these pages.

Prof. Tait remarked, after the reading of Mr. Lockyer's paper, that the photography which had been exhibited left no doubt in his mind that the greater part of the solar corona is produced in the earth's atmosphere. The rays are pretty obviously to be attributed to ice-crystals, and the various irregular protuberances sometimes seen may be due to germs and light particles blown off from meteorites before they become incandescent, which, according to a beautiful investigation of Stokes, descend with extreme slowness towards the earth. This simple consideration is sufficient to show the utter absurdity of the sneers with which Sir W. Thomson's suggestion has been received, and to justify it as a scientific possibility—all it pretended to be.

*On the Coming Solar Eclipse*, by M. Janssen. In the discussion on these communications, Sir W. Thomson said he joined warmly in what Mr. Lockyer and M. Janssen had said. M. Janssen had asked that Britain should join France and Germany in their friendly struggle. There was a challenge from France and Germany, and it would be a disgrace to England if it did not accept that challenge, and do its best to beat its rivals in the struggle.

*Report of the Committee for discussing Observations of Lunar Objects suspected of Change.*

Mr. W. R. Birt, to whom the execution of the work was confided, read the report on behalf of the committee, consisting of Edward Crossley, Esq., and the Rev. T. W. Webb. The report stated that much attention having of late years been given to lunar objects, the purpose for which the committee had been appointed would be best carried out by confining the discussion to the observations of a small but well-known portion of the moon's surface; and as the spot plate had presented during the last two years a variety of interesting and important features which had been well observed, it had been chosen as the most likely to yield results contributing to the advancement of selenography. Time having permitted the discussion of the observations of the bright spots only, it was requested that a further grant of 20*l.* should be placed at the disposal of the committee for the discussion of the observations of the streaks and markings on the floor which were intimately connected with the spots. Mr. Birt, in alluding to the work which he had executed on behalf of the committee, said that as his report was voluminous, he would content himself with a brief description of the results at which he had arrived. The area of Plato in which the spots exist measured about 2,700 square miles; as many as thirty-seven spots had been observed, but he wished it to be particularly understood that the whole had never been seen together; the greatest number observed on any one occasion was twenty-seven, the mean or average number being not more than eight. With the aid of diagrams drawn on the black board, he showed that the mean number seen at intervals of twelve hours of the luni-solar day varied during the progress of the day, so much so as to indicate that the number of spots visible at any given interval does not depend upon the angle at which the sun's light falls upon the floor of Plato Some spots, he said, had been seen more frequently at about sixty hours after sunrise upon the floor of Plato than at any other portion of the luni-solar day; the positions of these spots on the floor were pointed out, and it was remarked that they were situated in the western part of the crater, and they agreed in having been more frequently observed in August 1869, than at any other period of the observations. Other spots were observed more frequently at a later period of the observations than in August 1869, and they had been seen more frequently at a later period of the day, or after the sun had passed the meridian. Daylight at the moon is equal to fourteen terrestrial days and nights. These facts Mr. Birt argued were incompatible with the assumption that variations of aspect were entirely dependent upon variations of illumination, and rather pointed to the existence of activity on the moon's surface, the exact nature of which required further observations to elucidate.

*Report on Thermal Conductivity of Metals*, by Prof. Tait.

Prof. Tait, on the part of the committee appointed to report on this subject, drew attention to the relation that exists between electric and thermal conductivity of metals, and the effect on conductivity of a very small amount of impurity. He also sketched the apparatus made use of in the determination, and said that, as a new gasometer had been introduced, he had recommenced the whole of his investigations under better auspices and with hopes of very great accuracy.

*On a New Steam Gauge*, by Prof. Zenger.

*Experiments on Vortex Rings*, by H. Deacon.

*Observations of Parallax of a Planetary Nebula*, by D. Gill.

*On a New Form of Constant Galvanic Battery*, by Latimer Clark.

*On a Method of Testing Submerged Electric Cables*, by C. F. Varley.

*Description of Experiments made in the Physical Laboratory of the University of Glasgow to determine the Surface Conductivity for Heat of a Copper Ball*, by Donald M'Farlane. The experiments described in this paper were made under the direction of Sir W. Thomson during the summers of 1865 and 1871. A hot copper ball, having a thermo-electric junction at its centre, was suspended in the interior of a closed space kept at a constant temperature of about 16° C. The other junction was kept at the temperature of the envelope; the circuit was completed through a mirror galvanometer, and the deflections noted at intervals of one minute, as the ball gradually cooled.

The method of reducing the observations is explained at length in the paper. The differences of the Napierian logarithms of the differences of temperature of the junctions, indicated by the deflections, divided by the intervals of time, give the rate of cooling, and this, multiplied by a factor depending on the capacity for heat of the ball, and on the extent of its surface, gives the quantity of heat emitted in gramme water units, in the unit of time per square centimetre, per 1° difference of temperatures. Formulæ are given which express the results of the experiments very closely, and a table calculated by them exhibits the rates of emission for every 5° of difference throughout the range.

The first and second series have a range of from 5° to 25° only, which was too small to give decided results, but the third and fourth series, made with a polished copper surface and a blackened surface respectively, gave variations on the emissive power from ·000178 at 5° diff. of temperature to ·000226 at 60° diff. for the blackened surface, and exhibit throughout a nearly constant ratio of about ·694.

*On Wet and Dry Bulb Formulæ*, by Prof. Everett. He said, August, Apjohn, and Regnault have investigated formulæ for determining the dew point, by calculation, from the temperatures of the dry and wet bulb thermometers; but Regnault's experiments on the specific heat of air were not performed till a later date, and all their authors have adopted in their investigations the value obtained by Delaroche and Berard, which is ·267, whereas the correct value is ·237. But when this correct value is introduced into Regnault's formula, the discrepancies which he found to exist between calculation and observation are increased, and amount, on an average, to about 25 per cent. of the difference between wet bulb temperatures and dew point. August and Apjohn erred in assuming that the air which gives heat to the wet bulb falls to the temperature of the wet bulb, and becomes saturated. These two false assumptions would jointly produce no error in the result if the depressions of temperature in the different portions of air affected were exactly proportional to their increments of vapour-tension, and if some of the air were saturated at the temperature of the wet bulb. But it is probable that, when there is little or no wind, the mass of air which falls sensibly in temperature is larger than that which receives a sensible accession of vapour, and that, in high wind, the supposition that some of the air has fallen to the temperature of the wet bulb, is more nearly fulfilled than the supposition that it has taken up enough vapour to saturate it. The effect of radiation, which is ignored in the formulæ, leads in the same direction as these two inequalities, and all three are roughly compensated by attributing to air a greater specific heat than it actually has. The discrepancies above referred to are thus explained.

*On a New Key for the Morse Printing Telegraph*, by Prof. Zenger.

*On Clean and Unclean Surfaces in Voltaic Action*, by T. Bloxam.

*On the Corrosion of Copper Plates by Nitrate of Silver*, by J. H. Gladstone, F.R.S., and A. Tribe, F.C.S. In some recent experiments in Chemical Dynamics the authors had occasion to study the action of nitrate of silver on copper plates in various positions. They observed that when the plate was vertical there was rather more corrosion at the bottom than at the top. This is easily accounted for by the upward current which flows along the surface of the deposited crystals, and which necessitates a movement of the nitrate of silver solution towards the copper plate, especially impinging on the lower part. It was also found that when the copper plate was varnished on one side, it produced rather more than half the previous decomposition, and was more corroded at the edges of the varnish. By making patterns with the varnish this edge action became very evident. This was explained by the fact that the long crystals of silver growing out from the copper at the border can spread their branches into the open space at the side, and so draw their supply from a larger mass of solution than the crystals in the middle can do; and increased crystallisation of silver means increased solution of copper. This was proved by making the varnish a perpendicular wall instead of a thin layer, when the greater corrosion was not obtained. In a plate completely surrounded with liquid the greatest growth of crystals is also evidently from the angles. It was likewise observed that if a vertical plate be immersed, the lower part in nitrate of copper, and the upper part in nitrate of silver, there is greater corrosion about the point of junction. This was attributable to the greater conduction of the stronger liquid.

*The Influence of the Moon on Rainfall*, by W. Pengelly, F.R.S.

*On Units of Force and Energy*, by Prof. Everett, D.C.L.

All authorities are agreed that the units of length, time, mass, and force ought to be so settled as to satisfy the condition that unit force acting for unit time on unit mass generates unit velocity. Now, of the four elements, length, time, mass, and force, the first three can easily be referred to concrete standards available for reference at any part of the earth, but this reference is more difficult in the case of the fourth. The motion of the earth gives the mean solar second, a standard foot can be carried to any part of the earth, and if immersed in a mixture of ice and water, will have everywhere the same length; and a standard pound has the same mass to whatever place it is carried. But no material standard of force is easily provided, so that it is philosophical to make this the dependent unit, and define it in terms of the others; and this plan is that which has recently been followed.

Convenience of expression, however, requires several units of each kind. It is not convenient to express the distance from Liverpool to New York in inches, nor the diameter of a rifle bullet in decimals of a mile. Names have accordingly been provided for several units of time, length, and mass; but a similar provision has not yet been made in the case of units of force. With the exception of two letters by the author of the present paper that appeared in NATURE March 2 and May 4 of the present year, and another letter by Mr. Thomas Muir, no names for units of force dependent on specified units of time, length, and mass, seem ever to have been publicly proposed.

The unit of work stands in a simple relation to the units of force and length. It is the work done by unit force working through unit length. And that amount of energy which, in undergoing complete transformation, performs unit work, is the unit of energy. The same unit which measures work therefore measures energy. The only approach to a name that has been suggested on this subject is the "British absolute unit of energy," and the defect of nomenclature becomes often intolerable.

The author therefore repeated the proposals which had already appeared in NATURE, so that we need only briefly recapitulate a portion of the names proposed. The unit of force, corresponding to a second, a metre, and a gramme, as units of time, length, and mass, was called a *dyne*; the kilodyne was a thousand dynes, the megadyne a million dynes. The unit of energy or work was called the *pone*, and depended on the dyne and metre, the kilopone was a thousand pones, &c. In connection with the British system, the "British absolute unit force" was called a *kinit*, dependent on the pound, foot, and second, and the name *erg* was given to the corresponding unit of energy, the thousand and million ergs being written kilerg and pollerg respectively.

The dyne is about the terrestrial gravitating force of $1\frac{1}{2}$ grains, the kilodyne of $\frac{1}{4}$ lb., the megadyne of 2 cwt., and the kinit of $\frac{1}{2}$ oz. The kilopone is about $\frac{1}{10}$ of a kilogrammetre, the megapone about 723 foot-pounds, the kilerg is 31 foot-pounds, and the pollerg about the work done by a horse in a minute. On this subject a joint committee was appointed with Section G to frame a nomenclature of absolute units of force and energy.

*On the general Circulation and Distribution of the Atmosphere*, by Prof. J. D. Everett, D.C.L.

The object of this paper was to call the attention of meteorologists to a theory which is jointly due to Prof. J. Thomson of Belfast and Mr. Ferrel of Boston, U.S.A., and which gives the only satisfactory account of the grand currents of the atmosphere, and of the distribution of barometric pressure over the earth's surface, the irregularities arising from the distribution of land and water being neglected. Independent proofs were also given of some of Mr. Ferrel's results.

A body moving along the earth's surface with relative velocity $v$ (units a foot and second) tends to describe a curve concave to the right of the body in the northern and to its left in the southern hemisphere, the radius of curvature being $\frac{6850\,v}{\sin\lambda}$ feet. The deflection from a parallel of latitude into a great circle is usually negligible in comparison, being represented by the curvature of a circle of radius $R\cot\lambda$, $R$ being the earth's radius.

To keep therefore the moving body in a great circle or in a parallel of latitude requires a constraining accelerating force equal to $\frac{v\sin\lambda}{6850}$, and this formula applies alike to all horizontal directions of motion.

The air over the extra-tropical parts of the earth has a relative motion towards the east, and therefore passes towards the tropics with a force which can be computed from the above formula. If $v$ be the eastward velocity at any parallel, the increase of pressure per degree of latitude is ·0019 $v$ sin $\lambda$ inches of mercury, and this accounts for the observed increase of pressure from the poles to the tropics, which is roughly ·01 inch per degree.

If any stratum of air have less than the average eastward or westward velocity which prevails through the strata above it, it will not be able to resist the differential pressure from or towards the equator which their motion produces. For this reason the lowest stratum of air having its velocity relative to the earth kept down by friction, generally moves from the tropical belts of high barometer to the regions of low barometer at the poles and equator. This is the origin of our S.W. winds and of the prevalent N.W. winds of the Southern Ocean.

The tendency of a moving mass of air to swerve to its own right in the northern hemisphere explains Buys Ballot's law that the wind instead of blowing at right angles to the isobaric lines usually makes an angle of 20° or 30° with them, keeping the region of lower barometer on its left. The rotation of cyclones is an example of this law, and the pressure which the spirally-flowing streams exert to their own right in virtue of the earth's rotation is the main cause of the excessive central depression. The author referred to Prof. J. Thomson's paper (B.A. reports, 1857), to Mr. Ferrel's papers, and to NATURE, July 20, 1871.

*Remarks on Aerial Currents*, by Prof. Colding.

*On a Nutoscope for showing graphically the Curve of Precession and Nutation*, by Prof. Ch. V. Zenger.

In the case of a rapidly revolving solid, two things may take place according as the mass of the solid body is or is not uniformly distributed round the axis. In the first case, the axis of rotation steadily holds its position during the rotation, as in the case of the gyroscope. If a shock acts on the one side, the axis will describe a cone and its apex a circle, but if the mass on the disc be unequally distributed (which is practically done by fastening a small circular plate by an excentric hole to the axis) the motion becomes more complicated, and the apex of the axis has a precessional and nutational motion. This motion in the apparatus described is traced on a piece of blackened paper by the apex. The greater the disturbing weight is taken, the greater is the nutational motion, so that when it is very large the apex describes a spiral. The apex of the instrument is kept in slight contact with the blackened paper by means of a micrometer screw.

*On a Cause of Transparency*, by G. Johnstone Stoney. It is known that a gas becomes opaque if rays are passed through it of the same wave-length as that of the light which it gives forth itself when incandescent, and in the present communication the author proposed an explanation of this fact, and gave an account of some experiments he had made with regard to the motions of the molecules in chloro-chromic anhydride, the spectrum of which contained about 120 lines due to one motion of the molecules.

Prof. Stokes and Sir W. Thomson made some remarks on the paper, in the course of which it was mentioned that in order that a sound might be propagated to a great distance, it was not necessary that the disturbance need be strictly periodic; Sir W. Thomson also remarked that he believed the vibrations of a molecule of a gas would be found to more resemble the vibrations of an elastic plate than those of a string.

*Remarks on a new Dip Circle*, by Dr. Joule.

## SECTION C.

SOME relics of the Carboniferous and other Old Land-surfaces were described by Mr. Henry Woodward. Whilst admitting that during particular eras circumstances may have favoured the development of special groups of organisms, which, in consequence, flourished in greater abundance than the rest, the author deprecated the idea of the prevalence of peculiar conditions at any time since the advent of organic life on the globe. He referred to the fact of sedimentary deposits being formed at the bottom of the sea as positive evidence of the waste of neighbouring land surfaces, and he remarked that if conditions in the sea were favourable to the development of abundance of animal life, those on the land were in all probability equally so. He referred first to the abundant evidence of land-surfaces in Quaternary and Tertiary times. Truly marine deposits (such as the chalk) testify to the presence of land by the fossil remains of Pterodactyles, Cheloniæ, and other shore-dwelling reptiles, whilst the Wealden beds, the Purbeck limestone, and Oolitic plant-shales afford abundant proofs of Mesozoic lands. Even the marine Solenhofen limestone yields swarms of insects, flying lizards, and a true bird, beside plant remains. In the Triassic periods the earliest traces of Mammals appear, while ripple-marked slabs of sandstones show bird-like tracks and Labyrinthodont footmarks, telling of the denizens of the old sea-shores and lakes.

The author then described the Coal-period with its stores of land-plants and Reptilia, both aquatic and terrestrial, its insects and Mollusca. He controverted the arguments of Dr. T. Sterry Hunt as to the exceptional condition of the atmosphere of the Coal-period, and showed that the presence of animal life disproved the existence of an atmosphere charged with carbonic acid gas, and that plants would not be benefited thereby as Dr. Hunt supposed.

On Monday, August 7, a report *On Sections of Fossil Corals* was made by Mr. James Thomson, F.G.S. The structural characters and development of the Carboniferous corals (about 170 in number) were briefly pointed out and illustrated by a number of beautiful photographic plates. He explained the method by which the sections were prepared, and described a new process whereby he transferred the photographs of the structure to copper plates, that faithfully represented the most delicate parts.

Sir Richard Griffith, Bart., F.R.S., gave an interesting account of the Boulder Drift and the Esker Hills of Ireland. Pointing to his large geological map of the country, he gave a brief description of its physical features and general geological structure. He then described the boulder drift as consisting of sandy clay containing numerous stones and boulders, and having a thickness in the eastern part of about 100 feet; and he regarded it as formed by a great torrent moving suddenly and depositing rapidly. He next adverted to those remarkable ranges of hill, which varied in height above the surface of the boulder drift from twenty to sixty feet, the ascent being usually about twenty degrees on the west side, but less steep on the east. These Esker Hills were very numerous in the central portion of the country. Their general direction was from west to east; and one great esker, which extended from the county of Galway to Westmeath was used as the post road from Dublin to Galway for a length of thirty miles. These were formed after the Boulder Drift, by a shallow sea acting upon it. Sir Richard next directed attention to the occurrence of large erratic blocks, totally unconnected with the gravel, which were found resting on the surface throughout the entire district, from Galway Bay in an eastern and south-eastern direction, passing over the summits of the Sliebhbloom Mountains, near Roscrea, and extending from thence through the King's and Queen's counties. These blocks were all angular, and being composed of a peculiar porphyritic granite situated to the north of Galway Bay, they had evidently been transported by a current from the north-west.

In the discussion which followed the reading of this paper, Mr. Milne Home, alluding to the Esker drift, said that similar instances of ridges accumulated by the sea were to be found in Stirlingshire and Berwickshire, and one was now being formed in the Firth of Forth, which went by the name of the Whale's Back, and which was about two miles and three quarters in length; he also thought that the Chesil Bank presented a very similar structure to that of the Eskers, which he regarded as submarine banks. Mr. Geikie regarded the origin of the Eskers as still a puzzle to him. Mr. Symes, of the Geological Survey of Ireland, described the Eskers in County Mayo, which he had minutely examined, and the carved-out ridges of boulder clay in the neighbourhood of Clew Bay and West Port. He said that the Eskers were evidently of a newer creation, and of a different origin from the boulder clay ridges, or "drumlins" as they are called in Ireland. Mr. Kinahan pointed out on the geological map the general lie of the Eskers in the centre of Ireland, and suggested that they must be due to the meeting of two tidal waves in a glacial sea, which came respectively round the north and south coast. Meeting midway, as it were, in the channel, they were forced along what is now the low-lying central part of the country, and on again meeting a northern current in the valley of the Shannon, the Eskers were formed in a curve in a northerly direction, which would thus account for the general way in which these Eskers lie. Mr. Kinahan also said that in the low valley between Ballina and the mouth of the Shannon, there was a newer drift, the coast lines of which can be traced in numerous places in the counties of Mayo and Galway, and then southward to Cork.

A very important paper *On the Systematic Position of Sivatherium Giganteum* was read by Dr. Murie, but it was, perhaps, better suited to the Biological Section, as it met with but little discussion among the geologists. This animal appears to have been a ruminant, about the size of an elephant, in some respects deer-like, in others more resembling the antelopes; still stranger, it seems to have had some of the characteristic features of pachyderms, the tapir for example. Dr. Murie showed that it was one of those radical forms which by some may be regarded as one of the progenitors of diverse herbivorous groups. The *Sivatherium*, according to him, was unlike all other living ruminants but one, the prongbuck, from the fact of its having had hollow horns, evidently subject to shedding. It differs thus from deer, whose solid horns annually drop off, and from the antelope tribe, sheep, and oxen, whose hollow horns are persistent. Save one living form, the saiga, no recent ruminant possesses, as did the *Sivatherium*, a muzzle resembling in several ways the proboscis of the tapirs and elephants. Dr. Murie placed it in the family Antelocapridæ, from which radiated the *Bramatherium*, the prong-buck, the saiga, tapir, and antelopes.

*The Relation of the Quaternary Mammalia to the Glacial Period* was treated of by Mr. Boyd Dawkins, F.R.S. He divided the animals into five distinct groups, the first of which comprises those now living in the temperate regions of Europe and America, including the grizzly bear, the lynx, the bison, and the wild boar; these animals bind the Quaternary to the existing fauna. The second group comprises those animals which are now confined to cold regions, as the glutton, the reindeer, the musk sheep, and the tailless hare: they constitute the Arctic division of Quaternary Mammalia, and imply a cold climate. The third group consists of those animals which are now only found in hot regions—the canichow, and hippopotamus; and they indicated a hot climate. The only mode of getting over this discrepancy is to suppose that in those days the winter cold was very severe and the summer heat likewise very severe; so that in the summer time the animals now found in warmer regions migrated northwards, and in the winter time those now found in the Arctic regions went southwards. The fourth group consists of such extinct forms as the cave bear, the stag, the mammoth, and the woolly rhinoceros. The fifth group includes the sabre-tooth tiger, the Irish elk, *Rhinoceros megarhinus*, and *hemitæchus*, and they, with some others, show that there is no great break between the Quaternary and the Pliocene, such as would warrant any sharply-defined division of great value. The interest centred more particularly in the Arctic group, and so far as the evidence went, it seemed to be extremely probable that they were in occupation of the areas in Great Britain in which they were found during the time the other areas, in which they were not found, were covered by glaciers; and this period may be put down to that of the latest sojourn of the glaciers in the highest grounds of our islands, and even so far south as the district of the Avon.

Prof. W. C. Williamson, F.R.S., read a paper *On the Structure of the Diploxylon*, a plant of the Carboniferous Rocks.

The Silurian Rocks of Selkirk and Roxburgh were treated ot by Messrs. Charles Lapworth and James Wilson. The authors pointed out that these rocks were capable of division into well-defined and well-marked groups. They had discovered a large number of fossils which had been obtained from all parts of the district; from the lowest to the highest beds examined, and many of which were new to Scotland. For the purpose of comparison the strata described were split up provisionally into five formations, namely:—1, The Hawick rocks; 2, The Selkirk rocks; 3, The Moffat series; 4, The Gala group; 5, The Riccarton beds. The Gala group they believed to be of Upper Bala or Caradoc age, and the Riccarton beds were classed with the Wenlock formation.

Mr. Lapworth subsequently enumerated the graptolites of the Gala group, and described two new species.

Two very important papers *On Local Geology* were communicated by Mr. D. J. Brown. Unfortunately from their being postponed until late in the day no time was allowed for their proper hearing or discussion. Mr. Brown endeavoured to show that the Silurian Rocks of the South of Scotland as developed in Dumfriesshire and Peeblesshire do not all belong to one geological epoch, as has been hitherto supposed, but that they belong to different periods—a lower one represented by the Moffat Rocks, well-known by their beds of Anthracite shales and Graptolites, and an upper series of later age, which lies unconformably on the Moffat rocks. These beds have been long known, and more recently they have been pointed out at Galashiels by Messrs. Lapworth and Wilson. Mr. Brown also showed that in the Pentland Hills both the Wenlock and Ludlow divisions of the Silurian Rocks are represented, and that the lower Old Red sandstones formed no part of these beds: also that these Pentland beds are not the equivalent of the Lesmahago, but that these latter are a higher portion of the Ludlow than any found in the Pentlands.

Mr. John Henderson described two sections across the Pentland Hills, and showed that the Felstones cut through, indurate and enclose angular fragments of rocks belonging to the upper portion of the Lower Carboniferous formation, and that the so-called Old Red conglomerates contain limestone pebbles enclosing Carboniferous fossils.

## SECTION D.

### SUB-SECTION.—BOTANY

Prof. Dyer, B.A., B.Sc., read a paper *On the so-called "Mimicry" in Plants.* He said:—In all large natural families of plants there is a more or less distinctly observable general habit or *facies*, easily recognisable by the practised botanist, but not always as easily to be expressed in words. The existence of such a general habit in leguminous and composite plants is familiar to every one. What have been hitherto spoken of as *mimetic* plants are simply cases where a plant belonging to one family puts on the habit characteristic of another. This is entirely different from mimicry among animals, inasmuch as the resembling plants are hardly ever found with those they resemble, but more usually in widely different regions. *Mutisia speciosa* from Western South America, a composite, has a scandent leguminous habit closely agreeing with that of *Lathyrus maritimus* of the European shores. In the same way three different genera of ferns have species (found in distant parts of the world) indistinguishable in a barren state. The term Mimicry seems objectionable in these cases, and I propose Pseudomorphism as a substitute. As to the cause of the phenomenon, I can only suggest that the influence of similar external circumstances moulds plants into the similar form most advantageous to them. An illustration is afforded by the closely resembling bud scales which are found in widely separated natural orders of deciduous trees as modifications of stipules. I do not, however, think that the moulding influence need always be the same. I believe that different external conditions may produce the same result; in this respect they may be called analogous. Several identical plants are found on the seashore and also on mountains. The reason is, I believe, that they are equally able to tolerate the effect of soda salts and also of mountain climate; the tolerance of either unfavourable condition gives them the advantage over less elastically constituted plants, and the two are therefore analogous in their effects.

Professor Dyer's paper gave rise to an interesting discussion, in which Profs Balfour, Dickson, Lawson, Perceval Wright, and Mr. Carruthers joined, in the course of which attention was called to the fact that while there might be "pseudomorphs" in the vegetable kingdom, yet there were also true cases of what is now technically called "mimicry."

Dr. R. Brown read two papers *On the Geographical Distribution of the Flora of North-West America*, and *On the Flora of Greenland.* In the discussion on the latter paper Prof. Dickie stated that the Diatoms which he had catalogued for Dr. Brown had been for the most part obtained from the stomachs of mollusca. Prof. Lawson doubted if we knew more than just the coast flora of Greenland, all the mosses met with were just the commonest British species. Prof. Dyer alluded to the lack of positive knowledge that existed as to whether icebergs were or were not carriers of vegetable life.

Mr. A. G. More exhibited some living plants of *Spiranthes gemmipara* which had been collected by him in the last week in July, at Castletown, Berehaven.

Prof. Balfour submitted some observations on the cultivation of ipecacuanha plants in the Edinburgh Botanic Garden for transmission to India. As a curative for dysentery, the value of this plant was very great, and, in consequence of the partial failure, from various causes—such as the rashness and carelessness of collectors—of its cultivation in its native country (South America), its cultivation here for sending out India became a matter of much importance. A short time ago Mr. James M'Nab, of the Botanical Gardens, discovered that by making cuttings of the rhizome of the plant under the surface of the ground, numerous new shoots could be got, and the plant so propagated much more easily and plentifully. They had thus been able to send out a number of healthy plants to India, which it was hoped would be there equally successfully cultivated. Mr. M'Nab was also endeavouring, with fair prospect of success, to get the perfect seed of the plant, and if that could be done the difficulties of propagation would of course disappear. They had now two varieties of the plant in the Botanical Gardens, one of which had been cultivated there for forty years, and the other had just been got from South America, through the kindness of Dr. Gunning and Dr. Christison.

Dr. Cleghorn, F.L.S., late Conservator of Forests, Madras, expressed his delight at seeing the satisfactory result of the ipecacuanha propagation. Every army surgeon, he said, knew the great value of this remedy in the treatment of dysentery, and he hoped that the result of this experiment would be as successful as had been the introduction of cinchona. He thought much credit due to Profs. Balfour and Christison and Mr. M'Nab in this matter.

Mr. John Sadler read a paper *On the Genus Grimmia* (*including Schistidium*) *as represented in the Neighbourhood of Edinburgh.* After alluding to the varied character of the geological formation around the city, he stated that perhaps in no district of equal size in Britain would so large a number of species of this genus be found.

In Hooker and Taylor's *Muscologia Britannica,* published in 1818, a work which has never yet been surpassed for simplicity and correctness of description, there are seven species of *Grimmia* given as native of Britain. Two of these species, however, are now removed to other genera. In Wilson's *Bryologia Britannica,* or the third edition of the former work, published in 1855, there are fifteen species and many varieties described. Wilson, however, following Schimper, places three of these under the genus *Schistidium,* a genus which we think might with advantage to the student be easily dispensed with, seeing that its principal distinction from Grimmia rests on such an arbitrary character as the adhesion or partial adhesion of the columella to the lid. Since 1855 several species have been added to the genus in Britain, and noticed in the proceedings of different learned societies.

Greville, in his *Flora Edinensis,* published in 1824, describes six species of Grimmia as occurring within a radius of 10 miles of the city, viz.—*G. apocarpa, maritima, trichophylla, pulvinata, leucophœa,* and *Doniana.*

In Balfour and Sadler's *Flora of Edinburgh,* published in 1863, ten species, including Schistidiums, are recorded ; and in the second edition of the same work issued this day no fewer than fifteen species are given. If we take the rocks of Arthur's Seat we shall there find a wonderful development of Grimmias. They vary much, however, in their distribution over the hill. The most widely distributed are *G. pulvinata* and *subsquarrosa* ; next we have *G. conferta, pruinosa, leucophæa,* and *trichophylla,* less widely distributed ; while *G. anodon, orbicularis* and its variety *oblonga, commutata,* and *Doniana* are very limited. Another interesting fact is that all these species, with the exception of *G. trichophylla,* which occurs on different kinds of rock, seem to have a preference for the amygdaloidal trap, and very rarely occur on the basalt. If a stray specimen, however, does get on to the last-named rock, it has the most stunted and starved appearance. At one part of the hill, where the upper drive cuts the rocks to the back of the basaltic columns of "Samson's ribs," there is an area of very limited extent where the whole of the species which occur on the hill can be collected. In fact, any one at all acquainted with the plants and the rocks on which they grow would have no difficulty in securing the whole in the course of a very few minutes. In April 1870, in company with Mr. Bell, of the University Herbarium, who has paid much attention to the mosses of Edinburghshire, and to whom we are indebted for the discovery of *Grimmia anodon* in Britain, I collected in the space of one hour specimens of *G. apocarpa, conferta, anodon, pruinosa, subsquarrosa, pulvinata, orbicularis, orbicularis* (var. *oblonga*), *trichophylla* (2 var.), *leucophœa,* and *commutata.*

If we take the species to be met with within a radius of about 7 miles or 8 miles, and classify them in a sort of natural order according to affinity, we have first of all two sections—

1. Capsule immersed in the perichætal leaves.
2. Capsule exserted.

Under the first section comes—*G. apocarpa, maritima, pruinosa,* and *anodon.* The last named resembles the members of the second section in the structure of its leaves.

Under the second section comes—*G. pulvinata, orbicularis, orbicularis* (var. *oblonga*), *subsquarrosa, trichophylla, patens, Doniana, ovata, leucophœa, commutata,* and *torta.*

The author then went over each of the species, giving their distinguishing characters, and pointing out their geographical distribution over the world. The paper was illustrated by a complete set of dried specimens of the species referred to, as well as by drawings of the rarer species.

Mr. Sadler noticed the occurrence of *Cystopteris montana* in great abundance on the Breadalbane mountains this season, and presented dried specimens of this rare fern to the meeting.

Dr. Murie, in communicating a paper *On the Development of Fungi within the Thorax of living Birds,* referred to the circumstance of lowly-organised vegetable structures being not unfrequently found growing in animals and man, both externally and internally. For the most part these affected the skin, giving rise to several cutaneous diseases. They also flourished in the alimentary canal ; and among others, one peculiar form (*Sarcina*) had been described by the late Professor Goodsir from the human stomach. In nearly though not in all instances where vegetable organisms flourished within the living body, it was in organs where a certain amount of air had free access. It was more difficult, though, to account for the cases where vegetable parasites arose in, so to speak, closed cavities. The instances of this latter fact which he (Dr. Murie) brought forward as coming under his own observation were three in number—viz., a fungus-like growth in the abdomino-pleural membrance of a kittiwake gull, of a great white-crested cockatoo, and of a rough-legged buzzard. After a general description of the specimens in question, the author referred to them as in some ways bearing upon those doctrines which supposed living organisms to originate out of the tissues themselves. Weighty reasons undoubtedly might be given to the contrary, but as every fact, either furnishing doubtful evidence of, or opposed to the spontaneous generation theory, might be useful at the present juncture, he (Dr. Murie) thought a record of such worthy of being brought before the Association.

In the discussion which followed, Mr. Cooke and Prof. Perceval Wright questioned whether the vegetable structures spoken of by Dr. Murie might not be Algæ instead of fungoid bodies.

Dr. Bastian said that the question calling for most consideration was how these vegetable forms came to be found in a place cut off entirely from communication with the atmosphere. After mentioning the hypothesis that the spores of the fungi or algæ might have penetrated the tissues of the lungs or other vessels, and so reached the thoracic cavity, he explained his own views on the subject, illustrated by his experience in finding in the brain, and other portions of the human body isolated from the atmosphere, immense numbers of living organisms shortly after death, which, so far as could be ascertained, had no existence when the patient was alive, and insisted that either these organisms must have been previously present in the blood in a latent state—their germs being so minute as to be undistinguishable—or they must have come into existence by spontaneous generation.

Prof. Dickson read a paper entitled "Suggestions on Fruit Classification." [We give this valuable paper *in extenso* in another column.]

Prof. Dyer read a paper *On the Minute Anatomy of the Stem of Pandanus utilis.*

The Rev. Thomas Brown exhibited some specimens of fossil wood from the Lower Carboniferous rocks of Langton, Berwickshire. These fossil woods were described as occurring in the same Lower Carboniferous rocks in which Mr. Witham had found the stems figured more than thirty years ago, only that the rocks at Langton lie considerably in the lower series. One stem was particularlarly referred to, and drawings exhibited of the transverse and longitudinal sections. The transverse section was shown to present all the appearance of exogenous structure with pith rays and circular lines of annual growth. The longitudinal section showed that the seeming rays were vascular bundles, and that the stem from the pith to the circumference was a mass of scalariform tissue. Thus the longitudinal section seemed to indicate that this was the stem of a cryptogam, while the transverse section had all the appearance of an exogen. One tissue being obviously scalariform, the chief point of interest was the question whether the dark circles were really rings of annual growth. That they were really such the author argued on three grounds. First, no accidental infiltration of darker matter could account for a series of circular rings keeping their distance. Secondly the longitudinal section showed the same dark lines going down vertically through the stem and still keeping their relative distances, as in the concentric circles. Thirdly, on laying open the structure and examining the cause of the greater darkness of these rings, it is found to be due to the greater narrowness of the vessels forming the wood of the stem. The dark lines at their sides are crowded closer together. Thus it turns out that these circles were formed just as the rings of growth are in exogenous plants of the present day. The external markings of the stem were too obscure to determine the germs of this plant in the Carboniferous flora. Along with it were found specimens of *Stigmaria ficoides*, Flabellaria, and Lepidodendron. But the most abundant organisms in the bed were *Knorria acicularis*, now recognised as the internal cylinder of Lepidodendrons. Probably this was a similar structure of some other trees of the early Carboniferous period.

Prof. W. C. Williamson read a paper *On the Classification of the Vascular Cryptogamia as affected by recent Discoveries amongst the Fossil Plants of the Coal Measures*. After referring to the labours of Prof. W. King on this subject, for a knowledge of whose excellent paper he was indebted to Mr. Carruthers, and having dwelt at some length on the structure as interpreted by him of the stem of Lepidodendra, &c., he remarked that the conclusion to be drawn from the study of the structure of these fossil cryptogamic stems is, that so far as the structure of their medullary axis and ligneous zone is concerned, they are not in any sense *acrogens* but *exogens*; that they have a pith, consisting, in the lower fossil Lepidodendra, of a mixture of cells and vessels; that as we ascend to the higher forms the cells separate from the vessels, the former assuming a central and the latter a peripheral position; that the woody zone surrounding the medullary axis consists of radiating lines of vessels, which increase by successive additions to the external surface of the zone, which vessels are separated by mural arrangements of cellular tissue constituting medullary rays. Consequently, when such a process of growth has gone on until the result was a tree with a stem two or three feet in diameter, the application of the term *acrogen* to such cases is simply absurd. Such being the case, Prof. Williamson proposed to separate the vascular cryptogams into two groups, the one comprehending Equisetaceæ, Lycopodiaceæ, and Isoetaceæ, to be termed the Cryptogamiæ Exogenæ, linking the cryptogams with the true exogens through the cycads; the other called the Cryptogamiæ Endogenæ, to comprehend the ferns, which will unite the cryptogams with the endogens through the Palmaceæ.

Mr. Carruthers said:—The difficulties towards my accepting Prof. Williamson's interpretation of these plants are indicated by the terms which Prof. Williamson uses when he speaks of a vascular pith, and of medullary rays containing vascular bundles. The plants were true cryptogams, and in their organisation agreed in every essential point with the stems of Lycopodiaceæ. It was consequently necessary to apply to vascular tissues which had the position of medulla and medullary rays such names when these agreed with structures in the plants so closely allied to to them. The variations in recent stems of the Lycopodiaceæ were as great as in the fossil, and in some an average amount of the tissues agree with and fully illustrate the stems of Lepidodendron. In regard to the application of these structures to recent cryptogams, it was certain that an adherence to vegetative organs would set aside the natural classification based on the reproductive organs. And, indeed, the views advocated by Prof. Williamson would separate plants so closely related as the Hymenophylleæ and the Polypodiæ. It was important that in the fossil, as well as in the recent vascular cryptogams, the most satisfactory materials for determining their systematic position were obtained from their organs of reproduction.

Dr. M'Nab said:—I am very sorry I cannot agree with Prof. Williamson in his interpretation of the structure of these stems. Botanists are all agreed in this, that Lepidodendra and their allies are closely related to the lycopods. Now we know that the lycopods, like the ferns, have closed fibro-vascular bundles; bundles which can only grow for a certain time, and then all the cambium being converted into permanent tissue, growth must cease. It seems to me that the key to these structures is to be met with in Lycopodium Chamæcyparinus, in which we have a cylinder of wood-cells surrounding the central cylinder of united fibro-vascular bundles. This cylinder of wood-cells represents, and is a mere modification of the cellular tissue met with in the ordinary stems of lycopods. In this way it follows that the central portion is not a pith, but consists of the central group of fibro-vascular bundles. It also follows that the wood cylinder in these stems is not the homologue of the wood cylinder of an ordinary exogen. The classification of these plants proposed by Prof. Williamson seems also to me to be quite untenable.

Prof. Thiselton Dyer thought it was satisfactory that the papers on fossil botany were at last brought to the section where they could be properly discussed. It was most important not to separate the study of recent from that of fossil forms. If this had always been remembered, a great deal of wasted money and labour might have been saved in the publication of imperfectly understood material. Prof. M'Nab's description of the homologies of the stems in Lycopodium and Lepidodendron was the one accepted by all botanists who had looked into the matter. Prof. Williamson's classification was botanically untenable; it traversed every canon of classification. It separated the Equisetaceæ from the Ferns, and placed them with Lycopodiaceæ, with which they had nothing in common. The two types of stem which existed in the recent higher cryptogams existed equally in the extinct forms.

Prof. Williamson, in reply, said he did not attack a classification based on the organs of fructification, but that based on growth, and reiterated his belief that we had here a series of cryptogamic plants with an exogenous growth of their stems.

### SUB-SECTION.—ANATOMY AND PHYSIOLOGY

Professor Humphry read a paper *On the Caudal and Abdominal Muscles of the Cryptobranch*. He gave a general account of these muscles, and drew the following inferences:—1. That the abdominal muscles are an extension and expansion of the caudal muscles. 2. That the several abdominal muscles are derived from one simple muscular sheet, which is segmented into planes by difference in direction of the muscular fibres at different depths. 3. That the fibres of the external and internal oblique muscles are continued into those of the rectus, a gradual alteration from an oblique to a straight direction being observed in the fibres as they approach the middle line. 4. That the ilium and the ribs are the result of ossification in the course of the intermuscular septa and chiefly in those parts of their thickness which correspond with the plane of the internal oblique muscle.

### SUB-SECTION.—ANTHROPOLOGY

Lieut.-Col. Forbes Leslie read a paper *On ancient Hieroglyphic Sculptures*. All the hieroglyphics on Scotch rocks and monoliths can be assigned to two distinct types, the earliest of which consisted for the most part of circular cups or cavities worked in the stone, and also of circles, or parts of circles, variously combined. The second type, which seems to have superseded the first, did not entirely reject its figures, and, from the territory in which it is alone found, may be termed Pictish. The earlier form is found widely distributed on monoliths in Scotland, while the later form is much more restricted. The hieroglyphs were symbols of religious ideas.

In the next paper, by Dr. Conwell, *On an Inscribed Stone at Newhaggard, in the County of Meath*, curious hieroglyphic characters of an unknown age and inscrutable meaning were exhibited to the meeting.

Dr. Beddoe then contributed a paper *On the Inhabitants of the Merse*. There were, he said, two well-marked types, the one consisting of a rather long and narrow oval bead, almost Swedish or Frisian in form, light eyes and hair, and a tall, long-limbed figure; the other had a broader head, fuller temples, hair brown or light, and a robust frame. In stature and bulk the men of the Merse are hardly surpassed in Great Britain. Their large size may be inscribed to inheritance from the original Teutonic settlers of the district, and partly to the agency of a harsh and uncongenial climate, and a coarse, but plentiful diet of oatmeal, peasemeal, and milk. It is very probable, however, that the use of bread and tea instead of meal and milk, would cause a physical degeneration in the future.

Then followed a paper by Mr. J. W. Jackson, *On the Atlantean Race of Western Europe*, and *A Description of Paleolithic Implements*, by Mr. J. W. Flower. And in the absence of the authors of some of the papers Mr. W. Boyd Dawkins gave an account of the origin of the domestic animals of Europe. None of them date so far back as the Quaternary age. The sheep, goat, the small short-horned ox (*Bos longifrons*) the domestic *Urus*, the domestic horse, the dog, the tamed wild boar, and the turf-hog, to which all the European swine can be traced, appeared in Europe at the same time in the Neolithic age. He argued that they were probably derived from the East, and imported by a pastoral people from the central plateau of Asia. The evidence afforded on the point by the southern forms of vegetation found along with this group of animals in the Swiss lakes adds considerable weight to this view. In Britain, down to the time of the English invasion, there was no evidence of any larger breed than the small short-horned *Bos longifrons;* the larger breed of the *Urus* type were probably imported by the English, and is represented at the present day in its purity by the white-bodied, red-eared Chillingham ox. In the course of the discussion Dr. Sclater fully agreed with the views of the speaker as to the eastern origin of our domestic animals, since the East is the only region in which the wild ancestors of the domestic breeds are now found.

The President then read a paper *On Human and Animal Bones and Flints from a Cave at Oban, Argyleshire*. The cave contained the remains of man associated with flint-flakes and the bones of the red and roe deer, fox, otter, and possibly reindeer. The human teeth were unground, and contrasted strongly in their preservation with those of modern civilised races; the leg-bones also presented features which possibly may be platycnemic. The date of the cave is uncertain, but the association of flint implements with the human and animal bones pointed to a considerable antiquity.

W. Boyd Dawkins made some remarks on the Classification of the Palæolithic Age by means of the Mammalia. The eminent French naturalist, M. Lartet, acting on the *a priori* consideration, has attempted to divide up the palæolithic age into four distinct periods. "L'age du grand ours des cavernes, l'age l'elephant et du rhinocéros, l'age du renne, et l'age de l'aurochs." He said the very simplicity of the system had made it popular. A cave bear is found in a bed of gravel of a cave, and you put it down to the period of the great bear; you find an aurochs, and forthwith assign it to the latest age. There are, however, two fatal objections to this mode of classification. In the first place, nobody could expect to find the whole Quaternary fauna buried in one spot. One animal could not fail to be better represented in one locality than another, and therefore the contents of the caves and river deposits must have been different. The den of a hyæna could hardly be expected to afford precisely the same animals as a cave which had been filled with bones by the action of water. It therefore follows that the very diversity which M. Lartet insists upon as representing different periods of time, must necessarily have been the result of different animals occupying the same area at the same time. In the second place, M. Lartet has not advanced a shadow of proof as to which of these animals was the first to arrive in Europe. From the fact that the Glacial period was colder than the Quaternary, it is probable that the Arctic Mammalia, the mammoth, woolly rhinoceros, and the reindeer arrived here before the advent of the cave bear. It is undoubtedly true that they died out one by one, and it is very probable that they came in also gradually. The fossil remains from the English caves and river-deposits, as, for instance, those of Kent's Hole or Bedford, prove only that the animals inhabited Britain at the same time, and do not in the least degree warrant any speculation as to which animal came here first. Nor does it apply to France or Belgium, for in the Reindeer caves of both these countries the four animals in question occur together—the Mammoth with the Reindeer and the Aurochs with the Urus. In Belgium, indeed, the reindeer was probably living in the Neolithic bronze and iron ages, since it lived in the Hercynian forest in the days of Julius Cæsar.

## SECTION E.

At the opening of this Section on Monday morning two very interesting papers were read by Mr. C. R. Markham, *On the Recent North Polar Expeditions*, one was by Dr. Copeland, on the Second German Arctic Expedition, and the other by Capt. Ward, R.N., on the American Arctic Expedition. Both papers contained details of great interest, some of which have, however, already been published in Petermann's *Mittheilungen*, and elsewhere; and it is impossible in a short space to give even an abstract of them. They gave rise to interesting discussions.

The proceedings of this Section closed on Tuesday, August 8, when Mr. A. Buchan read a paper *On the Rainfall of the Northern Hemisphere in July contrasted with that for January*. The paper was illustrated by chart showing the distribution of rain in inches over the greater portion of the northern hemisphere in July. Mr. Buchan described the principles which guided him in drawing lines representing the rainfall of the globe—namely, to reject all places which, being in the immediate vicinity of hills or rising grounds, did not represent the average rainfall of the district; secondly, he drew lines of rainfall for each month separately. The months of July and January were selected, because in these months the greatest effect of heat and cold on the earth's atmosphere and its movements occurred. In July the line of the rainfall passed through the south of Spain, the north of Africa, through Syria, and thence westwards into the desert of Cobi, thus forming the northern boundary line of the rainless region of this part of the globe in July. The map further showed that the greatest amount of rainfall occurred in the centre of the continent of Asia and Europe, taking them both as one continent; and that the line of greatest rainfall passed through the centre of Europe and towards the centre of Asia to some distance north of the Caspian. In India, the line of the rainfall passed a little to the west of the Ganges, east of which the lines representing inches could not be shown; and the whole of this region was therefore marked by a deep red to show the rainfall was enormous; and the rainfall was also very excessive in Further India, and in the east of Asia generally. In America the line of the rainfall included California and the neighbouring regions. Very heavy rainfall occurred in the lake district of the north-western sides which sloped eastward—that is, those to the east of the mountains; but the heaviest rainfall occurred in the sides bordering on the Gulf of Mexico and the whole of the eastern slope of Central America. In the map contrasting the rainfall of July with that of January, there were two sets of lines—blue and red, the red showing those regions at which the rainfall of July exceeded that of January, and the blue those regions where the rainfall was less than that of January. Mr. Buchan showed that where there were prevailing winds blowing into warmer latitudes the rainfall was not defective, even though those winds came from the ocean, and illustrated his remarks by the summer rainfall of the south of Europe and the north of Africa, and by that of California. The greatest excess of the rainfall in July was in those regions to which the prevailing winds arrived after having traversed a vast extent of ocean, India and Central America. Illustrating this connection, on the western slopes of the British Isles the rainfall in July was less than that of January, but on the eastern slopes it was greater. In July, when the prevailing winds blew from the Atlantic eastwards into the centre of the great continent, the rainfall of the hills of this immense tract was greatly in excess in July of what it was in January. Mr. Buchan also pointed out the importance of inquiry in reference to the great movement of the atmosphere, especially the vapour which was condensed into rain, and which must come from some neighbouring surface. The important bearing of the subject on physical geography and climate, and the distribution of vegetable and animal life on the globe, was also pointed out.

On the conclusion of the paper, Colonel Yule remarked that Mr. Buchan had not gone beyond six inches in his calculations, but he wished to state that in the place where his earliest service began—in the district of Assam—there fell, in the month of August 1841, 30 inches of rain on six days continuously, or 180 inches in all, while the whole rainfall of Edinburgh for a year was about 26 inches. During that same month of August the rainfall was 264 inches, or 22 feet. He thanked Mr. Buchan heartily for his paper, and hoped that his maps and observations would be published before long in a shape in which they could all have access to them.

The only remaining paper of more than pure geographical interest was by Captain L. Brine, R.N., *On the Ruined Cities of Central America*. It stated that it was not until the year 1870—more than than 200 years after the Spanish conquest—that the existence of ruined cities and temples lying hidden in the jungles and forests of Central America was revealed to the knowledge of the Spanish Government. A small party of Spaniards, travelling in the State of Chiapas, happened to diverge from the usual track leading from the southern limit of the Gulf of Mexico to the Mexican Cordilleras, and accidentally discovered in the dense forest remains of stone buildings palaces and temples, with other evidences of a past and forgotten civilisation of a very high order. These ruins were those of Palenque. Some years subsequently to this discovery, the King of Spain ordered an official survey to be made, and this survey was made in 1787 under the direction of Captain del Rio. Later official surveys were also made in 1806 and 1807; but these, with the usual secresy of the Spanish conquerors, were not generally made public, and thus it happened that only as recently as the year 1822, at the revolution of Mexico, did the existence of these ruins first become known in Europe. Since then other hidden cities or temples had been discovered—Copan, in the State of Honduras; Ocosingo, on the frontiers of Guatemala; and several in Yucatan, of which Uxmal and Chichen Itza are the most famous. It was very remarkable that all these ruins, evidently the work of one particular and highly-civilised race of Indians, should only be found in a very limited area. None exist in South America, and none in that part of the continent commonly distinguished as North America—they all lie within the tropics, between the 14th and 22nd parallels of north latitude, and were chiefly adjacent to the Mexican and Honduras Gulfs, or in the plains on the west of the Cordilleras of Central America. On the eastern or Pacific slopes and plateaux, within the same parallels, are also remains of ancient fortifications and sacrificial altars, but these are of a less elaborate type, and are allied to the Aztecan structures of Mexico. The paper went on to give an interesting account of a journey undertaken by the writer across the continent, in the spring of last year, from the Pacific, through Guatemala to the Atlantic, to enable him to examine in detail the mixed populations and conditions of the lands between the Cordilleras and the Pacific, the central plateaux, with their aboriginal Indian races and ruins, the region—almost entirely unknown—inhabited by those unbaptised Indians called the Candones, near which lie the ruins of Ocosingo and Paleque, and finally concluding the journey by traversing Yucatan, visiting the strange ruins with which the country abounds, and emerging on the northern coast of the peninsula at Sisal.

## SCIENTIFIC SERIALS

THE *Journal of Anatomy and Physiology*. Conducted by G. M. Humphry, M.D., F.R.S., Professor of Anatomy in the University of Cambridge; and William Turner, M.D., Professor of Anatomy in the University of Edinburgh. No. VIII. May, 1871 (Macmillan and Co.).—This number is quite up to the standard of its predecessors, but the papers it contains are so numerous that we can do little more than indicate the subjects of most of them. Mr. Perrin heads the list with a couple of papers on muscular variations observed in the dissecting room of King's College, London, during two winter sessions; and Mr. Wagstaffe, demonstrator at St. Thomas's, Mr. Bradley, of the Manchester Medical School, and Mr. Cameron contribute similar papers, and thus illustrate one great use of the journal, for without it such observations would probably go unrecorded.—Mr. W. A. Hollis gives an account of the so-called salivary glands of the cockroach, and seems to show satisfactorily that they are really part of the tracheal system of the insect, and not glandular at all.—Dr. Wickham Legge contributes some observations on the physiological action of hydrochlorate of cotarnamic acid, a derivative of narcotine obtained by the late Dr. Matthiessen; the most interesting points about the new poison are the length of time (often several days) which elapses before its effects show themselves if it be administered by the mouth, and the great diminution of blood pressure and the paralysis of the cardiac branches of the vagus which it produces.—Mr. Garrod, of St. John's College, gives an account of a very simple cardio-sphygmograph which appears likely to prove useful, and also a description of the telson of Schyllus arctus, in which he endeavours to show that it is not a mere azygos appendage as it is usually supposed, but is a true body segment, possessing appendages of its own.—Dr. Wilson Paton has a paper on the influence of certain drugs, of diet, and of mental work, on the urine; one of his most important results being that neither the infusion, alcoholic extract of alkaloids of broom tops, have any effect in increasing the quantity of any of the constituents of the urine, at least in health, although they are so commonly regarded and prescribed as diuretics.—Prof. Cleland gives an account of a case occurring in his practice which showed that the trapezius plays an important part in keeping the bones of the shoulder joint in contact; he also describes a case of epispadias.—Prof. Rutherford describes a modification of Stirling's section machine, which is especially fitted for getting microscopic sections of frozen tissues, and also gives some experiments on the excitability of the trunk of a spinal nerve which go to negative Pflüger's "avalanche" theory.—Dr. Kennedy contributes an account of a young Aino cranium; and Prof. Turner concludes the original articles of the number with papers on the "Two-headed ribs of whales and man" and on the "Transverse processes" of the seventh cervical vertebra in Balænoptera Sibbaldii. The review of the recently published works bearing on the natural selection theory is peculiarly full and interesting, and the reports on the progress of anatomy and physiology during the preceding three months, which conclude the number, are drawn up with their usual completeness.

*Symon's Meteorological Magazine* has now reached its fifth yearly volume, and it maintains its character of being a useful monthly medium for the interchange of meteorological jottings, which are not of sufficient importance to form papers for scientific societies. It contains, in addition to reviews and abstracts or reprints of papers published elsewhere, some valuable notices of special investigations carried on by private observers, such as a discussion on solar radiation temperatures, conducted by the Rev. F. W. Stow and Mr. Nunes. The tornado of October 19, and, of course, the aurora of October 24 and 25, find a place in its pages. The standing portion of the magazine, however, consists of monthly rainfall returns and notes on weather from about fifty stations, and thus forms a sort of supplement to the annual volume, "British Rainfall," brought out by the same author.

*Journal of the Chemical Society*.—The last number of this journal contains the "abstracts of chemical papers" which have been already noticed in our columns, and two papers read before the Society, the first being "The Action of Heat on Silver Nitrite," by Dr. Divers. The author finds that when silver nitrite is submitted to the action of heat it is decomposed, the products of the action consisting principally of silver nitrite, metallic silver, and oxide of nitrogen, but that the relative proportions of these vary according to the conditions of the experiment. When the nitrite is heated in an open dish, the result may be represented by the equation $3\,NO_2Ag = N_2O_3 + Ag_2 + NO_3Ag$, but if it is heated in a vessel nearly closed, so that the gaseous products may be kept in contact with the undecomposed nitrite, the loss of weight is less, and the amount of nitrite formed is greater, the hot silver nitrite apparently reducing the higher oxides of nitrogen to nitric oxide. When the nitrite is freely exposed to a moist atmosphere, and heated, it tends to yield only metallic silver and nitrogen peroxide. Mr. Gill, in "Laboratory Notes on the Examination of Glucose containing Sugars," after remarking on the effect produced by the use of an excess of lead subacetate in decolouring sugar solutions for optical examination, the action of inverted sugar on polarised light being greatly altered by the presence of this reagent, proposes the use of a strong solution of sulphur dioxide as a satisfactory method for removing the lead.

IN the *Journal of Botany* for August the most interesting article is a "Flora of Hyde Park and Kensington Gardens," by the Hon. J. L. Warren. This apparently unpromising field for botanising yielded to a careful search no fewer than 190 species of indigenous flowering plants, some half-dozen of them by no means common plants, and the list might probably be considerably extended. A hundred years hence this list will be of considerable interest to the botanist of the future. The other original articles in this number are of a more technical character.

## SOCIETIES AND ACADEMIES

### BRISTOL

Observing Astronomical Society.—Observations to July 31.—*Solar Phenomena*.—Mr. T. W. Backhouse, of Sunderland, observed a large spot in the sun's south hemisphere from the 12th to the 22nd of July. He obtained the following measures of its dimensions:—

| Date. | | Penumbra. Length. Miles. | Umbra. Length. Miles. | Umbra. Breadth. Miles. |
|---|---|---|---|---|
| July 12 | 9.12 a.m. | — | 20,000 | about 10,000 |
| July 15 | 9.15 a.m. | 36,000 | 17,000 | — |
| July 18 | 7.45 a.m. | 37,000 | 22,500 | 14,500 |
| July 20 | 7.55 a.m. | 41,000 | 27,500 | 18,000 |
| July 22 | 9.15 a.m. | — | 22,500 | 7,500 |

"It was comparatively small on July 9. The umbra was one of the largest I have ever seen."

*Comets I. and II.*, 1871.—Mr. John Birmingham, of Tuam, reports that he "had several observations of Comet I., from April 22 to May 8, but under very unfavourable circumstances, caused by the state of the atmosphere and strong twilight and moonlight. Still notwithstanding its faintness a nucleus was easily detected, and the comet seemed in general to present a granulated appearance. On April 22 it was not visible in the finder, but bore magnifying up to 126 very well. There was a slight elongation in the normal direction of a tail. By the best measurements that I was able to apply, the comet seemed always slightly in advance of the position computed by Pechüle. On July 17, at $12^h$ $15^m$, Dublin mean time, I first found Comet II., the cloudy weather having rendered a previous search ineffectual. This comet was of extreme faintness, and without the sharpest attention it might easily pass unnoticed across the field. When first observed it was in contact with a small star, not identified, from which it gradually detached itself, and its position seemed to agree well with Pechüle's calculation. It was best seen with 56 and 99, and with the latter, after intent gazing, the momentary flickerings of minute points in its misty form could be caught at instants of good definition. This so strongly suggested the appearance of a nearly resolvable cluster that I was not satisfied with the comet's identity until I perceived its motion. Previous to this observation I had not read the description of the object by Herr Tempel, the discoverer, but subsequently I was pleased to see his allusion to its appearance as if 'sprinkled' with little stars towards the middle. If the light of the comet is sufficient, I shall not be surprised to hear of its giving indications of a continuous spectrum in addition to the usual bright lines." Mr. Charles Hill, of Bristol, also observed Comet II. on July 18, and found that it was an exceedingly faint object. It was scarcely perceptible in his $8\frac{1}{4}$ in. equatorially mounted reflector.

*Venus.*—Mr. Henry Ormesher has succeeded in detecting the dark markings on several recent occasions. On April 22 they were rather pale, but the terminator was clear and well defined. On May 10 they were well seen, and appeared to him to be very similar to the dusky markings on the surface of Mars. On May 21 and 29 he also saw the markings. Mr. H. W. Hollis examined the planet on July 17, at $6^h$ $30^m$, with his 6 in. O.G. power 150. "The rounding off of the southern cusp was evident at a glance, and the prolongation of the northern one more remarkable than I have ever seen it before. A dusky ill-defined spot, of uncertain shape, was visible. On the 18th, at 5h 15m, I suspected the presence of this spot again somewhat nearer the terminator, but of this I cannot speak positively." Mr. John Birmingham, of Tuam, writes: "I have been carefully observing Venus at every opportunity without detecting any definite markings besides the well-known peculiar forms exhibited by the cusps, which appear to me brighter than other parts of the planet."

*Saturn.*—Mr. H. W. Hollis reports as follows:—"On July 17, at $10^h$ $20^m$, I inspected Saturn on the meridian, and notwithstanding his low altitude, found the shadow of the ball upon the the rings clearly visible. I could trace Ball's division all round in moments in fine definition. But what especially interested me was that, under a power of 250, the eastern opening between the ball and the inner ring was unmistakeably wider than the western one. Of this I am certain. The air was too unsteady to admit of any operations with the micrometer."

PARIS

**Academy of Sciences**, August 14.—M. Faye in the chair. M. Dumoncel sent a note on the Influence of Electrodes in the Generation of Electricity. It is well known already by every working electrician that a cool surface may be enlarged with advantage for a certain distance and surface of zinc.—M. Demosquay sent a paper, which showed that many Communist Nationals were wounded when intoxicated. The result of it was an immense mortality amongst them, which could not be prevented by the most assiduous care. The temperature of the patients was remarkably low, which is always indicative of great danger.—M. Arson, chief engineer of the Parisian Gas Company, has conducted with great care for many years experiments with the view of correcting the compass from every variation produced by the iron contained in ships. These experiments were worked out in the great La Villette gas works, where the company keeps about twenty large gas-meters. A commission appointed for the purpose will in course of time publish an elaborate report. M. Lependry, engineer of the canalisation in the same company, has commenced to study all accidents analogous to the extraordinary gas-lighting by thunderbolts, described in M. W. de Fonvielle's memoir.—M. Resal, an engineer in the mining service, sent a note showing that connecting chains as they are used by railway carriage constructors are not strong enough.—M. Durand Claye, an engineer in the Ponts et Chaussées, sent a paper strongly advocating a radical change in the way of dealing with the sewage of Paris. The learned engineer shows, by conclusive arguments, that it is absolutely necessary to deal with these refuse waters according to the British system, as worked for years in Edinburgh, &c., &c. It is not the first time that learned members of our scientific administration have tried to naturalise that excellent method. But every real progress was stopped by the prevalent system of Imperial red tape and corruption. It is to be hoped that the French Republic will not lose time in putting an end to the disgusting waste of manure now permitted to poison a noble stream. M. Durand Claye proposed to irrigate the celebrated Gennevilliers peninsula, whose extent is only 5,000 acres.—M. Leverrier read a report on observation of the August meteors, of which a summary was published in the *Times* of August 17. He read also a letter from M. Coggia stating that his bolide was not an imaginary one. M. Elie de Beaumont expressed an opinion that possibly that extraordinary bolide was not of planetary origin, but merely some fire formed in our superior atmosphere, possibly by the agency of electricity. M. Henry Sainte-Claire Deville read a memoir written by MM. Nagi and Hautefluelle, two teachers in the normal school, on a peculiar fact of chemistry relating to a combination of chlorine of silicium, which appears to be destroyed from 700° to 1,000° C., and to be formed again from 1,000° C. to 1,500° C.—M. Charles Sainte-Claire Deville read a short note on a stroke of lightning which exploded within his meteorological observatory, two hours only before an academical sitting had taken place. The natural electricity was attracted by a leaden tube, in which an isolated metallic wire belonging to the telegraphic system of the observatory had been inserted. The leaden tube had performed the part of a condenser, and the spark was created on the spot of the explosion as the primary wire was in connection with the earth.

## BOOKS RECEIVED

ENGLISH.—Bird Life, Part 1: A. E. Brehon (Van Voorst).

FOREIGN.—(Through Williams and Norgate).—Geschichte der Glycosurie von Hippokrates bis zum amfange des Neunzehnten jahr hunderts: Dr. Max Solomon.—Ueber die Helligkeitsverhältoisse der Jupiterstrabanten: Dr. R. Engelmann.—L'Homme pendant les Ages de la Pierre: M. E. Dupont.

## PAMPHLETS RECEIVED

ENGLISH.—British Railway Reform: J. H. Watson.—Western Chronicle of Science.—Cassell's Book of Birds, Part XXII.—On the Spirit Circle: Emma Hardioge.—Transactions of the Boys' Literary Society, Sidcot School, 1870-71.—Sidcot School Board Chronicle.

AMERICAN AND COLONIAL.—What are they doing at Vaoar?—Fourth Annual Report of the Trustees of the Peabody Museum.—Transactions of the Entomological Society of New South Wales, Vol. ii., Part 2.

FOREIGN.—Bulletin de la Société de Géographie for May, June, and July, 1871.—L'Institut, No. 1,921.—Allegemeine für Deutschland, No. 33.—La Tour du Monde, No. 565.—La Révue Scientifique, No. 9.—Rendiconti, Vol. iv., No. 15.

## CONTENTS



*NOTICE*

*We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception.*

THURSDAY, SEPTEMBER 7, 1871

## *THE IRON AND STEEL INSTITUTE*

AFTER having on so many different occasions dwelt upon the importance and advantages to be derived from the cultivation of Science by those engaged in the industrial undertakings of this country, we cannot do otherwise than refer in terms of deep interest to a meeting, which took place during the past week at Dudley, under the presidency of Mr. Bessemer, in the heart of the oldest iron-making district of Great Britain.

About three or four years ago a few gentlemen in Cleveland, the youngest seat of the iron trade in the world, propounded the idea that it would be beneficial to all concerned to organise an association of those interested in the manufacture of iron and steel, to meet and discuss all matters connected with these branches of metallurgical science, but from which all questions of a merely trade character should be carefully excluded.

To dispel any idea of this Iron and Steel Institute, as it is designated, being intended by its original promoters to be confined to their own locality, they solicited and obtained the consent of the Duke of Devonshire to act as their first President. Looking at his Grace's position as one largely interested, but in an entirely different district, in the manufacture this body was intended to foster, and having regard to the literary and scientific attainments of this distinguished nobleman, a more judicious selection could not have been made.

From the day of the first introduction of this association to the public to the present, we find an unflagging interest has been maintained in the papers submitted at the gatherings, and in the discussions which have followed their reading. As a natural consequence we are glad to find that among its 450 members is included almost every name of note in this very important branch of our national industry.

We know of no manufacturing operation requiring for successful enterprise a more extensive acquaintance with scientific truth than that of the iron-smelter and its associated branches. His work is conducted in apparatus of a very costly and gigantic character, and under circumstances which render experimental research very difficult; while, on the other hand, its prosecution upon a commensurate footing is attended with so much expense as to render failure almost ruin.

It unfortunately happens also, that the pursuit of pure scientific inquiry connected with the subject is impeded by obstacles of no ordinary character. A blast furnace containing twenty or thirty thousand cubic feet of space and filled with nearly 1,000 tons of materials, chiefly in a state of intense ignition, is not a field to which the chemical philosopher can, without considerable preparation, transfer his labours. From the crucible of the laboratory to such an enormous and almost inaccessible mass, the focus of very intricate and violent chemical action, is too great a step to be made by the chemist for a few hours with any chance of success; for the very questions in which the iron-master would desire his assistance are the results of anterior circumstances, which themselves must be well known to him who attempts to explain their consequences.

On the Continent—in France, in Belgium, in Germany, in Sweden—there are to be found men of great reputation who have identified themselves with this union of science with art, because in these countries are to be found educational establishments so located as to afford the professors who fill the respective chairs abundant opportunity of making themselves personally acquainted with the action of the iron furnace, and, indeed, with every step in this and in other branches of manufacture.

We can adduce no better proof of the real value of the labours of the Iron and Steel Institute than the estimation in which they are held by some of the Continental professors, two of whom we noticed were present at the meeting to which we have alluded.

In our own country, without saying to whom the blame, if any, belongs, men of science and men of industrial occupation have not been brought sufficiently together. As a rule our schools of science are remote from the scenes where science is practically applied. In consequence our professors are, perhaps, less familiar with and less interested in, pursuits, which, in the eyes of a Leoben or Louvain teacher of metallurgy, possess sufficient attraction to induce him to undertake a long journey to be present at a meeting, or to study the operations of our own iron makers, rarely or never visited by the learned of their own nation.

It is this reflection, perhaps, more than any other, which has induced us to notice so favourably the proposition to found in the centre of a great mining and manufacturing district the proposed College of Physical Science at Newcastle-on-Tyne. We regard it as a desideratum no less important to the philosopher than to those who may seek for instruction within its walls, for it is one which will afford to him who has to instruct ample opportunity of studying the application of those great and important truths which it is his office to teach.

We cannot conclude this brief notice without heartily commending the spirit in which the members of the Iron and Steel Institute, throwing aside all narrow-minded prejudice and jealousy, communicate to each other the result of their own individual research, and make known for the use of all the progress each has effected in his own sphere. It seems to us that everyone is acting under the feeling that, on giving information, he is in reality promoting his own advancement. However this may be, society at large, not the least interested in their progress, cannot fail to profit by assistance thus rendered and received, and therefore we most cordially wish all prosperity to the Iron and Steel Institute.

## *INSTRUCTION TO SCIENCE TEACHERS AT SOUTH KENSINGTON*

DURING the months of June and July, a number of science teachers from various parts of England, Scotland, and Ireland, were assembled in London, for the purpose of attending special classes, arranged for their instruction under the auspices of the Science and Art Department. We propose to give some account of the course of instruction in the principles of Biology, which was directed by Prof. Huxley, to whose suggestion, we believe, liberally accepted by Mr. Forster and acted upon by the Government, this important scheme for raising the character of science teaching in the various schools and classes at present in relation with South Kensington is due. It had long been felt by those who annually examined teachers and pupils for certificates in various branches of science, under the Science and Art Department, that the candidates displayed a sad want of practical acquaintance with the subjects in which they presented themselves for examination; many showed considerable ability and great book knowledge, but a knowledge of the things themselves with which science deals, a proof of personal intercourse with Nature, which after all is the only foundation of scientific knowledge, and without which all the 'ologies are so much bookwormery, was to a very great extent wanting. Under the existing state of things it seemed almost impossible to get out of this vicious condition, for the scholars who were in their turn destined to become teachers were for the most part taught by men who were deficient in practical knowledge; and with the increasing demand for science teaching there appeared to be a probability of the evil being increased by the rapid accession of these book-taught students to the position of instructors. The only way to meet this difficulty was to find teachers who had the requisite familiarity with "the solid ground of Nature," and set them to work to leaven the mass. The readiest means of doing this was undoubtedly that adopted by the authorities—namely, to summon to a central class the ablest of the teachers at present distributed throughout the kingdom, and to impart to them as much practical acquaintance and personal familiarity with the *things* of which they had read in books, as was possible in a given time. By annual repetition of this plan there can be little doubt that the body of science teachers throughout the country would be materially affected. Being already acquainted with the outlines and much of the detail of their subjects by hearsay, they would readily understand and appreciate the facts and methods of investigating facts placed before them, and after passing through such a course of instruction would be prepared to proceed further in the same direction by their own individual efforts, and what is more important, to teach, not at second-hand, but from experience, not as fluent repeating machines, but as thoughtful students of phenomena.

Thirty-nine students, of whom one was a lady, attended the course of instruction in the principles of Biology, their expenses (involved in coming to London) being defrayed by the Government. The course occupied six weeks; the students attended every day, with the exception of Sundays, from ten in the morning until half-past four in the afternoon (Saturdays until two). Each morning at ten o'clock a lecture, occupying from an hour to an hour and a half, was given by Prof. Huxley, and the remainder of the day was employed in dissection, microscopic work, and demonstrations, in carrying out which Prof. Huxley was assisted by Prof. Michael Foster, Prof. Rutherford, and Mr. Ray Lankester. The students were placed in pairs at large working tables, and each table was provided with a microscope (with inch and one-eighth inch objectives, and two eye-pieces furnished with micrometric square-ruling), with four scalpels, two pairs of scissors, two pairs of forceps, pins, thread, dissecting needles, watch-glasses, beakers, pie-dishes, glass tubing, and camel's-hair brush.

The practical instruction proceeded *pari passu* with the lectures, the students at once taking their places at the tables after the lecture, and setting to work at materials provided for them to dissect or examine with the microscope in illustration of, or rather as the sequel to, the lecture which they had just heard. Each student was required to send in full reports and drawings as the result of his day's work, many of which proved very excellent; an abstract of the lecture was also given in by each student, with the report of his practical work, and the lot were returned at the end of the course (after due examination by the lecturers) to the students for their future reference. Two prizes—which were two microscopes similar to those used by the members of the class, and provided like them with inch and one-eighth inch objectives—were offered to the students who should be considered to have done best during the course, especial weight being given to excellence in the practical work, as judged both by observation of the student when at work, and by the reports sent in. The names of the students were placed in two classes of merit at the termination of the course, arranged in alphabetical order.

Now as to the subjects which were gone over in the time, which, though limited to six weeks, yet, by dint of hard work, was made to take in more than many a six months' course. The yeast plant occupied the first lecture, and each student was provided with some yeast, which was carefully examined and drawn under the microscope. Each student sowed some in Pasteur's solution which he had himself prepared, and on the following day studied its germination. In like manner the Penicillium mould was studied, sections being cut through the crusts, and careful drawings made of mycelium, hyphæ, conidia, &c. The latter were sown, and their development accurately observed and drawn by each student. A solution of hay was given to each, and the formation of a Bacterium film was studied, the form and movements of Bacteria were compared with the Brownian movements of gamboge rubbed up in water. The structure of the higher Fungi was then studied in specimens of a common toad-stool, and thus a general notion of the morphology and life-history of the Fungi was obtained. Protococcus in its various stages, Palmella, and Volvox next formed the subjects of lectures and practical work, and from these simpler forms the students passed on to Spirogyra and Chara. In Chara the advance in cellular differentiation was noted by each student on specimens supplied to him, and the male and female reproductive bodies examined in detail, and the Antherozooids were obtained in active movement. The phenomenon of cyclosis was also very carefully gone over, each student comparing that of Chara with that seen in Valisneria, and in the hair of the nettle and of Tradescantia; drawings and descriptions being made and the specimens prepared by every student for himself. During this time a certain amount of familiarity had been obtained by all with the use of the microscope—not half a dozen of the class, be it remembered, having previously ever used the instrument at all, still fewer one of adequate power—and as well as the instrument itself, the use of various reagents had been learnt, such as iodine-solution for demonstrating starch, and for delineating protoplasm, acetic acid, magenta-solution, &c. From Chara the class proceeded to the study of the Fern—the sori and sporangia were examined in the first place, and the general form of the fern-frond; then each student was provided with spores which had been previously allowed to germinate, of two stages of development, the one set with the quite young pro-embryo-like prothallium, the other more advanced exhibiting numerous archegonia and pistillidia, the structure of all of which were examined and drawn; and in many cases active antherozooids were obtained. The structure of the fern stem followed, exhibiting typical scalariform, dotted and spiral ducts, and other forms of tissue; also the leaf of sphagnum; the methods of recognising starch and cellulose being here again used. From the fern the class passed on to the study of a bean plant as typical of a phanerogam. Its general morphology, the microscopic structure of its tissues, the minute structure of the flower and the histology of the essential reproductive organs were examined during three consecutive days, and finally the development of the seed and the growth of the young bean plant were studied.

In this work each student used a razor for making sections of the parts to be studied, and portions of turnip were made use of for embedding delicate pieces of tissue, such as leaves, in order to facilitate the cutting of thin sections. A few typical flowers (*e.g.*, Campanula, Rosa, Viola, various Orchids) were next studied as examples of the kind of modification of parts exhibited by phanerogamous plants, and also the female flowers of a small Conifer. Before proceeding to the animal kingdom, a lecture was devoted to a retrospect of the steps through which the class had passed from the simple to the more complex forms, a comparison of the various methods of reproduction, and an outline of the physiology of vegetable life.

Amœbæ, the colourless corpuscles of the Triton's blood, and the amœboid particles of Spongilla, were the first examples of animal life studied, each member of the class making drawings of the various forms due to protoplasmic movement presented by an individual example of each of these cases of simple organism whilst in the field of his microscope. The Gregarinæ of the earthworm next occupied a day, and every student was able to observe and draw the actively moving nucleated Gregarina, its simple encysted condition, and its various stages of breaking up into pseudonaviculæ.

The structure of Infusoria was next examined, as exemplified in Vorticella and Vaginicola, the nucleus, contractile vacuole, mouth, &c., being fairly observed and drawn by all the students. Specimens of Hydra were provided on the following day, and the endoderm and ectoderm, thread-cells and reproductive organs studied. To this followed a copious supply of *Cordylophora lacustris* (from the Victoria Docks), in which the class were able to study a typical compound Cœlenterate, and to recognise not only the male and female gonophors, but the larval "planula-form" as it escaped from the reproductive capsules. Plumatella as a typical Bryozoon succeeded this, and then two days were given to the dissection and histology of Anodon, of which each student was provided with two or three specimens. The lobster as a typical Arthropod was then examined, a fresh specimen being supplied to each table; the heart and vessels were first studied, then the alimentary canal, liver, reproductive organs, and green glands. A large piece of mill-board covered with paper was used by each pair of students for placing out in order, numbering, naming, and comparing the twenty somites and their appendages, an instructive preparation being thus made. The corresponding parts were again examined, and the microscopic structure of the muscular tissue, blood, liver, and gills, in specimens of the river cray-fish. The careful dissection of the frog next occupied some days, and to this succeeded the rabbit.

Simultaneously with the dissection of these vertebrata, the study of the microscopic structure of the various tissues and organs was commenced, so that whilst one student was using the microscope, his companion at the table was dissecting, and *vice versâ*. The blood of the frog and of man, the movements of the colourless corpuscles in both cases, and the action of acids on them, the varieties of epithelium, the various forms of connective tissue and its corpuscles, cartilage, bone, muscular tissue smooth and striped, nerve fibres and cells, the termination of nerve in muscle, and the structure of the more important organs, were examined by the class, *not* in already prepared and mounted "slides," but in specimens which each student took for himself, usually from the animal under dissection, and treated with various reagents, the methods of cutting thin sections and embedding tissues in wax or paraffin being learnt at the same time.

A simple injecting apparatus (formed by two Wolff's bottles and a large vessel of water) was put up, and the method of injecting a frog shown to each student. The best part of a day was spent in a thorough dissection of a sheep's heart, and another in the dissection of the sheep's larynx. Vertical antero-posterior sections of the sheep's head were supplied to the various tables, and in these the parts of the brain and cranial nerves (already made out in the rabbit), the tongue, the relations of the cavities of the mouth, nose, and ear, the ducts of the salivary glands, and the muscles of the eye, were studied. The structure of the eye was again examined by each student, in specimens of those of the bullock, supplied in quantity, and the internal ear and auditory ossicles were demonstrated in rough preparations of the sheep and rabbit.

But little time could be afforded to Physiology; and, indeed, it was hardly possible that each member of the class should perform many physiological experiments for himself. The movements of the heart in the frog after excision, and the localisation of the nerve-centre, was made out by each student for himself; also the phenomena of reflex action in the frog after the destruction of the cranial portion of the cerebro-spinal nervous system. Again, each table was supplied with simple galvanic forceps, and the irritation of nerve and of muscle examined, also the action of chemical and mechanical stimuli on the nerve. The action of curare poison on the frog (Bernard's experiment) was examined by every student, and the condition of the poisoned and the unpoisoned leg compared. Every member of the class was made familiar with the simplest way of demonstrating the circulation in the frog's foot, tongue, and mesentery, under the microscope, and repeatedly examined the phenomenon for himself. Rigor mortis and the artificial rigor produced by warm water were examined. The conversion of starch into sugar by the saliva, and the methods of proving the presence of starch and grape sugar, were made the subject of experiment by every individual of the class. The peristaltic movements of the intestine and the absorption of the chyle by the lacteals were exhibited and closely examined. A model of the circulation, consisting of india-rubber tubes and pump, was used for demonstrating the nature of the pulse, the pressure (by means of manometers placed in connection) in the arteries and veins, and the effect of dilatation and contraction of the capillaries and of rate of pulsation on this pressure. Finally, the thorax was opened in a narcotised rabbit, and the heart exposed, and each student satisfactorily witnessed the pulsations of that organ and the inhibitory effect of irritation of the vagus nerve; the blood-pressure was exhibited to each member of the class in a similarly narcotised dog by means of the hæmodynamometer, a tube being placed in the animal's carotid artery; and as a concluding demonstration the important fact of the influence of nerves upon gland secretion was demonstrated by the beautiful experiment of Bernard, the chorda-tympani being irritated, whilst a canula was placed in the duct of the submaxillary gland. Great care was taken that none of the experiments exhibited to or performed by the members of the class should be open to the charge of cruelty, the animals used being either completely narcotised, or (as in the case of the frogs) having the cerebral portion of the nervous system destroyed in the proper manner.

Throughout the course the morning's lecture was made preparatory to or an extension of what was afterwards brought under actual observation. The concluding lecture was devoted to a retrospect of the work which had been gone through, and an exposition of the idea which had guided the scheme of study pursued, the object having been not to make botanists, nor zoologists, nor anatomists, of the members of the class, but to give them a practical insight into the structure and activities of living things, in such a way as to enable them to observe for themselves the relations and connections of the various forms of life, and to follow from actual examples the characteristics and increasing complexity of different plans of structure.

The reports of work and lectures daily sent in by the members of the class were entirely satisfactory, and the spirit and enthusiasm displayed throughout proved how greatly the value of the course was appreciated. When it is remembered that, with scarcely an exception, these teachers had hitherto never used the microscope, never dissected a single organ or organism for themselves, nor seen one properly dissected, the advantage gained by the experience they have now obtained, even if only a portion of what was condensed into six weeks' work remains with them, is something very considerable, for it is something of a *new kind*, a form of knowledge which they had entirely failed to obtain before.

It is exceedingly interesting to find that no difficulty was experienced in going over all these matters in a class which was not confined to men alone, and most heartily do we hope to see in the future a larger proportion of women engaged in this and other branches of scientific study. Those who imagine that women have some innate incapacity, and assert that if admitted to classes now limited to men they would be unable to profit by them, or would hinder the progress of the class by the greater attention they would require in order to keep them to the level of male students, may take this fact to heart—one of the microscopes offered as a prize for the best work done, and the best record of the lectures and the day's work, was adjudged simply upon the merits of her reports and work to the one lady among the thirty-nine students who formed the class. On the other hand, this fact will probably stimulate that unavowed feeling, akin to the trades-unions' hostility to competition, which is the cause of the arbitrary exclusion of one half of the community from our greatest educational institutions. E. R. L.

## *MAGNUS'S BONES OF THE HEAD OF BIRDS*

*Untersuchungen über den Bau des Knöchernen Vogelkopfes.* Von Dr. Hugo Magnus, Assistentarzt an der Klinik des Herren Prof. Dr. Förster, zu Breslau. Mit sechs Tafeln. (Leipzig: Engelmann, 1870; pp. 108. London: Williams and Norgate.)

THIS work is a systematic description of the form, structure, and relations of the various bones of the head of birds. Each bone is taken separately, and the chief varieties it presents in the several sub-classes are described, and are illustrated by carefully drawn plates. In here giving a brief notice of the work, we need scarcely say that the details of the several bones in the adult state are very well and clearly given, and the author has had opportunities, of which he appears to have thoroughly availed himself, of studying and comparing the skulls of a large number of birds. The mode in which the variations from typical structure are given is instructive and accurate. We will give an extract to show the mode in which he deals with the subject, and select a part of his account of the squama of the Temporal:—

"The squama of the Temporal Bone (Squama, Sheitelbein, Geoffroy) closely resembles that of Mammals in its form and position. It is an elongated, scale-like bone situated upon the lateral wall of the skull above the tympanic cavity, and is posteriorly in contact with the occipital, above with the parietal or temporal, and anteriorly with the great wing of the sphenoid, with which, as we have already seen, it frequently unites to form the posterior orbital process. The external convex surface of the squama for the most part enters into the formation, sometimes more and sometimes less, of the temporal fossa, especially in the long-billed marsh and aquatic birds. From the processus orbitalis posterior a semicircular line runs upon or around the squama, separating it with its striæ or ribs from the planum temporale. Near the anterior border of the squama, and usually below the posterior orbital process, a process shoots forth from its surface, which is the Processus Zygomaticus of Carus and the temporal process of Köstlin. This in some birds, as in the Larks, Parrots, and Fowls, is tolerably well marked, and fuses with the processus orbitalis posterior. In the singing birds, this process is very variable in size; in Thrushes, Sylviadæ, Motacillidæ, and Hirundinidæ, it is rather feebly developed, resembling a small blunt head. In the Fringillidæ it is developed into a slender rod, as it is also in Edolius, and (though to a less degree) in Lanius. In the Paridæ it forms a broad lamina. In the Corvini, it is considerably developed; while in the Falcons it appears to be entirely absent. In Owls it is slender and acicular. In the Woodpeckers it is very large, and occupies a special groove of the Quadrate bone. In the marsh and aquatic birds it approximates very closely to the articular surface of the os quadratum, and is for the most part of very small size." He then proceeds to describe the internal surface of the squama, and its junction with other bones to form the tympanic cavity.

The principal defects of the book are obviously that the author has little acquaintance with the history of the embryological development of the class of birds generally, and does not appear to have studied the serial homologies of the several bones in other classes.

As an instance of the former defect, we may note that Herr Magnus maintains with Nitzsch that the os dentale, or median portion of the lower jaw, is developed from a single point of ossification. "I have never," he says, "been able to discover the presence of two nuclei, even in the youngest animals I have examined, nor any trace of a suture." We would refer to Mr. Parker's paper on the head of the fowl, as clearly showing the double nature of this bone, though no doubt the two parts early coalesce. Taking the ethmoid bone again, we find Dr. Magnus describing it rightly as a cranial bone, or rather as one belonging to his animal sphere. He notices the singular modification in form it undergoes throughout the whole class, and observes that it is a thin bony plate lying between and in front of the globes of the eye. The anterior portion, he goes on to say, situated in front of the eyes, and provided with lateral processes that shut off the nasal from the orbital cavities, may be regarded as the labyrinth of the ethmoid. And then comes the remarkable statement that "the plate extending backwards from this and separating the two orbital plates from each other is the *crista galli*, whilst the short plate extending forwards into the cavum narium represents the lamina perpendicularis." This scarcely appears to us to be an accurate representation. The crista galli is to all intents and purposes an intracranial projection of bone, we might even consider it to be a portion of the falx cerebri which has undergone ossification, and such result must be above the plane of the canals for the nasal branch of the fifth; in point of fact, it may be seen in all birds in a rudimentary form between the two passages for the olfactory nerves. (See note to Mr. Parker's paper "On the Structure of the Skull of the Common Fowl," Phil. Trans. 1869, p. 762.) The septum between the two orbits is chiefly and not to a small extent only, formed by the os perpendiculare or lamina papyracea. Putting aside the references elsewhere made to this point in the course of the descriptions of the bone, we must praise the very full account that is given of its relations in the different classes of birds, proving not only that the means of investigation at Dr. Magnus's disposal are extensive, but that he has made excellent use of them.

As an example of the second point, on which we have ventured to criticise Dr. Magnus's work, we may refer to the entire section on the bone to which he has applied the term Paukenbein, or Tympanic, bone, which, in part at least, corresponds to Mr. Parker's Basi-temporal, and the relations of which the latter writer has worked out so well. Its nature is essentially misunderstood by Dr. Magnus, who appears to have drawn his conclusions from heads examined at too late a period of development, whilst he scarcely makes any reference to its homologies, so important in determining a difficult and disputed relation of this kind. H. P.

## OUR BOOK SHELF

*The Elements of Plane and Solid Geometry.* By H. W. Watson, M.A. (Longmans, Green, and Co.)

THIS is one more Text-book of Geometry. It adopts completely the general principles of the geometrical reformers in England, in the classification of the rems according to their subjects, the free use of super-position, the separation of problems from theorems, the art from the science, and the avowedly arithmetical treatment of proportion. It is distinguished from most that have preceded it by its greater length, especially in its treatment of ratios, by its somewhat wider range of illustration, and its comprehending the elements of solid geometry. But the book is disappointing. A well-trained and well-read mathematician, with plenty of experience in teaching, and we imagine plenty of leisure for writing, ought to turn out a better book. In a text-book which does not profess to be original in its matter, the arrangement and manner are of the first importance; and in both these respects the book in our judgment fails, and fails openly. The large number of miscellaneous propositions with which several of the books open give a real confusion to the whole volume. And it would be easy, if space permitted, to show that the arrangement is unnatural in some important points. Moreover, some of the demonstrations are very inelegant, such as Book I., pp. 11, 17, and Book II., pp. 12, 13; indeed the latter pair are more than inelegant.

On the whole, therefore, we believe that the book before us, though not without merit, is not a very valuable addition to geometrical reform. It seems to show very clearly what the reformers must aim at, and take infinite pains to achieve, the establishment and recognition of a standard syllabus of geometry. When this is agreed upon, we shall see better text-books than have yet been written.

*Victoria.* (1) *Mineral Statistics of Victoria for the year* 1870. Presented to both Houses of Parliament by his Excellency's command. (Melbourne: By authority: John Ferres, Government Printer.)—(2) *Reports of the Mining Surveyors and Registrars.* Quarter ending March 31, 1871. (Melbourne: By authority: John Ferres, Government Printer.)

THESE reports are models of what such statistical reports should be; the tables are methodically arranged, easy of reference, and apparently exhaustive; the printing would be creditable even to a London printer. In the former, besides the interesting summary and the appendices, there are fifty-three admirably constructed tables, setting forth the statistics, from every possible point of view, of the mining operations in all the districts, divisions, and subdivisions of Victoria for the year 1870. Of course the statistics relate mainly to gold, the metal most sought after; but all obtainable information is likewise given with reference to whatever other mineral produces are found in the province—silver, tin, copper, antimony, lead, cobalt, manganese, coal, &c. Every means has been taken to make the statistics reliable, and the result, with regard to gold, is that there has been a falling off of the produce in 1870, as compared with 1869, to the extent of upwards of 40,000 oz., which decrease is largely accounted for by the heavy and unprecedented floods of 1870 interrupting the mining operations, the decrease in the number of mines, and the falling off in the yield of gold from several of the deeper alluvial mines. It is stated that during 1870 several scientific gentlemen volunteered to deliver to the miners gratuitously lectures on subjects connected with mining, but received no encouragement from the district authorities, who seem not to have thought it worth their while to provide a room. The interests of science are, however, by no means neglected. We learn from these reports that during last year more than 800 groups of minerals, rocks, and fossils, were added to the collection of the mining department. Efforts have also been made to obtain specimens of the mineral products of other countries in exchange for native products. Another colony is now likely to reap a rich reward, as already many specimens have been sent both from Europe and America. We are glad to learn that Dr. Von Mueller is preparing a report on the large collection of native fossils which has been made. The second report, for the quarter ending March 31, 1871, is considerably more interesting than the former, in a scientific point of view. Besides full and valuable mining statistics, there are two appendices: (A) "Notes on the Rocks and Minerals of the Owen's District," with a sketch map, by Mr. E. J. Dunn, containing much valuable information on the geology of the district; (B) an interesting paper containing succinct observations on what the author, Ferd. Von Mueller, Director of the Melbourne Botanic Garden, considers a new genus of Fossil Coniferæ, to which he has given the name *Spondylostrobus.* It is allied to *Cupressinites* of Bowerbank. We are sorry we have not space to copy the author's description. The validity of the genus, Mr. Mueller declares, rests chiefly on the extraordinary development of the columella, if so it may be called; this columellar portion forming indeed the main body of the fruit, the so-called new genus differing in this respect from all other cupressineous genera living as well as extinct. The paper is illustrated by a beautifully executed lithograph, containing several coloured figures, natural size, of the fossil, and also by a plan of the field, and sections of the strata in which it is found. We have much pleasure in commending these interesting, and on the whole, encouraging reports, to the notice both of statisticians and geologists.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Pendulum Autographs

HAVING read with much interest Mr. Hubert Airy's communication to NATURE (No. 94), on "Pendulum Autographs," I wish to say a word on the compound pendulum long ago devised, I believe for the first time, by Prof Blackburn, of Glasgow.

I construct the pendulum as follows:—A piece of soft iron wire, about $\frac{1}{50}$th of an inch diameter, is fastened by its ends to two points in the ceiling, and a heavy bob is hung from its middle point. A second wire of the same length is similarly attached to the ceiling and to the bob, so that the wires form two superposed isosceles triangles with the line between the suspension points for their common base. A light deal rod about the same length, more or less, as the distance of the suspension points, has a fine saw cut about ¼th of an inch deep, made lengthwise at both ends. The wires which form the sides of the first triangle are put into one cut, and those of the second triangle into the other; and the rod may then be slid up or down along the wires, to different heights, so that when the pendulum is at rest the rod is horizontal, forming the base of one isosceles triangle, with sides of double wire, and its vertex down at the bob, and also of other two isosceles triangles, equal and similar to one another whose sides are of single wire, and whose vertices are the points of suspension in the ceiling respectively.

It is now evident that the rod is rigorously constrained to oscillate in a plane perpendicular to the line joining the suspension points, while the vertex of the triangle below the rod, which is the point of suspension of the bob, is free to move, at any instant, only in a plane at right angles to the plane of motion of the rod. As the amplitudes of the oscillations are practically made small compared with the lengths of the component pendulums, we thus obtain, with almost any desired degree of exactness, the composition of two simple harmonic motions of different periods of adjustable ratio, and in rectangular directions. It is easy also to see how, by making the wires of unequal length, and dividing them proportionally at the point of suspension of the bob, the simple component motions may be adjusted to different inclinations. In order absolutely to prevent the bob from creating indeterminate motions about its point of suspension, it would be needful to substitute, for the wires below the suspended rod, stiff pieces rigidly attached to the bob. But with due care in swinging the pendulum no very sensible motion of the bob, relatively to its suspending wires, need occur.

To record the motions of the pendulum, I have most frequently adopted the old plan of sand running out at a fine hole at the bottom of the pendulum bob. But for class experiments at the University of St. Andrews, I have also made the following arrangement:—A heavy bob of lead, in metallic connection with its suspending wires, has a metal point projecting from its lower end. Wires from an induction coil are connected, one with either of the suspension points in the ceiling, the other with a sheet of a tin foil which rests on a table, and over which is placed a sheet of paper, all but touching the point projecting from the bob. The pendulum having been first got to swing steadily, the induction coil is put in action, and the sparks, passing from the pendulum point to the tin foil, trace on the paper, if it be suitably prepared, a record of the pendulum motion. I used one of Ruhmkorff's original coils, which, with a single Grove's cell, was quite sufficient. The rheotome acted automatically, and with considerable regularity. The dots on the paper made by the sparks showed distances varying from one element of the pendulum track to another, and thus exhibited in a very interesting manner the variation in the velocity of the pendulum bob.

WILLIAM SWAN

Ardchapel, Dumbartonshire, Aug. 24

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PERMIT me to state that the diagrams in No. 94 of the 17th June to Mr. Hubert Airy's "Pendulum Autographs," are identical with the "Kinematic Curves" by Mr. Perigal, drawn by him upwards of thirty years ago, and discovered by Mr. Sang of Edinburgh two years previously (On the Vibration of an Elastic Spring, Ed. Ph. Tr.), autographic copies being in the possession of the Royal Society, Royal Institution, and Royal Astronomical Society. *Vide* my application of the Binomial Theorem to Perigal's Bicircloids (Lond. Phil. Mag. 1849-1850). Mr. Perigal calls these curves, Lemnoids, Paraboloids, &c.

August 28 S. M. DRACH

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### Thickness of the Earth's Crust

THE question in debate is not a mathematical one. Accepting Archdeacon Pratt's calculations as correct, they would show that certain facts in the earth's motion are what they would be if the earth were a rigid mass, or nearly so. But this at present is not disputed. What is disputed is the soundness of the inference drawn from these facts respecting the fluid or solid state of the earth's interior, for it is contended that in either case the movements in question might be practically the same, provided only they were slow enough. I do not think this is replied to by Archdeacon Pratt in his letter in NATURE, August 31.

Whatever the disturbing forces may be, they amount to a motive impulse given to some portion of the mass of the earth. This impulse may have two effects: either it may alter the shape of the mass by causing part of it to move in some direction faster than the rest can follow, or it may alter the position of the mass by causing the whole to move together. If the portion which receives the impulse is able to move the rest as quickly as it moves itself, the whole will move together; and where there is any cohesion at all, there must be a degree of slowness at which this condition is attained.

Mr. Pratt's rope of sand, if dealt with here, is a system of particles between which there is no cohesion. They are not able, by attractive power, to move each other at all. But if hung out in free space, they would certainly assume a definite shape as a whole, and would retain it with complete "rigidity" in spite of any applied force which was not able to move any of them faster than they could move each other.

Suppose the earth were projected bodily along the line of its axis towards the pole star, what would happen to a loose stone lying on the surface at the south pole? If the earth moved northward ten feet in a second, the stone would, at the end of the first second, be still upon the surface. If the earth moved twenty feet in a second, the stone, at the end of the first second, would be a yard behind it, but before the end of the next second it would be on the surface again. Are not the relations between the rigid, the fluid, and the elastic states all illustrated here? What would be the real cohesive force in a molten earth, as compared with a congealed one, is another matter. "Molten" does not necessarily mean "limp," and the question, if determinable, has not, I imagine, been determined. The molten earth would no doubt be less compressible; and this, in some cases, may be equivalent to an increased cohesion. Let me add that I have no theory as to the earth's interior. A. J. M.

Sept. 5

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### Spectrum of the Aurora

MAY I call your attention to an error which has occurred in the engraving of the Spectrum of the Aurora which I sent you last week. The lines are marked in strength exactly the reverse of what they should be. Thus: No. 1 is the strongest, and is a sharp line easily seen, and in the drawing it is the weakest; and so with the others. No. 1 is the brightest, No. 5 is the faintest.

47, Brook Street LINDSAY

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### Transparent Compass

I BEG leave to draw your attention to a contrivance that I think very suggestive, of improvements in getting up compasses for iron and wooden vessels. This I propose to effect by using glass globes with transparent needle-cards, and thus making a transparent mariner's compass, visible in all directions, that may be either supported or suspended by very simple and compact fittings wherever most convenient.

In iron vessels this transparent compass can be readily placed beyond the local attraction of the iron. In appearance like a pearl, and in good taste.

Please draw attention to this very simple remedy for so many real or alleged complaints of the deviation of the compass on board of iron vessels. GEORGE FAWCUS

North Shields, Sept. 4

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### A Substitute for Euclid

SINCE Prof. Tait has given the weight of his authority to the attack for some time past directed against Euclid, I, and perhaps some others who like me have sons whom they wish to educate as mathematicians, would be much obliged to Mr. Wilson, or any other of your correspondents, who would recommend a book which is suited to lay the foundation of geometry in the future. A FATHER

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### Monolithic Towers of Cement Rubble for Beacons and Lighthouses

IT occurs to me to suggest the trial of common rubble set in Portland or other equally good cement in the construction of beacons and seamarks, as also for lighthouses. The advantages of employing cement rubble, not in prepared blocks but by continuous building, are the following:—

1. The dispensing with all squaring or dressing of materials.
2. The suitableness for such work of any stone of hard quality, thus rendering it unnecessary to bring large materials from a distance, or to open quarries for ashlar.

3. No powerful machinery is needed as for moving or raising heavy materials.

4. A saving in the levelling of the rock for a foundation for the tower.

5. The ease of landing on exposed rocks small fragments of stone, as compared with the landing of heavy and finely-dressed materials.

The erection of an experimental beacon on the plan I have suggested would be attended by comparatively small expense.

Edinburgh, Aug. 29 THOMAS STEVENSON

### Neologisms

IT may be something of a bull, but I wish to consult your correspondents about a neologism which does not exist. It must exist very soon, however, because it is urgently wanted, as will be seen from the following considerations :—

A straight line has a certain *direction ;* we all know what is meant ; it is an inherent property of a straight line. The answer to the question what property two parallel straight lines have (independently of their being produced) is that they have *the same direction.* I do not invite discussion on the propriety of this definition, but only call attention to the fact that the words exist by which this conception of parallels can be expressed.

Now to speak of planes. A plane has a certain —— ; two parallel planes have the same ——. The same *what?*

Again, here are two theorems, which are, in fact, reciprocal :— (1) planes parallel to given straight lines are themselves parallel ; and (2) if two intersecting planes are respectively parallel to two other intersecting planes, the lines of intersection are parallel ; which may be better enunciated as follows :—(1) two directions determine one —— ; and (2) two —— determine one direction.

The German geometers have no difficulty about it. They say, "Durch 2 Richtungen ist eine Stellung, durch 2 Stellungen eine Richtung bestimmt." J. M. WILSON

Rugby, Sept. 1

### The Nucleus of Adipose Tissue

WILL you allow me to make one or two observations upon the remarks made by the writer of the article on the last part of the "Physiological Anatomy and Physiology of Man?" In few words the reviewer has drawn attention to several of the most important points advanced in this part, and for this I feel much indebted to him. But with reference to adipose tissue, he observes that I convey the impression that the adult fat cell "consists merely of an envelope containing oily matter—no mention being made of the fact that by proper treatment a nucleus also can be almost always demonstrated." This is strange, for I believe I was the first to demonstrate the "nucleus" in the fat cells of adult adipose tissue. In my lectures at the Royal College of Physicians in 1861, I showed specimens illustrating the fact, and in the work reviewed I have endeavoured to show that the so-called "nucleus" (germinal matter or bioplasm) is concerned in the formation of the fatty matter, and in its removal from the fully formed fat vesicle (p. 305) whenever we get thin from the absorption of fat. In fig. 198, plate xx., fat vesicles in various stages of development are represented, the "nucleus" being given in every one, while in fig. 132, plate xv., are shown some fat vesicles in cartilage, the nucleus being seen in every instance. If I have not made this point sufficiently clear in my description, it arises from the circumstance that I desired to leave as much as possible of the general description given by my predecessors in the first edition. In the early part of the chapter the nucleus has not been mentioned, which is to be regretted, but in the latter part, containing the new matter, very frequent allusion to it has been made.

It is this *constant presence* of the "nucleus" (bioplasm) in all tissues, at every period of life, at least as long as their activity lasts, to which I have long attached such great importance. I have endeavoured to show that this nucleus matter (bioplasm or germinal matter) is alone instrumental in the formation and removal of all textures, and at every period of life. This alone, of all the matter of the body, is in a living state, and capable of formation. Contrary to the generally received opinion, I have proved its presence even in all forms of yellow elastic tissue, and have shown that this texture is formed upon the same principles as other tissues.

61, Grosvenor Street LIONEL S. BEALE

### Eclipse Photography

FROM letters I have received, it appears that the table of exposures given in my "Notes" is not correctly understood. It is necessary to explain again that the reason why the plate exposed 8 secs. gave a better result than the one exposed 30 secs. was because the eclipsed sun was nearly covered by cloud during the long exposure, and was quite clear during the short. The 30 secs. plate would have been greatly over-exposed for certain details, but the outer corona would probably have been more clearly defined. By giving some plates short and others long exposures, it was intended to show different effects, as would certainly have been the case if we had been favoured with a cloudless sky.

I am informed that it is proposed to attempt to obtain uniformity of results by using the same kinds of instruments and chemicals at all the stations. So far, good. But where is the certainty that the hands that will use the chemicals and instruments will produce equality of results? It is about the same as giving to a dozen men pens, ink, and paper, and expecting from them twelve specimens of caligraphy all alike. It would be preferable to decide beforehand whether negatives, or positives, or both are to be taken, and then to allow the operators to choose their own methods. A. BROTHERS

### The Museums of the Country

IT must be obvious to any scientific person visiting the provincial museums of this country, how inefficient they are for the purpose of preserving Geological and Natural History collections, which are being formed more or less throughout the land.

Whilst the British Association directs so much attention towards the advancement of science by means of investigation, and grants money for the purpose, it is short-sighted on its part to neglect the subject of Local Museums as means for preserving collections for the benefit of science and of posterity.

To give an illustration of the way in which such museums are too often conducted. In a west-country museum there has lately been an addition, consisting of a valuable collection of cave bones, and that is well-preserved and arranged, but why? In a great measure because one of the members of the local society happens to take an interest in that department. But in what condition is the local geological collection? In a state of neglect and disorder, because in that department no one takes an interest. In other museums, where there is nobody to take an interest in the subject, the state of the collections may be imagined.

It is much to be regretted that museums should remain in such a condition. The formation and preservation of local collections ought not to depend upon impulse, or the chance enthusiasm of individuals, but should be the result of a generally recognised business-like system ; and it should be the interest of the various local societies to provide competent curators. It should also be the duty of these societies to preserve for the museum of the district the collections which have been formed, by local geologists or collectors, and not to permit them to be scattered or added to those in the British Museum and to that in Jermyn Street, where they may be said to become buried, and where the geological collections are already of an unmanageable size.

F. G. S.

### Thunderstorms

THE prevalence of thunderstorms accompanied by serious accidents during the last two months has led me and many others to consider whether the phenomena of electrical discharges are thoroughly understood. We have heard of several instances in which the electric fluid "came down the chimney, filled a room with sulphurous vapour," and terrified or injured persons sitting near the fire-places. One fatal accident took place within a few hundred yards of my own house. A gentleman's coachman driving along the turnpike road was instantaneously killed on his box, "the lightning," it is said, "having struck him on the head and passed through his body to the iron work of the carriage, and thence to the ground." From the appearance of the body there is no doubt that the fluid did pass through the poor fellow and caused his death, but my opinion is that he was killed by an ascending current which was attracted to the wheels of the carriage, passed upwards through his body, issued at his head, and shivering his hat (made of felt, and therefore a bad conductor) to fragments, passed to the cloud above. During the same storm I was watching the lightning playing on the hill which is separated by a valley from my house. Every flash I observed was double, composed I imagine of an ascending and descending current. In every instance one of the two flashes was brighter than the other; but I could detect no difference of time; as far as the eye could judge they were simultaneous. The inference I am disposed to draw from these facts is this, that during thunderstorms ascending currents are to be guarded against no less than descending ones; that when chimney-pieces are shivered and people sitting by the fire side are killed, the electric fluid has not come down the chimney at all, but has proceeded from the earth, and, having found a good conductor in the fender and grate, has passed through them harmlessly, and has then overflowed, so to say, into the room, or shattered the non-conducting masonry. Continuous lightning-conductors, on Sir Snow Harris's principle, afford sufficient protection to public buildings, but metal pinnacles terminating below in masonry or woodwork are likely to cause mischief, and iron pillars, unless insulated below by some non-conducting substance, must be equally objectionable.

C. A. JOHNS

## SIR WILLIAM THOMSON ON THE LAW OF BIOGENESIS AND THE LAW OF GRAVITATION

A PASSAGE in the address of the President of the British Association appears to me so remarkable, and so much at variance with the notions entertained by biologists of various shades of opinion, that I am surprised that no observations were made upon it during the sectional meetings, and beg now to draw attention to it. I may mention that in the discussion on spontaneous generation in Section D on the last day of the meeting, I did say substantially what I now write to you, but no one present defended Sir William Thomson's position. The passage in question is as follows: "But science brings a vast mass of inductive evidence against this hypothesis of spontaneous generation, as you have heard from my predecessor in the Presidential chair. Careful enough scrutiny has, in every case up to the present day, discovered life as antecedent to life. Dead matter cannot become living without coming under the influence of matter previously alive. This seems to me *as sure a teaching of science as the law of gravitation*. . . . I confess to being deeply impressed by the evidence put before us by Prof. Huxley, and I am ready to adopt as an article of scientific faith, true through all space and through all time, that life proceeds from life, and from nothing but life." * In the first place it is to be remarked upon this passage, that the reference to his "predecessor in the Presidential chair," and to "the evidence put before us by Prof. Huxley," is made in such a way as would lead an uninformed person to suppose that not only was the speaker simply availing himself of that evidence, but also merely following or re-enunciating a belief previously expressed by Prof. Huxley. This I do not for a moment suppose was in any way the meaning of Sir W. Thomson, who unintentionally has made it appear that Prof. Huxley comes to the same conclusion from the consideration of certain facts, as he does. So far from this re-assuring concord having an existence, I doubt if any single biologist of name (of whatever philosophic tendencies) would venture to assert that it is as sure a teaching of science as the law of gravitation that dead matter cannot become living without coming under the influence of matter previously alive, and conclusions derived from a consideration of a vast series of facts prohibit an evolutionist from accepting such a doctrine without the most complete and widely-reaching evidence in its favour. Sir William Thomson's authority must be accepted as unquestionable as to the amount of sureness which may be attributed to the law of gravitation; but with great deference to him, I should like to ask if he would definitely maintain that it is no greater than that which may be attributed to the dogma "no life except from life." It is the fact that within human observation the law of gravitation is a true statement; it is also the fact that within human observation the dogma "no life except from life" is a true statement; but how can it be for a moment supposed that this places the two statements in the same position of sureness? Does not all depend on that term "within human observation?" Will not the sureness depend on the extent and thoroughness of the observation? And is it not the case that whilst human observation of bodies in relation to the law of gravitation is of the most vast character—embracing not only all varieties of terrestrial matter, but innumerable extra-terrestrial bodies—the human observation of the way in which living matter originates or grows, is a mere trifle so insignificant in extent that it is as a drop in the ocean? Sir William Thomson speaks of being "deeply impressed by the evidence put before us by Prof. Huxley," and is thereupon ready to adopt an article of scientific faith "true through all space and time." What was the evidence in question? The merest fragment, as Prof. Huxley would himself acknowledge (though associated with much more evidence upon allied matters)—simply this by no means astonishing though much controverted fact, that when out of the unspeakably many kinds of mineral matter which you might take, you take one or two and boil them and seal them up and submit them to a variety of processes, the object of which is not to produce favourable conditions for the evolution of life, but to prevent the access of already living matter—you don't get life produced. The whole of this kind of experiment, and of the evidence which so much impressed Sir William Thomson, cannot—attended as it is with negative results—have anything to do with the general question of the *de novo* origin of living matter. Such evidence merely relates to a particular supposed case of such origin, one out of thousands conceivable. Yet this is what it seems to me—I write with diffidence—Sir William Thomson has taken as evidence of the same value as that on which rests the law of gravitation. Because it seems rather more probable than not that organisms do not arise *de novo* in boiled and sealed solutions of tartrate of ammonia, in hay-decoctions and turnip-juice, *therefore* it is true through all space and all time that dead matter never becomes living without the action of living matter; therefore nowhere to-day on the whole earth—in the sea charged with gases, open to sunlight and atmosphere, holding salts and complex semi-organic compounds, suspended and in solution—is this process going on; in no pond; under no moss; and not only to-day, but we are to conclude that never at any time did Nature in her great laboratory produce life from mineral matter, because in certain arbitrary, crude, and utterly artificial conditions of isolation she refuses to do so. Is it true that the law of gravitation is no surer a teaching of science than the dogma about the origin of life which rests on such logic?

That I have not misrepresented the utter poverty of observation upon the origin of life will, I believe, be admitted by all naturalists—possibly individuals unacquainted with biological phenomena may have conceived it to have been relatively more extensive. We have been able to trace the commencement of so many of the various living forms to eggs, that it becomes waste of time to examine into cases of alleged spontaneous origin of *complex* forms from mineral matter; and biologists have now to look for the formation of simple organic material. Observations therefore which merely tend to disprove the spontaneous productions of maggots, worms, ciliated infusoria, and fungi, are not to be reckoned as "evidence on the origin of life," they do not bear on the question as it now presents itself, the working hypothesis of science being, not that *animals* or *plants* originate *de novo*, but that *organic matter* has at one time done so, and is doing so still. It is, I believe, just to assert that observation bearing on this hypothesis is almost entirely wanting, and

* NATURE, Vol. iv., p. 269.

indeed, the few experiments of the French observers, and of Gilbert Child and Bastian in this country are the only ones at present made.

The reason is obvious, the conditions of the experiment and observation required are so difficult that we have not yet mastered them. They are first, to ensure all the favouring circumstances in the laboratory experiment which natural stations afford, and, of course, to ensure them it is necessary to know or have some idea (which biologists have not) as to what they may be; second, to exclude simultaneously all living matter; third, to make the observations *throughout* with the greatest minuteness which the microscope permits—a necessary condition, on account of the possible smallness of particles of living matter. When we have had experiments performed in this way with a vast variety in the first-named set of conditions, so as to obtain and study the action of various natural circumstances which might possibly be present in the *de novo* origination of living from mineral matter—then we may speak of evidence on the question. As it is, we have but a very incomplete and discordant series of observations on one class of conditions in which there is a presumption of spontaneous generation (the case of Bacterium), and which have been selected for experiments on account of a supposed facility for isolation, without interference with the conditions, but of which very little is understood at all. I venture to submit that this single case, in which there has been some little investigation with, be it granted, negative result, so far from warranting the enunciation of a dogma, which is declared to be as sure as a great law expressing the concurrence of almost infinitely numerous, varied, and reiterated observations, does not even justify an opinion; it has no possible bearing upon the source of the minute protoplasmic particles which the microscopist finds abundantly in sea-water, nor upon the origin of the atmospheric germs which are so largely invoked by some persons. It leaves us necessarily to *a priori* considerations in regard to the origin of life on the earth, and until direct researches are made, the hypothesis developed by *a priori* argument must have far more claim on the adhesion of an unbiassed mind, than a pseudo-law, though the latter bear an authority so great in some departments of science as is that of Sir William Thomson.

E. RAY LANKESTER

## RECENT FRENCH ZOOLOGICAL DISCOVERIES

TWO naturalists, who have been more than usually successful in their investigations of the faunas of distant and little-known countries, have recently returned to France, and are now engaged in working out the results of their arduous expeditions. These are M. le Père Armand David, and M. Alfred Grandidier.

M. le Père Armand David is a missionary priest of the Order of Lazarists, who was for many years resident at Pekin. Here he devoted much time and attention to the fauna of the surrounding country, which was at that period little known, and entering into communication with the authorities of the Jardin des Plantes of Paris, supplied that establishment with many interesting novelties. Amongst these one of the most remarkable was a new deer with very peculiar horns and a long tail, which was named by M. Alphonse Milne-Edwards *Elaphurus davidianus*, after its indefatigable discoverer. But about two years ago Father David moved the seat of his investigations into still more promising quarters. It was, we believe, the magnificent new species of Pheasants transmitted by Bishop Chauveau from Ta-tsien-leou—a town in Western Szechuen upon the frontiers of Tibet—that first called his attention to the probable richness of this district in other departments of zoology. Nor have his expectations been in any way disappointed. The collections of Mammals, Birds, and Reptiles, obtained by Father David during the recent exploration of *Mopin*, as this portion of the Celestial Empire is termed by the French writers, have of late years seldom been equalled in any part of the world for extent or variety. The fauna of these mountains seems to be a sort of *pendant* to that of the Himalayas, which, some years ago, was so successfully investigated by Mr. Hodgson. The singular *Ælurus* or Wah, of Nepal, is replaced by a larger and even stranger form, the *Ælurapus* of M. Milne-Edwards, a large bear-like mammal, quite distinct from anything previously known. A long-haired monkey inhabits the pine forests, remarkable for the development of its nose, which the same naturalist has proposed to name *Rhinopithecus*. The *Takin* of the Mishnees of Upper Assam (*Budorcas taxicolor*) is represented by a second species of this most singular genus of Ruminants. A new form of Cervidæ is remarkable for its small horns and well-developed canines; and there are a host of interesting novelties belonging to the insectivorous and rodent orders in Père David's series. In birds, M. Jules Verreaux, to whom the working out of this part of M. David's collections has been assigned, has already discriminated upwards of thirty new species. Amongst these many belong to the remarkable genera discovered by Mr. Hodgson in the hill-forests of Nepaul, and hitherto unknown to occur elsewhere. Perhaps the most noteworthy of them is a small Passerine form allied to *Paradoxornis*, which has only three toes, a phenomenon hitherto unknown in that typical order of birds. The Reptiles and Batrachians obtained by Father David in Moupin are also said to contain many novelties. Since the lamented death of Prof. Duméril, their investigation has, we believe, been undertaken by Prof. Blanchard, who has within these few last days brought before the French Academy a notice of one of the most extraordinary animals of the latter group. This is no other than a gigantic aquatic Salamander allied to, but distinct from, the now well-known *Sieboldia maxima*, of Japan. The discovery of this form of life in continental Asia is a fact of the highest significance as regards geographical distribution, as it was previously believed to be in the present epoch confined to the Japanese Islands, though remains of a closely allied animal (*Andrias scheuchzeri*) are found in the tertiary freshwater deposits of Central Europe.

We have mentioned only a few of the principal discoveries of M. David, but enough has been said to show the importance of the additions he has made to zoological science, and to heighten the interest attaching to the complete investigation of the fauna of the Chinese frontier of Tibet, which this distinguished naturalist has thus inaugurated.

While Father David has been labouring among the snows of Central Asia, another not less arduous devotee of science has been risking his life in the tropical forests of Madagascar, and has likewise made many brilliant discoveries. M. Alfred Grandidier, who has now returned from, we believe, his *third* voyage of discovery in that strange island, has shown that the riches and eccentricities of its fauna have not yet been exhausted. His collections, which have only reached the Jardin des Plantes very recently, although brought to France before the political storm of last autumn commenced, have not yet been fully examined. But they are said to contain very full series of several species of Lemuridæ, the comparison of which is likely to lead to important results, besides examples of a new genus of Rodentia, and many other Mammals of high interest. M. Grandidier has also paid much attention to the fossil deposits of Southern Madagascar, which contain the remains of the extinct gigantic bird, *Æpyornis maxima*, and has arrived at some important results (such as the former presence of *Hippopotamus* in Madagascar) which may ultimately tend to modify some of the views generally held concerning the true nature of the fauna of this island and its origin.

P. L. S.

## *PENDULUM AUTOGRAPHS*

### II.

IT naturally occurred to try what would be the effect of introducing a third joint in the suspension of the pendulum, and altering the angles between the different joints. First I tried three joints at angles of 60°, by means of two intermediate pieces attached to the rod, to the cross-bar, and to one another, by string-hinges. The result did not differ from that obtained with two joints. This surprised me, for I had expected that each joint in taking its share of the swing would somehow assert its claim to its own proper period distinct from the periods of the other two. But on close examination, attaching long slender splints of wood to the cross-bar, the two intermediaries, and the rod, and bringing their depending ends near together, by

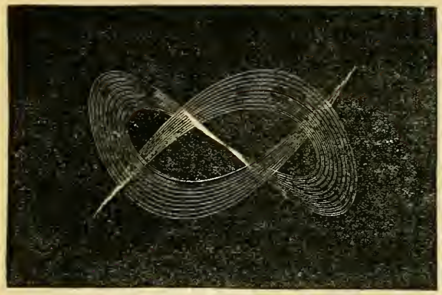

Fig. 7.—Proportion 3 : 5.—Cusped type.

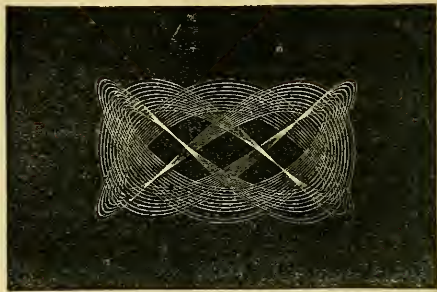

Fig 8.—Proportion 3 : 5.—Looped type.

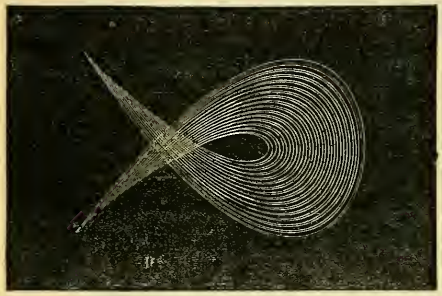

Fig. 9.—Proportion 2 : 3.—Cusped type.

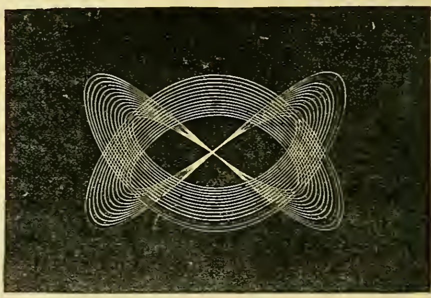

Fig. 10.—Proportion 2 : 3.—Looped type.

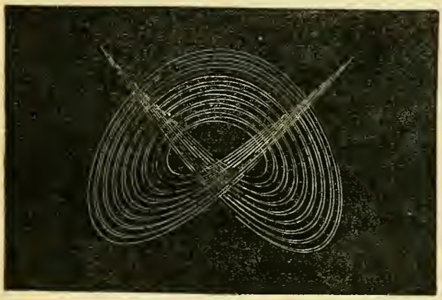

Fig. 11.—Proportion 3 : 4.—Cusped type.

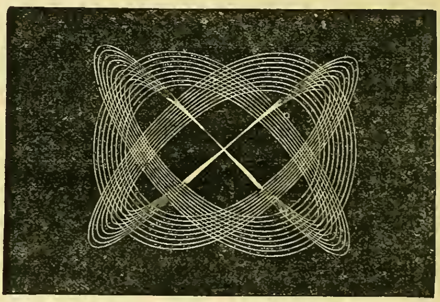

Fig. 12.—Proportion 3 : 4.—Looped type.

pairs, for comparison of relative motions, I found that the two upper joints alone were able to make good their title to independent periods of swing: the third joint acted merely as subsidiary to the second, merely serving to facilitate the bend at the second joint, and always synchronous with that bend, never able to establish a proper period of its own. All three joints conspired to allow undeviating vibration in a plane at right angles to that allowed by the topmost joint alone, just as though there had been only two joints at right angles. I also tried three joints at angles of 90° and 30°, and two joints at an angle of 60°. They all gave similar results. It is clear that the planes of slow and quick vibration are determined solely by the position of the topmost joint, the former being that in which the pendulum can swing by the topmost joint alone, and the latter that in which the topmost joint takes no share at all, or the least possible share, at right angles to the first. However the joints may be arranged, they conspire to establish a virtual second joint, at right angles to the topmost joint, and at some point below it not necessarily coinciding with the position of any one of the actual joints, and apparently varying for different positions of the plane of vibration.

At this stage I showed some of my pendulum's tracery to my brother-in-law, Mr. E. J. Routh, M.A., of St. Peter's College, Cambridge, and received some hints from him which led me to adopt an arrangement whereby the position of the lower joint could be varied, so as to bring the two periods of vibration into any simple proportion, as 1 : 2, 2 : 3, &c. This was done by a very simple mode of suspension,—two cords hanging from two fixed points some distance apart, and passing through a small ring that could slide up and down the concurrent cords and be fixed at any height. In this arrangement I found the germs of an infinite variety of curious and elegant curves, that gave a new lease of life to my course of experiment. Before attempting anything, I drew in my pocket-book the skeletons of the curves that would be described corresponding to the proportions, 1 : 2, 2 : 3, 3 : 4, 4 : 5, 5 : 6, 6 : 7, 7 : 8, 8 : 9, in the periods of quick and slow vibration. Then I hung a bullet by two threads from two points about ten inches apart on the circumference of a child's wooden hoop, fixing the hoop upright with the bullet swinging inside the circle. Another bit of thread furnished the sliding-ring to nip the two convergent threads at any desired point, and a few trials enabled me to fix it pretty accurately for the proportion 1 : 2, so that the bullet would swing in the plane of the hoop in half the time it took to swing at right angles to the plane of the hoop, making two vibrations in the plane of the hoop to one vibration athwart. (The length of the pendulum varies as the square of the period of vibration, so the slider was at a point one quarter of the distance from the bullet to the horizontal line between the two points of suspension.) It was with great interest that I watched the motion of my pensile bullet, and greeted the verification of my pocket-book sketch. Pull the bullet aside and start it obliquely, and it describes a crescent-moon, the two horns being formed by the double swing in the plane of the hoop, while the length of the crescent measured from tip to tip is given by a single swing athwart the plane of the hoop. Start the bullet from rest at the centre by a sudden blow obliquely given, and it describes a figure of eight with its length athwart the plane of the hoop. (See Figs. 5 and 6.) Other proportions gave still more curious results in accordance with my skeleton sketches, and made me impatient to try them on a larger scale. The lead at my command I packed into a long-shaped zinc box, with a tubular orifice at the top, which in my service became the bottom. To the lead I added a stone jar containing some 10 lbs. of mercury, and made everything secure with cordage. The dependent tubular orifice seemed made on purpose to accommodate a few inches of wooden roller which carried the glass pen, and a diagonal beam in the ceiling of my bedroom offered a capital fixture for two rings about four feet apart, giving suspension to two iron chains by which my incongruous pendulum-bob was doomed to swing. To nip the chains together at the requisite height, I used a loose link which hooked into the corresponding links in the two convergents, and made a very rude and coarse adjustment, which left all accuracy to chance. It chanced, however, that the adjustment for the proportion 2 : 3 was beautifully accurate, and I shall never forget the feeling of delight which I experienced while watching the marvellous fidelity with which the pen-point traced the curve appropriate to that proportion. The pendulum was drawn aside and started obliquely on one side of the plane of slow vibration, and having to make three vibrations across that plane to two vibrations to and fro, it compounds these into a curve like a capital Q with two tails, one on each side, looking like a swallow-tailed balloon. (See Fig. 9.) At the end of the second to-and-fro vibration the pen returns to the point whence it started, except that friction compels it to fall short little by little at every stroke; but if the adjustment is accurate, as it was in this particular case, the shape of the curve remains the same from first to last, and the figure is filled up to the very centre by the orderly description of curve within curve conspiring to produce a web of lines of astonishing regularity. If the adjustment of the connecting link is very slightly inaccurate, the curve begins to change its shape little by little at every stroke, in one way or another, according as the link is too high or too low; and wonderfully intricate is the result, for after a due series of intermediate stages, the original figure reappears, but reversed; and after another series of changes it presents itself again to view in its original posture, but much diminished by the friction that has been in operation throughout all these changeful phases. It may be imagined how intricate the web becomes, though the limits of illustration do not allow me to give a specimen here. It was easy to eliminate all the transition-curves from the tracery, by depressing the paper for the proper interval, and allowing it to return to contact with the pen only at those distinctive phases when the original figure was reproduced either erect or reversed. I obtained a very curious specimen by applying this selective method to the case of the proportion 1 : 2, allowing the pen to mark only the crescents and the figures of 8 in alternate series, converging orderly to the centre.

For the suspension of the paper, I fixed four pairs of upright rods at the four corners of a shallow tray, which could be slipped under the pendulum, and each rod gave support to an india-rubber band, which, with its fellow at right angles, was attached by a small hook to the corresponding angle of the paper. Each band could be slid up or down its rod, to allow of nice adjustment of the level of the paper, and the whole tray could be raised on a footstool or chair, to suit the elevation of the pendulum when the slider was run aloft in attaining the proportions nearer unity, such as 7 : 8, or 8 : 9.

The iron chain was soon exchanged for strong cords, passing through a narrow wire ring, which could be arrested at any point by a needle driven through both cords below the ring. This was a small improvement, allowing more accuracy in the adjustment of the slider, and therefore more accuracy in the proportion between the two periods of quick and slow vibration. But it was still very far from satisfactory. Meanwhile I had ordered a cylinder of lead, weighing half a cwt, to be cast, with a hole through the axis; for my zinc box full of "notions" was so tall that I could not bring the slider near enough to the centre of gravity to obtain any proportion lower than 1 : 3, and that only with great trouble. When the cylinder of lead appeared, I sawed it into two unequal portions, so that I could use either or both; and instead of simple cords, which twisted in a most troublesome manner below the sliding ring I introduced a stiff rod of fir to carry the lead by a cross pin, and I used two pairs of cords passing through holes in a slider on either side of the central rod. This slider was a small block of wood pierced to fit the rod, and provided with a lateral screw to fix it at any required height. This arrangement ensured admirable steadiness and freedom from torsion, and a great many sheets were filled with the improved performances of the machine; but there still remained an important defect to remedy. The coarse cords, at the point where they entered the holes in the slider, made a very rough hinge for the cross vibration to rely upon, and it was manifest in the tell-tale records of the curves described that considerable change of period took place between the beginning and the end of the web; and the change was always such as to increase the disparity between the two periods, which could only mean that the level of the centre of quick vibration in the cords immediately above the slider was lowered when the range of oscillation diminished. It was easy to see that a large oscillation would strain the cords to a greater height above the slider than would be called for in a smaller oscillation. The truth of this surmise was proved by the success of the remedy applied. Instead of cords I used two pairs of broad tapes, and instead of a solid slider I made one in two halves, embracing the rod in the centre and nipping the concurrent tapes on either side between their opposed faces, being clamped together by thumb-screws beyond the tapes on either side. Here the slider had no firm hold on the rod beyond the accuracy of its fit, which served to prevent torsion, but had firm hold on the pairs of tapes, pinching them with especial accuracy at the upper edge of the slit in which they lay between the two halves, and reducing the hinge there to a narrow line no thicker than the pairs of tapes instead of the gross thickness of the cords which they superseded. The improvement of the pendulum's performance on paper was very striking. When well adjusted, it was scarcely possible from beginning to end to detect any change in the shape of the figure described; *scarcely* possible, I say, for even now our hinge is not a mathematical line, and we do not obtain perfect mathematical accuracy in our results. Further improvement might be obtained by refinement in tapes and slider, or by increasing the total height of the pendulum, or by substituting some other form of hinge; but the form which I have described is so simple, and its performance so good, that I am content to accept its one very small fault for the sake of its many excellent qualities.

Figures 1-12 are the produce of this pendulum thus improved. They are only a few of the most interesting out of an endless variety of interesting curves, and are chosen as characteristic specimens of a series too extensive to be fairly represented except by a much larger number of illustrations. Figures 1 and 2 represent the proportion 1 : 3, the lowest that is easily attainable without a loftier pendulum; and the following pairs of figures show successively the proportions 2 : 5, 1 : 2, 3 : 5, 2 : 3, 3 : 4. Each of these is illustrated by two figures exhibiting the two chief types of the curve proper to that proportion. They may be termed the cusped type and the looped type. It will be seen that the two cusps in the first figure of each pair are opened into loops in the second, and that each loop in the first is doubled in the second. Between these two typical forms we have an infinite series of intermediate forms possessing features of great interest, those nearest the cusped type especially being characterised by a peculiar "watered" appearance, due to the intersection of two sets of lines very slightly inclined to one another. This is seen, for example, in Fig. 3, which errs a little from the perfect type.

Accuracy of proportion between the two periods of vibration could only be arrived at by repeated trials. The sliding-clamp sufficed for coarse adjustment, but for fine adjustment it was found necessary to attach a subsidiary weight below the large one in some way admitting of considerable range of position, so as to alter minutely the position of the centre of gravity. A heavy iron nut travelling easily on a screw-thread cut on the depending shaft that carried the pen supplied this want, and greatly facilitated the attainment of the utmost accuracy at command.

With a pendulum only seven or eight feet high, there is great difficulty in obtaining the curves that correspond to any proportions lower than 1 : 3, because the slider cannot be brought within a certain distance of the centre of gravity, which lies somewhere in the middle of the lead. To obtain the proportion 1 : 3, that is, to make the pendulum swing three times across for every one swing to and fro, we must lower the slider within a foot of the centre of gravity (the length of the pendulum varying as the square of the period of oscillation), and to obtain the proportion 1 : 4, the distance between the slider and the centre of gravity must be 1-16th of the height of the pendulum, or only six inches in the present instance; but three or four of those six inches are taken up with the thickness of the lead and the attachments of the tapes, and the rest with the depth of the slider, and so the curve cannot be obtained without a more lofty suspension for the pendulum. This greater elevation I found in the great octagonal room which Sir Christopher Wren built as the chief room of the Royal Observatory in Greenwich Park. By means of two hooks fixed above opposite windows in this room, from which my tapes converged to the middle, I got a height of eighteen feet, and was able to reach such proportions as 1 : 4, 1 : 5, 1 : 6. At this extreme it was really amusing to watch the busy haste of the manifold cross-vibration over-riding the staid gravity that marked the slower oscillation to and fro. To obtain proportions lower yet than these, I should want a great increase in height of suspension; but there is no great inducement to attempt this, as the nature of the curve may be foreseen at a glance, and is marked by extreme simplicity—merely a zigzag or a string of beads.

Some of these experiments with lofty suspension were made on stormy days; and while watching the travels of the delicate pen-point, I could see that their regularity was slightly disturbed by every gust of unusual violence that beat against the high walls.

But this article would never end if I allowed myself to dwell on all the points that called for attention in the course of experiment which I have been describing. I fear I have exceeded due limits already, and feel that I owe an apology to the reader for so large a trespass on his patience. My apology must be the elegance and exquisite symmetry of these natural curves in their admirable obedience to a purely natural law, and the great pleasure I have enjoyed—the sense of high privilege I have felt—in their investigation. I understand that these curves, or some of them, have been demonstrated before, by means of a stream of sand flowing from a hole in the base of a vessel that was used as the weight of the pendulum, and I believe that steel springs of elliptic or oblong cross-section have been made to trace such curves as that which first attracted my attention in the vibration of my slender acacia-twig; but I am not aware that any specimens of the series have ever before been exhibited in a form that rendered them accessible to the public eye.

HUBERT AIRY

## *SOME SPECULATIONS ON THE AURORA*

IN preparing a lecture on the Aurora Borealis some months ago, I was led to some speculations which may or may not be new, and may or may not be of some value. I will submit them to the readers of NATURE.

I assume of course that the auroral rays extend to great heights above the surface of the earth, that they are sensibly parallel, and that their apparent point of convergence is, generally speaking, that to which the freely-suspended magnet points. In the great aurora of October 24, 1870, this point was close to $\eta$ Pegasi at 8.30 P.M., coinciding very well with the direction of the magnet.* Remembering that this aurora was witnessed over a large part of the northern hemisphere, and that there was a contemporaneous aurora in the southern hemisphere, and, assuming that at each place the direction of the auroral streamers is approximately parallel to the magnet, we must conceive the earth, during such an auroral display, as a globe with streamers of light radiating and diverging from its polar regions, and spreading far out into space. The general direction of these streamers at different spots on the earth will be got by placing a magnet below a sheet of paper and getting the magnetic curves with iron filings,

* To-night it is a few degrees below $\alpha$ Cygni (but not clearly defined) at 11 P.M.

and then describing a circle, to represent a section through the axis of the earth so that the magnet shall occupy the central part, about two-thirds of its diameter. The portion of the magnetic curves outside the circle will cut the circle at different angles, and fairly represent the directions of the auroral streamers.

Now, Arago, in his catalogue of auroras, shows that during the months of September, October, March, and April we are especially favoured with auroras; and that in these months they are both brighter and more frequent than at other times. This periodicity indicates an extra terrestrial origin for auroras. Does it not show that during those months we pass through an auroral region, just as in November and August we pass through meteoric regions, or, in other words, that we intersect a ring of some substance capable of being electrified by the earth in its passage, when there is any change in its magnetic power, and so rendered luminous? But it is impossible not to conjecture that this ring or disc is the very disc which is visible to us as the zodiacal light; for besides the fact of zodiacal light being specially visible during the same months, there is the positive evidence of spectrum analysis to the identity of the substances luminous in the aurora and the zodiacal light. We are led then to the hypothesis that there exists round the sun, and extending as far as our earth, an atmosphere, consisting of an unknown element, a gas of extreme lightness, and that this atmosphere is especially condensed in the form of a disc extending round the sun, but probably not concentric with it. The same element appears to exist in the solar corona, and was also detected in the vague phosphorescent luminosities of the sky on a particular evening, by, I think, Angström.

I wish to suggest, therefore, that catalogues of auroras may, like catalogues of meteors, determine auroral regions in the earth's orbit, and that two such regions are, in fact, already shown by Arago's catalogue, and that this periodicity, as well as the results of spectrum analysis, indicate a cosmical origin for auroras.

There is one more point which may be interesting. The luminous streamers have a lateral motion; they shift sideways, and in fact rotate round their pole. Is this motion of rotation always, or even generally, in the same direction? I have not observed it often enough to speak with confidence. But if so, it must have some definite cause, and will be analogous to that of rotation in a definite direction of an electrical current round the pole of a magnet. The earth must be looked upon as a delicate solar electroscope and magnetometer, and the electrical discharge round the earth is stratified, and is in lines and strata that have, perhaps, motions in definite directions.

It may be worth remarking that the 22nd, 23rd, 24th, and 25th of October are the most famous days in the year for auroras, at least in the present century, and that the greatest displays of all on those days have happened at intervals of multiples of eleven years. Last year we had splendid auroras on the 24th and 25th; there is, therefore, some ground for expecting fine auroras on the same nights this year, if the auroral cycle corresponds to the sunspot and magnetic cycles.

J. M. WILSON

## NOTES

WE learn from Indianapolis journals, received at the moment of going to press, that the American Association for the Advancement of Science commenced its sittings on the evening of August 21 by an opening address from the retiring president, Prof. T. Sterry Hunt, on the Iron Interests of Indiana, in which he completely sustained every claim that had been made for the State, showing conclusively that it has the elements within its borders from which to secure a manufacturing future that shall make Indiana the mediterranean workshop for the whole country. The sections commenced their sittings on the following day, and San Francisco was fixed on as the next place of meeting. An extra double number of the *American Naturalist* for September 15 will give a full report of both the opening address and the sectional proceedings. In a future number we shall give an epitome of all matters of interest discussed at the meeting.

M. JANSSEN has been commissioned by the French Government to proceed to the East to observe the total Solar Eclipse of December next. He has, therefore, been compelled to decline the offer made to him by the British Association to take part in the British Expedition.

THE President of the Royal Society has received a telegram from the Government Astronomer, Melbourne, that the Eclipse Expedition will leave that port on November 20.

WE regret that owing to the omission of a sentence, the note respecting the distinguished visitors at Section A of the late meeting of the British Association, read incorrectly in a small proportion of the edition of our last number. We now supply the omission by giving the following probably unexampled list of Senior and Second Wranglers and Smiths' Prizemen who were present:—Adams, Cayley, Challis, Stokes, Hon. J. W. Strutt, Hopkinson, Kelland, Tait, Wilson, Thomson, Maxwell, Sylvester, Clifford, Jack, J. W. L. Glaisher; of these the first nine were Senior Wranglers.

WE learn from the *British Medical Journal* that in accordance with the will of the late Dr. Lacaze a prize of 10,000fr. is to be awarded by the Faculty of Medicine of Paris every second year to the best work on phthisis and on typhoid fever alternately. The first prize will be awarded at the end of the academical year 1871-2, for the best work on phthisis. Essays (with a distinguishing motto and the author's name in a sealed envelope) must be sent in before July 1, 1872. The prize is open to foreigners.

IN a paper read before the Natural History Society of Boston (U.S.), Mr. W. T. Brigham gives an account of several remarkable earthquakes that have occurred in New England, with a list of all such phenomena that have occurred in that region from 1638 to 1870. Some of these disturbances appear to have been violent and protracted.

WE understand from the *Geological Magazine* that there will shortly be published a Geological Atlas of England, by Mr. W. Stephen Mitchell. The Atlas will contain the following Maps:—1. Cambrian (of Survey); Lower Cambrian (of Sedgwick). 2. Lower Silurian (of Survey); Middle and Upper Cambrian (of Sedgwick). 3. Upper Silurian (of Survey); Silurian (of Sedgwick). 4. Old Red Sandstone; Devonian. 5. Carboniferous Limestone; Yoredale Beds. 6. Millstone Grit; Coal Measures. 7. Permian (of Survey); Pontefract Group (of Sedgwick). 8. New Red Sandstone; Rhætic (Penarth). 9. Lias. 10. Lower Oolite. 11. Middle Oolite. 12. Upper Oolite. 13. Wealden; Neocomian. 14. Gault; Upper Greensand; Chalk and Chalk Marl. 15. Eocene. 16. Crag. 17. Alluvium. 18. Bone Caves. 19. Metamorphic (?) 20. Igneous. The Maps will be printed in colours, each Map exhibiting only the range of one formation, and the names of places on the formation. In some few cases where it is requisite, as a clue to the locality, to introduce the names of places near, but not on, the formation, these will be printed in a different type. The Maps (11¾in. by 9¼in.) are based on a photographic reduction of the last edition of the Greenough Map, which is published under the direction of a committee appointed by the Geological Society. In all cases where, through researches more recent than this last edition, any changes have been adopted in the grouping of the beds, this atlas conforms with the latest alterations. The revision of the proofs of particular maps has been kindly promised by Mr. W. Boyd Dawkins, F.R.S., Mr. W. Whitaker, Mr. H. Bauerman, Mr. J. W. Judd, Mr. Charles Moore, Mr. W. T. Aveline, and others. Letter-press will accompany each map, giving in a tabulated form the subdivisions of the formations, the origin of the names of the groups of beds, their lithological characters, thickness, range, &c., with a historical notice of the various classifications that have been at different times employed. The lists of fossils will be arranged on a new plan, showing in a tabulated form for each formation the genera that first appear, those that last appear, and those that are numerically abundant in that formation. Separate tables give the characteristic species. These lists are prepared expressly for this work by Mr. R. Etheridge, F.R.S., &c., Palæontologist to Her Majesty's Geological Survey of Great Britain.

THE continental scientific journals record the death of Dr. Milde, a well-known botanist, whose contributions to systematic cryptogamic botany are especially valuable.

WE have to notice the death, at a very advanced age, of James De Carle Sowerby, the first secretary of the Royal Botanic Gardens, Regent's Park, an office which he held till last year, when he resigned it in favour of his son. Mr. Sowerby belonged to a family, many members of which have distinguished themselves by their devotion to various branches of science, and to the pictorial illustration of natural objects.

THE Essex Institute publishes an obituary notice of its late president, Mr. Francis Peabody, of Salem, who died October 31, 1867, and who was noted for his researches in mechanical physics.

THE trustees of the Manchester Grammar School are so satisfied with the excellent work done in the Physical Science Department, under the superintendence of Dr. W. M. Watts, that they have begun to fit up a second and larger laboratory, at the cost of from 700*l*. or 800*l*. It is only three or four years since this department of the school was opened, and already many valuable scholarships and other honours have been gained by the boys.

THE following eminent archæologists are announced as contributing papers for the next session of the Society of Biblical Archæology:—M. Heinrich Brugsch, F. C. Chabas, Clermont Ganneau, and the Chevalier de Sauley. The first part of the society's transactions will be ready early in the spring, and will contain articles by Dr. Birch, J. W. Bosanquet, M. Ganneau, Prof. Lowne, Lieut. Prideaux, G. Smith, and H. Fox Talbot.

THE Society of Arts have consented to give their co-operation to the Polytechnic Exhibition, to be held at Moscow next year, in celebration of the two hundredth anniversary of the birth of Czar Peter the Great.

ACCORDING to recently-published statistics of the University of Edinburgh Botanical Class, in the session of 1871 the number of pupils was 306. Of these, 241 (including 5 ladies) were medical students, 12 pharmaceutical students, and 53 general students.

THE Archæological Society, whose gathering at Weymouth we recorded last week, devoted Wednesday to an examination of objects of antiquarian interest in that town, including the Corporation regalia and muniments. On Thursday papers were read as follows:—By Mr. H. S. Cuming, F.S.A., "On the Patron Saint of Dorset, St. Edward, King and Martyr." By Mr. J. Drew, F.R.A.S., F.G.S., "On Art Treasures and their preservation." By Mr. G. Eliot, "On the Antiquities of Portland." There was afterwards an excursion to Corfe and Dorchester, visiting several objects of interest on the way. The papers read on Saturday and Friday evenings were as follow:—Mr. J. R. Planché, Somerset Herald, "On the Family of Robert Fitzgerald, the Domesday Tenant of Corfe." Mr. Edward Levien, M.A., F.S.A., Hon. Sec., "On Wareham and its Religious Houses." Mr. W. H. Black, F.S.A., "On Wareham and the Earliest Historic Monuments in Dorset." Rev. William Barnes, B.D., "On the origin of the name and people of Dorset." Mr. Joseph Stevens, M.D., "On newly-discovered Roman and Saxon remains at Finkley near Andover." The meeting was brought to a close on Saturday evening. Saturday's excursion was first by rail to Bindon Abbey, thence to Wareham, and afterwards by rail to Corfe Castle. The concluding meeting was held at the Royal Hotel on the return of the excursionists to Weymouth, at 8.30, when, after the reading of some papers, the usual formal resolutions and votes of thanks to the gentlemen who had assisted the Association in conducting the proceedings were passed and the congress was brought to a close.

THE Annual Meeting of the Devonshire Association for the Promotion of Literature, Science, and Art, recently held its sittings at the picturesque little town of Bideford, occupying three days, the retiring President, Mr. J. A. Froude, resigning the chair to the Rev. Canon Kingsley, who gave an eloquent and interesting address. Papers were read, mostly of an archæological and geographical character, by Mr. Pengelly, Mr. Spence Bate, and other distinguished Devonians.

IT is stated that Prof. Watson, of the University of Michigan, has discovered a new planet in the constellation Capricorn, of the tenth magnitude. This is the 115th of the series.

MR. J. R. HIND, F.R.S., has calculated the Ephemeris for Greenwich mean time of Tuttle's Comet, which will be visible during this and next month. According to Prof. Luther, its next perihelion passage will occur about the 30th of November. The following are Mr. Hind's figures:—

| 1871 | Right Ascension. | Declination. |
|---|---|---|
| Sept. 1 | 100° 13·2′ | 62° 22·7′ |
| „ 7 | 106° 36·6′ | 60° 55·6′ |
| „ 15 | 115° 12·8′ | 58° 20·7′ |
| „ 23 | 122° 38·8′ | 54° 59·8′ |
| Oct. 1 | 129° 15·7′ | 50° 49·5′ |
| „ 7 | 133° 44·3′ | 47° 2·6′ |
| „ 13 | 137° 51·5′ | 42° 35·5′ |

PROF. A. HALL sends us some careful Equatorial Observations made at the U.S. Naval Observatory, Washington, and Supplementary Notes on the observations for magnetism and position made in the U.S. Naval Observatory expedition to Siberia to observe the Solar Eclipse of August 7, 1869.

A RECENT number of the "Astronomische Nachrichten" contains an elaborate paper by Prof. E. Schönfeld, "On the Change of Light of Variable Stars."

THE *Journal of the Society of Arts* states that a memorial monument has been erected in New South Wales to the memory of Captain Cook, at the supposed place at which he landed from the *Endeavour* in April, 1770. On the monument are two brass plates, one bearing the following inscription:—"Captain Cook landed here 28th April, 1770. Victoria Regina. This monument was erected by the Hon. Thomas Holt, M.L.C., A.D. 1870. The Earl of Belmore, Governor." The other contains the following words from Captain Cook's journal:—"We discovered a bay and anchored under the south shore, about two miles within the entrance, in 6 fathom water, the south point bearing S.E., and he north point east. Latitude 34° S., longitude, 208·37." The entrance to the bay where Cook landed has other memorials. On the north side is the column erected, on behalf of the French nation, to the French navigator, La Perouse. The enclosure around the column is planted with trees and flowers. The monument erected by Mr. Holt is on a place less elevated, but it can, nevertheless, be seen from several parts of distant suburbs. Public subscriptions are being made for a monument of a more costly kind, to be erected in one of the parks of the city of Sydney.

THE last number of the Bulletin of the Société d'Acclimatation of Paris contains an interesting and important report on the

International Fishery Exhibitions of Boulogne, Arcachon, and Havre.

AT the recent annual meeing of the Royal Cornwall Institution, a discussion arose on a paper read by Mr. Robert Blee "On the Comparative Health and Longevity of Cornish Miners," in the course of which the startling statement was made, that a death occurred every other day among the Cornish miners from the mode in which the meu were raised from the pits.

PROF. DANIEL WILSON, of Toronto, publishes in the *Canadian Journal* an essay on "The Huron Race and its Head-form," illustrated with a lithograph and many outline drawings. Prof. Wilson's investigations lead him to believe that the comprehensive generalisations of earlier American ethnologists, under the guidance of Dr. Morton, which led to the doctrine of a homogeneous cranial type for the American aborigines, have everywhere failed when subjected to the crucial tests of detailed observation, and that we everywhere find transitions from one to another and essentially distinct ethnical group. There is, he concludes, no longer an assumed American man, as distinct from every type in the Eastern Hemisphere as the Catarhine Simiadæ of the Old World from the Platyrhine group of New World monkeys.

ON Monday, August 21, between three and four o'clock in the morning, a large waterspout burst over the village of Ollon and the adjacent mountains in Switzerland. Great damage was done to the roads and vineyards, but no loss of life is reported.

A VIOLENT hurricane and some earthquake shocks are reported from the Island of St. Thomas, in the West Indies, on the 21st of August. Hundreds of houses were destroyed, and over 150 persons killed or wounded.

FROM Indian sources we learn that the rainfall in Bombay this season is generally less than half the average of former years.

A VIOLENT typhoon raged at Kobe in Japan, on the 4th of July. Many vessels were wrecked, and about 400 lives were lost. Great damage was done to property on sea and on land.

THE news of most terrible earthquake shocks and volcanic disturbances comes to us from the Philippine Islands. In the small island named Camiguin, near to Misamis, for some months past a succession of most violent earthquakes has been experienced, causing crevices, &c., in the open country. On the 1st of May, about five o'clock in the evening, the earth burst asunder, and an opening was formed 1,500 feet long. Smoke and ashes, earth and stones, were thrown up and covered the ground far and near. At about seven o'clock, as darkness was coming on, this crater burst into activity with a loud explosion, followed by a shower of lava and ashes. About 150 persons were destroyed. The eruption of the new volcano has since been so tremendous that the inhabitants have forsaken the island, and of the 26,000 previously there, not 300 are left. Camiguin is only about thirty-six miles in circumference, and was very productive in abacá (the Manilla hemp) yielding annually from 30,000 to 40,000 piculs, or more than a tenth of the produce of the world. There is little hope of the island ever being again reoccupied or cultivated.

THE *American Journal of Microscopy* recommends, as the best plan of collecting diatoms in large quantities, to tie a thin, fine piece of linen over the faucet of the hydrant in the evening, and allow a small stream of water to pass through it all night. In the morning take off the cloth and rinse it in a little water in a goblet. When ready to examine, take a drop of water from the bottom of the goblet with a small pipette, or glass rod, and place it on a flat slide, or a slide with a concave depression, holding a few drops. Then, with a power of 100 or 350, sweep the field, and you will be rewarded with the sight of a wondrous collection of beautiful and unique forms.

## *THE BRITISH ASSOCIATION MEETING AT EDINBURGH*

### SECTION A.

*Report of the Tidal Committee*, by Sir W. Thomson.

He stated that the work performed for the Tidal Committee since the last meeting of the British Association had consisted chiefly in the evaluation of tide components in a similar manner to that described in the previous reports. Mr. Parkes having again placed the tracings of the curves of the Kurrachee (Manora) self-regulating tide gauge at the disposal of the committee, a second year's observations had been read off and completely reduced. In addition to the tide components evaluated for Liverpool and Ramsgate, others had been introduced to correct the lunar diurnal (declinational) tides for parallax. Those components had been found to have sensible values for Kurrachee, where the diurnal tides are comparatively large. The solar elliptic semi-diurnal components had also been included, now that two complete years' observations were available. The comparison between the calculated and recorded heights from Liverpool not being considered as good as might have been expected from the labour bestowed on them, it was determined to continue the analysis of the Liverpool tides, with the view, if possible, of detecting the cause of the largeness of some of the differences. It would be seen on comparing the results contained in the previous report with the results arrived at, that the chief tides (the lunar and solar semi-diurnal) are now more retarded by about 4° than during the year previously analysed. The calculated heights in the comparison should therefore more nearly represent the heights about eight minutes after the hours assigned to them. An examination of the differences would show this to be the case. A fresh calculation and due allowance made for atmospheric pressure would doubtless very considerably reduce the discrepancies. The gradual increase in the height of the mean level of the water, probably arising from the filling in of the bed of the river, and consequent increase of friction, would account for some portion of this increased retardation. There was a very violent rise in the mean level for the year 1868-69, amounting to four-tenths of a foot. It, however, in the following year, had again subsided to almost its anticipated height. The uncertainty in the mean level of the water is an element which must at times seriously affect the differences between calculated and recorded heights, in any method of computation of heights from a *fixed* datum. It was very much to be regretted that the authorities at Liverpool had chosen the George's Landing-Stage for a tide float, affected as it must be (sometimes to a considerable extent) by the ever-varying weight it has to bear. This would affect the whole of the tide components evaluated, but more especially the solar components, and will account for the different values of the solar semi-diurnal tide, which, judging from the corresponding lunar component, should agree within much narrower limits. It was therefore thought that, should it be determined to again discuss the Liverpool tides, it would be better to take the tide curves as self-registered at Helbie Island, at the mouth of the Dee, in preference to those of the George's Pier. The Helbie Island tide curves, it was considered, would give much superior results. Through the kindness of the United States Coast Survey Office, two years' tide observations, taken at Port Point, San Francisco Bay, California, had been received. Here again there was an abrupt diminution in the height of mean level for the first two years. It having come to the knowledge of the Tide Committee that the United States Coast Survey Office were in possession of a series of hourly tidal observations, taken at Cat Island, in the Gulf of Mexico, and which were of a very remarkable and interesting character, it was thought a favourable opportunity of testing the value of the harmonic analysis for the evaluation of the components of the tides of this place, which appeared very complicated and peculiar. Application having been made, a series of about thirteen months had been received, and were now in course of reduction. It was extremely interesting to find that, although the lunar and solar semi-diurnal tides were very small in value, the series of means from which they were obtained were extremely regular and good, and the consequent determination of the phase of spring tides from their respective epochs was probably correct within a few minutes. The proportion between the amplitudes of the lunar and solar semi-diurnal tides was the nearest to equality yet obtained, being in the ratio of 11 to 6. The proportion between the lunar and solar diurnal (declinational) tides was about 4 to 1. After reading the report, Sir William said that one chief object which the originators of this investigation had in view was the determination of long period tides, and particularly the lunar declinational fortnightly tide, and the solar declinational semi-annual tide. The reason for desiring the determination of such tides with great accuracy was that this would give a means of estimating with absolute certainty the degree of elastic yielding which the solid earth experienced under the tide-generating influences of sun and moon. It was quite certain that the solid earth did yield to some degree, as it must do so unless they were infinitely rigid. It had long been a favourite assumption of geologists that the earth consisted of a thin shell of solid rock twenty to fifty miles thick, according to various estimates, inclosing an inter or filled with melted material, lava, metals, &c. This hypothesis was, however, absolutely untenable, because, were it true, the solid crust would yield with almost as perfect freedom (on account of its thinness and great area) as if it were perfectly liquid. Thus the boundary of the solid earth would rise and fall under the tide-generating influences so much as to leave no sensible difference to be shown by the water rising and falling relatively to the solid, showing that if the earth, as a whole, had an average degree of rapidity, equal to that of glass, the tides would be very much diminished from the magnitude corresponding to a perfectly rigid globe with water like that of our seas upon it. This consideration, he had shown, rendered it probable that the earth had considerably more average rigidity than a globe of glass of the same size. The mathematical calculation showed a somewhat startling result, to the effect that a globe of glass of the same size as the earth, if through out of exactly the same rigidity as glass on a similar scale, would yield, like an indiarubber ball, with remarkable freedom to the tide-generating influences, thus leaving a very much smaller difference to be shown by water if placed on the surface of such a globe, and estimated in its rise and fall relatively to the solid bottom on which it rested. The precise agreement of precession and nutation with dynamical estimates, founded on the supposition of the earth being perfectly rigid, made it probable that the earth was in reality vastly more rigid as a whole than any specimen of surface rock in the condition in which it was when experimented on in our laboratories. In speaking on this subject about ten years ago to Dr. Joule, that gentleman suggested that probably the great pressure in the interior produces in the material—which might be of the same substance as surface rocks—a greatly-increased rigidity in its actual position at any great depth below the surface; but the proposed tidal observation and calculation was the only method which gave directly, and without any possibly doubtful suppositions regarding interior arrangement of density on the earth, a measurement of its elastic yielding to the tide generating influences. Now that observations from so low a latitude as that of Cat Island were available for comparison with those of the tides on our own coast, the committee might advance hopefully to this part of their inquiry, which, accordingly, they proposed to make a primary object in the calculations to be next undertaken.

The other papers read were almost entirely confined to pure mathematical subjects. They were as follows :—

*Report on Hyper-elliptic Functions* by W. H. L. Russell, F.R.S.

*Note on a Question in Partitions*, by Prof. Sylvester, F.R.S.

*On the Number of Invariants of a Binary Quantic*, by Prof. Cayley, F.R.S.

*On Linear Differential Equations*, and *On Focal Properties of Surfaces of the Second Order*, by W. H. L. Russell, F.R.S.

*On Certain Families of Surfaces*, by C. W. Merrifield, F.R.S. If $z = F(xy)$ be a surface, then writing

$$\alpha = \frac{d^3z}{dx^3},\ \beta = \frac{d^3z}{dx^2dy},\ \&c.$$

If the surface be a cone $(\alpha\delta - \beta\gamma)^2 = (\alpha\gamma - \beta^2)(\beta\delta - \gamma^2)$; and if a cylinder $\alpha\delta - \beta\gamma = 0$, $\alpha\gamma - \beta^2 = 0$, $\beta\delta - \gamma^2 = 0$. In the present paper it is investigated whether solid surfaces, fulfilling these conditions, are necessarily cones or cylinders.

*Description of a Model of a Ruled Cubic Surface*, by Prof. Ball. The cubic surface was $z(x^2 + y^2) - 2axy = 0$.

*On Vortex Rings*, by Prof. Ball.

*On the Mathematical Theory of Atmospheric Tides*, by Prof. Challis, F.R.S.

*Remarks on Napier's Original Method of Logarithms*, by Prof. Purser.

*On the Calculation of e (the base of the Napierian Logarithms) from a Continued Fraction*, by W. L. Glaisher, F.R.A.S. The continued fraction from which $e$ was calculated was

$$\frac{e-1}{2} = \frac{1}{1+}\ \frac{1}{6+}\ \frac{1}{10+\&c.}$$

A formula far more convergent than the ordinary one for $e$ in a series. The calculation gave $e$ to 137 decimals, and confirmed the result given by Mr. Shanks, the value of $e$ given in all the editions of Callet's logarithms being incorrect from the fortieth figure.

*On Certain Definite Integrals*, by J. W. L. Glaisher. The integrals were $\int_0^\infty \sin(x^n)\,dx$. $\int_0^\infty \cos(x^n)\,dx$.

*On Lambert's Proof of the Irrationality of π, and on the Irrationality of certain other Quantities*, by J. W. L. Glaisher. The quantities referred to were chiefly circular and exponential fractions.

*On Doubly Diametral Quartan Curves*, by Prof. F. W. Newman. A large number of drawings of quartic curves were exhibited to the Section.

*On a Canonical Form of Spherical Harmonics*, by Prof. W. K. Clifford. The canonical form in question is an expression of the general harmonic of order, $n$, as the sum of a certain number of sectorial harmonics, the number being when $n$ is even, $\frac{5n-10}{2}$, and when $n$ is odd, $\frac{5n-9}{2}$

## SECTION C.

THE papers to be read on Tuesday numbered twenty-three, so that but little time could be allowed to each author, and then there was time for no more than half the papers to be brought forward. A report by Prof. Duncan, M.B., F.R.S., on British Fossil Corals was read, wherein he pointed out the relations between the neozoic and palæozoic corals. Then Prof. Geikie read his report on the Progress of the Geological Survey of Scotland, a notice of which appeared in NATURE of August 10. Mr. Henry Woodward described a new and nearly perfect Arachnide from the Ironstone of the Dudley coal-field. The Pennystone Ironstone nodules of the coal measures have long been celebrated for their fossil contents having yielded King Crabs, wings of Orthopterous insects, a supposed beetle, and numerous plant remains, both ferns and fruits of Lycopodiaceæ. The specimen described by Mr. Woodward is perhaps the most perfect form hitherto described. It is identical with one figured and described by Buckland as a Diamond beetle (*Curculio*) and named by him *Curculioides Prestvicii*. By means, however, of the specimen now obtained, the author clearly showed that it was not a Coleopterous insect, but a true Arachnide, closely related to the recent genus *Phrynus*. Mr. Woodward proposed, therefore, to name it *Eophrynus Prestvicii*, the genus *Curculioides* being retained for *C. Ansticii*, another specimen also figured by Buckland, which may be a true Rhynchophorous insect. Dr. Bryce called attention to some fossils from the Durine Limestone of Sutherland, Prof. Harkness exhibited one of the earliest forms of Trilobites, and Mr. John Miller furnished some remarks on the so-called Hyoid Plate of *Asterolepis*, and pointed out that it was really the dorsal plate.

Mr. Milne Home brought forward a notice of a scheme for the Conservation of Remarkable Boulders in Scotland, and for the Indication of their Position on Maps. Mr. Moggridge mentioned that in Switzerland a right of property in some of the boulders had been acquired by natural history societies and museums with a view to their preservation, and that on these a brass plate had been fixed with the word "Investable" marked upon it.

Prof. Traquair noticed some additions to the Fossil Vertebrate Fauna of Burdiehouse, near Edinburgh, and also called attention to a Labyrinthodont skull, seven inches long, from the same limestone quarries (of Lower Carboniferous age), probably belonging to Huxley's genus *Pholidogaster*; this was the lowest geological horizon from which the remains of Labyrinthodont Amphibia had been as yet described.

At the meeting of the British Association at Liverpool, the Rev. John Gunn, F.G.S., &c., expressed the opinion that

Boulder Clays ought rather to be regarded as an evidence of a temperate climate in the districts where they are found, than of a glacial epoch; and in a communication now made he maintained that there is no occasion to invoke any additional causes of change of climature over and above those which are known to exist. He made some remarks on the agency of the sea in scooping out valleys and bays while clearing off or gathering over the surface of any area.

Mr. J. E. Taylor read an interesting paper *On the later Crag Deposits of Norfolk and Suffolk.* Mr. Prestwich remarked that the belief was gradually gaining ground, that the Red Crag was contemporaneous with the Norwich Crag. In regard to the fossil contents, he pointed out the difficulty there was in distinguishing the extraneous species.

Mr. P. W. Stuart Menteath read a very important paper *On the Origin of Volcanoes*, which, unfortunately, had to be hurried through in such a manner that but little could be gained from the hearing of it.

L'Abbé Richard read a paper (in French) *On Hydrogeology.*

Mr. W. S Mitchell reported *On the Leaf-beds of the Lower Bagshot Series.*

Mr. C. W. Peach made some additions to the list of Fossils and Localities of the Carboniferous Formation in and around Edinburgh, and mentioned the occurrence of *Lituites giganteus.*

The Rev. W. S. Symonds exhibited a new *Onchus* spine from the Lower Old Red sandstone of Hay, Brecon.

A number of papers were held over until Wednesday, when it was arranged to read them; but as none of the authors of papers put in an appearance, Prof. Geikie adjourned the reading of the papers until the next meeting of the Association.

## SECTION D

### SUB-SECTION.—ZOOLOGY AND BOTANY

PROF. WYVILLE THOMSON made some observations on the palæontological relations of the fauna of the North Atlantic, as brought to light in his recent dredging explorations in the North Atlantic. In introducing his observations on these fauna, the professor called attention to the fact that, great as the results of the expedition in Her Majesty's ship *Porcupine* might fairly be held to be considered as an addition to scientific knowledge, still, the actual ground got over by dredging at any very considerable depth was of very small extent comparatively with what yet remained to be done. The field for these investigations, therefore, might be called in a sense unlimited.

Prof. Van Beneden read a paper, *On the Bats of the "Mammoth epoch as contrasted with those of the present day.*

### MISCELLANEOUS

Among these we may particularly mention a paper by Mr. W. A. Lewis entitled *A Proposal to modify the strict Law of Priority in Zoological Nomenclature in certain cases* The author insisted that it was perfect infatuation to serve blindly under word of the code drawn up under the sanction of the British Association now thirty years ago, and proposed that where there was now (August 1871) a universal agreement about a specific name, that name shall not be displaced on account of any prior name being discovered.

Dr. Sclater made some remarks on what he held to be an appropriate opportunity of establishing zoological observatories in connection with certain astronomical observatories which were to be established for the purpose of taking observations of the transit of Venus in 1874. On the occasion of the approaching transit, the Astronomer-Royal proposed to organise observing expeditions to the following five stations:—(1) Oohu, Sandwich Islands; (2) Kerguelen's Island; (3) Rodrigues; (4) Auckland, New Zealand; (5) Alexandria. At the first three of these stations it would be necessary to have a corps of scientific observers resident for twelve months previous to the transit, in order that the absolute longitude of these places, which was not now correctly known, might be obtained. Dr. Sclater pointed out how little was yet known of the terrestrial and marine zoology of these three islands, and specified various particulars in the case of each of their faunas, which it would be especially desirable to investigate. He then urged that the addition of one or more zoological collectors or observing naturalists to the corps of astronomical observers in each of these stations would occasion very slight additional expense, and suggested that application should be made to the Government to allow such naturalists to accompany the expedition, and to undertake the necessary explorations. He stated that there was a precedent for this course in what had been done in the case of the Abyssinian expedition.

The department unanimously concurred in the suggestion, and the desirability of such an application to Government being made.

Dr. Grierson read a paper *On the Importance of forming Provincial Museums, in which the Products of Districts might be Exhibited.* These museums could be connected with consulting and lending libraries. and from a central source there could be sent articles for exhibition at different times, and also persons who could give instructions on such subjects. Such institutions would not only tend to spread knowledge amongst the people, but they would be a means of preventing intemperance and improving their moral habits.

Miss Lydia Becker said she took an interest in this subject as one of those to whom a small share of responsibility had been given in enforcing the Education Act, being a member of the School Board of Manchester. That Board was now about to issue a scheme for a general course of instruction, and had appointed a committee for that purpose, of which she was a member. It had always seemed to her to be a matter of extreme importance to introduce such habits of observation as would follow from the introduction of natural science into elementary schools. She believed there was no portion of the population who were more likely to be interested in the matter than the children who attended these schools. They came there with their minds fresh and open to receive those impressions which were given in childhood, and they were very apt scholars. It had been said that the difficulty was in teaching boys; but she thought it was of as much consequence to teach girls natural science as boys. With regard to the principles of physiology and the laws of health, she thought that if any difference was to be made between the sexes, the girls should be first considered in the matter, as so much of the health of the population depended on the intelligence of women in these matters.

Sir Walter Elliot read a paper *On the Advantage of Systematic Co-operation among Provincial Natural History Societies.* It stated that a comparatively hurried inquiry had disclosed the existence of 115 such societies in Great Britain and Ireland. With reference to their publications, although the volumes of a few of the more important were found in several public libraries, the transactions of by far the greater number did not extend beyond their own localities. In this way not only were the great body of naturalists shut out from much useful information, but the isolation which existed must be detrimental to the societies themselves. Two modes of remedying the evil suggested themselves to his mind. One was, to have a central committee or single editor to collect and condense the most useful materials in all the local transactions; and the other, to form groups of societies, and publish the more original and valuable papers in each group under a joint editorship.

Mr. Symonds, who had been connected with the Cotswold Field Club for many years, said one of the great difficulties connected with these societies had been condensing the reports and publishing the papers that were worth publishing in one general volume of transactions. In Gloucester, paper after paper was published of the most valuable kind that would have done honour to the Royal Society if they had been read there, but which it was impossible for persons to obtain unless they were members of the club, or had friends who were members of it. He thought the difficulty could be met by having a council composed of the presidents, vice-presidents, and secretaries of field clubs throughout the length and breadth of the land, by whom the papers which were worth publishing could be selected. The paper which Sir Walter had now read would, he hoped, have the effect of producing some organisation among these clubs such as he had suggested.

A short discussion took place on the desirableness of some effort being made to utilise the information which was contained in many of the papers read before these clubs, and Sir Walter Elliot said he believed that before these meetings had closed a meeting would be held of those interested in this matter, to consider what should be done.

Three papers on Spontaneous Generation were read; the first of which, by Dr. Ferrier and Dr. Burdon Sanderson, F.R.S., was *On the Origin and Distribution of Bacteria in Water,* *and the Circumstances which determine their Existence in Animal Liquids and Fluids.* In this were detailed the results of a large number of experiments undertaken to throw light upon the phenomena of contagion. The authors employed Pasteur's solution, and also certain animal fluids, but they wished it to be understood that the conclusions at which they had arrived had reference merely to the different fluids employed, and had no distinct bearing upon the possibility or non-possibility of spontaneous generation occurring in other fluids. They did not find any evidence to show that organisms arose *de novo* in their fluids. On the contrary, they thought that the occurrence and number of organisms had to do with the extent of exposure to germs either in air or water. Some of the results arrived at were very important. Boiling the fluids employed was always found to destroy all *Bacteria* and their germs, and other experiments were recorded, tending to show that the air did not contain living *Bacteria*, as so many have assumed. They also ascertained that *Bacteria* were unable to resist the effects of desiccation even at the ordinary temperature of the air. Their examination of the fluids of the body tended to show that these, in their normal condition, did not contain the germs of *Bacteria* or other organisms. Blood and serum, when received in super-heated vessels and exposed only to super-heated air, did not undergo putrefaction—apparently because these fluids did not contain the germs of living organisms.

Dr. Dougal then read a paper *On the relative Powers of various Substances in preventing the Generation of Animalcules, or the Development of their Germs, with special reference to the Germ Theory of Putrefaction*, in which he detailed the results of his experiments upon the power which various poisons, antiseptic substances, and salts have in arresting the development of organisms, and in preventing the phenomena of putrefaction. His conclusions were wholly adverse to the germ theory of fermentation and putrefaction.

Dr. Charlton Bastian, F.R.S., followed with a communication *On some new Experiments relating to the Origin of Life.* After calling attention to the fact that not-living mineral materials were continually being converted into the substance of plants during their growth, and that no special "vital principle" was now believed by physiologists to exist in plants, he said that the question that had to be settled was, whether the elements of not-living matter could group themselves anew, so as to produce living matter under the influence of the same physical forces which were concerned in bringing about the growth of the plant; or whether such combination could only be effected in the presence of pre-existing living matter in which (as was generally admitted) no special forces were resident. This question could, he thought, only be settled by experiments. Fluids deemed suitable for the production or development of living things had to be enclosed within hermetically sealed vessels, and then such flasks and their contents had to be exposed to a degree of heat which could be proved to be destructive to any pre-existing living matter which they might contain. If, after the lapse of a certain period, the flasks still remaining hermetically sealed, the fluid showed evidence of the existence and multiplication of life, then it was argued such living things must have been evolved *de novo* from some new combination among the organic molecules contained in the solution. It was therefore obviously impossible to come to any conclusion on the subject until it had been definitely ascertained what amount of heat living matter (existing in the form of the lowest organisms) could withstand. The evidence on this subject was, Dr. Bastian thought, very clear and decisive. In the first place, he had taken water containing large quantities of *Amœbæ*, ciliated infusoria, and other organisms, and had ascertained that they were invariably killed by raising the temperature of the water in which they were contained to 140° F. When we have to do with organisms of this size there can be no difficulty in ascertaining what the effects of the heat have been. Some of the organisms were partially disorganised by this temperature, and none of them ever showed any signs of life after the exposure, although kept under observation for 24 hours or more. Dr. Bastian then referred to other experiments which he had elsewhere recorded, showing that *Bacteria*, *Torulæ*, and their germs, whether visible or invisible, were destroyed by exposure for ten minutes to 140° F.

A solution of tartrate of ammonia when inoculated with a drop of fluid containing living *Bacteria* and *Torulæ*, became quite turbid in the course of one or two days, owing to the presence and multiplication of myriads of *Bacteria*. But when a similarly inoculated solution was exposed to the temperature of 140° F. or upwards, it afterwards remained perfectly clear, even though freely exposed to the air, thus showing, not only that the organisms and their germs which had been inoculated were killed by exposure to this temperature, but that the air did not contain any such multitude of living *Bacteria* germs as had been alleged. Even had he been unable to fix the precise degree of heat which was fatal to all those lower organisms, it would be important to remember that the greatest unanimity of opinion prevailed among almost all experimenters (such as Pasteur, Huxley, Pouchet, Wyman, and others) as to the fact that the lower organisms were killed in fluids heated to 212° F. Knowledge as to the limits of "vital resistance" to heat being declared the necessary starting-point for further investigation, he had made twenty-four experiments at temperatures ranging from 266° to 302° F., and he called particular attention to the fact that in about one-half of these experiments no living things had been obtained from the sealed flasks. His conclusion, therefore, as to the possibility of the *de novo* origin of living matter could not be rebutted by other experimenters who hastily recorded one or two negative results with the view of showing that he had been in error. Three of the most successful of his more recent experiments in which he had resorted to these high temperatures were then recorded. In two of these strong turnip infusions, neutralised by *liquor potassæ*, were employed, one of which was exposed to 266° F. for twenty minutes, and the other to 293° F. for ten minutes. The hermetically sealed flasks and their contents were subsequently kept in a warm place for eight or nine weeks, and they were exposed for several hours daily during eight days to the direct influence of sunlight. Before opening the flasks the vacuum was ascertained to be still preserved. After breaking the necks of the flasks the fluids were found in both cases to have become slightly acid, and to present a somewhat sour odour. On microscopical examination of the fluid *Torulæ* in all stages of development were found in both, and in that which had been exposed to the temperature of 226° F. a considerable number of *Bacteria* were also present. In the third experiment a strong infusion of a common crucifer was made, and the sealed flask into which it was introduced, after having been exposed to the temperature of 266° F. for twenty minutes, was subsequently maintained at a warm temperature, and also subjected to the influence of direct sunlight for a time. The vacuum having been ascertained to be well preserved, the flask was opened at the end of eight weeks, and among the contents of the flask there were found three slowly moving, very minute *Protamœbæ*, and many extremely active tailed *Monads*, in addition to multitudes of *Bacteria* and *Torulæ*. The active tailed *Monads* obtained from this flask were almost immediately exposed in an experimental hotwater oven to a temperature of 140° F. for ten minutes, and the result was that all these *Monads* taken from the hermetically sealed flask which had been heated to 266° F. were killed by the much lower temperature of 140° F. This result was subsequently confirmed by other observations which tended to show that *Monads* were not only killed by a temperature short of that at which water boils, but that they were more or less disintegrated by such an exposure. The experiments, supported as they were by many others of like nature, were, Dr. Bastian contended, of so strict and crucial a nature as to entitle us to believe that living matter might be born *de novo* in solutions, owing to the occurrence of new combinations therein. He further contended that such new-born living matter might, as the experiments tended to show, more or less directly assume the forms of some of the lowest organisms, just as specks of crystalline matter assume those more or less complex shapes which characterise the crystals of various saline substances.

A general discussion then followed on the three papers, and perhaps the most practical contribution to it was furnished by Miss Becker, who said that the question had an important bearing on domestic economy, in relation to the making of preserves and the preservation of jam from mould. She advised the ladies present, when making preserves, to exclude the air before the preserve had cooled. The President afterwards took back the audience to the regions of pure science, and congratulated his hearers that this most important subject was now attracting the attention of many earnest and philosophical workers.

### SUB-SECTION.—ANTHROPOLOGY

On Tuesday, August 8th, the Anthropological section, in consequence of the crowded attendance, moved into the largest lecture hall in the Science and Art Museum. Mr. Kaines read the first paper *On the Anthropology of Auguste Comte*," in which he expounded the views of that author according to the principles laid down by Mr. Darwin He argued that man's intellectual and moral nature, as well as his body, were derived by natural causes, from the lower animals; and he maintained that Auguste Comte's worship of humanity would be the great doctrine of the future. He stated also that there were evidences of man's development furnished by the low condition of the human skulls of the palæolithic age.—Canon Tristram, in the discussion on this paper, granted what Comte and his followers had to say about the physical organism of man being like that of the lower animals, but was there not a metaphysical side to this question which ought to be heard.—Mr. Boyd Dawkins considered that Mr. Haines confounded two different propositions together in his essay. It was assumed, that because man's body was probably derived from that of the lower animals, his mind was equally thus derived. All naturalists were agreed, that man, so far as his body went, was descended from the lower animals; that he was the crown and front of the animal kingdom. But as regards man's mind, and his moral and intellectual faculties, he denied that any evidence whatever had been brought forward to show that their rudiments were to be found in any of the lower animals. The very least that can be done is to wait for more evidence, and the very worst to confound body with mind. He also denied that the palæolithic skulls afforded any trace of a lower state of intellect than at present. The skull of man found in the Dordogne was rather larger than usual; and that of Neanderthal, according to Prof. Huxley, might have enclosed the brain of a philosopher. We had no right to ascribe the actions of the lower animals to the same motives as our own, or to judge of their intellectual faculties by our own standard. On the evidence at present before us we must be content to confess our own ignorance.

On Wednesday, August 9, the following papers were read in the Anthropological department:—W. J. W. Flower commenced *On the Succession of the several Stone Implement Periods in England.* He argued that the two eras, Paleolithic and Neolithic, which had been given to these implements, were not now enough for England, the drift-period being separated from that of the cave, and that again from the tumuli and barrows.—Mr. Pengelly objected to the difference in time being made between the rough and polished flint implements, suggesting that it was probable that men who wished implements for rough and ready purposes, would break them off and form them, and would not go to the trouble of polishing them. He thought the fourfold arrangement of flint implements suggested by Mr. Flower might be convenient, but that at the same time the different kinds might be of contemporary formation.—Col. Lane Fox was inclined to think many of the types were accidental, arising from want of time for the worker to employ his talent. There were two great designs, however; one in which an end is rounded off so that the implement can be used in the hand, the other design being pointed at both ends, so that the implement might be inserted in a haft. As to the duration of the stone period, he thought we required a great deal more investigation and information.—Mr. Prestwich, assuming that the rivers of the flint period were frozen five months of the year, as they were now in Siberia, said some of the rough implements would have been used for cutting holes in the ice, while others would be used for digging roots. Another form, common to a later period, were the scrapers, used for scraping the skins of animals.

A paper was read from the Rev. W. Webster, *On certain Points concerning the Origin and Relations of the Basque Race.* It was in contravention of some ethnological theories propounded by Professor Huxley at a former meeting of the Association. A brief discussion took place, in which it was shown that the Basque language had similar inflections to those of the eastern languages.

Prof. Struthers gave a paper *On Sagittal Synostosis*, which was almost entirely anatomical. A controversy between design and evolution was introduced in the discussion of it.

Prof. McCann read a paper in opposition to *Mr. Darwin's Views on the Moral Sense in the Lower Animals*, maintaining that the moral sense was only implanted in man, and was the result of Divine intuition. A discussion took place on this, in which Prof. Struthers and Mr. W. Goodsir addressed the section.

The business was concluded by the President recapitulating generally the transactions of this department during the meeting. A cordial vote of thanks was passed to Prof. Turner for his services in the chair.

The sub-section was very well attended throughout the meetings, in spite of the desultory nature of the discussions, and the heterogeneous character of the papers, and of the absence of the usual debates.

## SCIENTIFIC SERIALS

THE fifth number for the present year of the *Bulletin de l'Académie Royale des Sciences de Belgique* (May 1871) contains several important papers, among which we may particularly notice a Synopsis of the Cordulinæ, by M. E. de Selys Longchamps, and some investigations on the Evolution of the Gregarinæ, by M. E. Van Beneden. The former is a monographic revision of the extensive sub-family of Dragonflies, of which the genius *Cordulia* is the type; it contains a general sketch of the group and its subdivisions, and descriptions of all the known genera and species, with indications of the chief synonymy. This fresh instalment of the author's synopsis of the Dragonflies will be welcomed by entomologists. M. van Beneden's paper is a most valuable contribution to the history of those obscure parasites, the Gregarinæ; his observations were made on an unusually large species, measuring as much as 16 millim. in length, and found in the intestine of the lobster. This species, named by the author *Gregarina gigantea*, is figured in various stages in the plate accompanying the paper.—From M. J. J. d'Omalius d'Halloy we find a short note on the natural forces, in which he argues for the existence of a distinct vital force, and expresses the opinion that the vital force of man differs from that of other living beings.—M. F. Duprez discusses the observations on atmospheric electricity made at Ghent, and compares them with those made at other places; and M. de Koninck gives a tabular list of the fossil corals of the Carboniferous formation, showing their distribution in various parts of the world.

THE first publication of the Anthropological Institute has just appeared in the form of a double number of the journal, to which is attached an appendix extending over 160 pages, and containing the proceedings of the Anthropological and Ethnological Societies prior to the date of their union. Amongst the most important papers in the appendix we may mention those on "Some of the Racial Aspects of Music," by Mr. Kaines; on "The Kinnerian and Atlantean Races," by Mr. M'Lean; on "The Concord and Origin of Pronouns, and the Formation of Classes or Genders of Nouns," by Dr. Bleek; on "Some Stone Implements from Africa and Syria," by Sir J. Lubbock; on "The Prehistoric Antiquities of Dartmoor," by Mr. Spence Bate; and on "East African Tribes and Languages," by Dr. Steere. The journal itself contains eight papers, all of considerable value, but amongst which we may especially refer to those by Sir J. Lubbock "On the Development of Relationships;" by Mr. C. Stanisland Wake on "The Mental Characteristics of Primative Man, as exemplified by the Australian Aborigines;" by Dr. Bleek on "The Position of the Australian Languages;" and by Mr. Boyd Dawkins on "The Results obtained by the Settle Cave Exploration Committee out of Victoria Cave in 1870."

NO. 3, Vol. I. Ser. 2, of the *Proceedings of the Royal Irish Academy* has just been published. It contains, Science:—J. W. Dawson, LL.D., Addendum to paper on Eozöon; Professors King and Rowney on the Geological Age and Microscopic Structure of the Serpentine Marble or Ophite of Skye, Plate XIV.; also on the Mineral origin of the so-called Eozöon Canadense; Prof. Hennessy, F.R.S., on the Flotation of Sand by the rising tide in a tidal estuary; Dr. J. Purser, Report on the Researches of Prof. Cohnheim on Inflammation and Suppuration; C. R. C. Tichborne, report on the Molecular Dissociation by Heat of Compounds in Solution (abstract). Polite Literature and Antiquities:—H. Stokes, a List of the existing National Monuments of Ireland in the County of Kerry; A. G. More, F.L.S., on an Ancient Bronze Implement found near the Hill of Tara; R. R. Brash on an Ogham Stone at Kelbonane, County of Kerry, Plate XIII.; and an Appendix contains minutes of the proceedings of the Academy.

## SOCIETIES AND ACADEMIES

### LONDON

Hackney Scientific Association, June 6.—From the report read by the hon. secretary, Mr. H. W. Emons, it appeared that the society had made good progress during the past session, the number of members having more than doubled, and the papers communicated having been both numerous and important. Amongst the actual contributions by members to the progress of science during the session may be mentioned—A Method for ascertaining the Existence of Lunar Activity (Mr. W. R. Birt, F.R.A.S., vice-president), A new variable Star *e* Herculis (Mr. H. T. Vivian), Experiments to ascertain the Absorption of Atmospheric Nitrogen by Plants (Mr. G. E. Davis), A Determination of the Dimensions of the System of Algol (Mr. A. P. Holden), A List of the Fossil Mammalian Remains in the Lea Valley (Mr. R. E. Olliver), A new Section Cutting Apparatus (Mr. W. West), A new classified Catalogue of variable Stars, &c. Numerous original papers had also been read. Good progress had been made with the library, which had been enriched by contributions from several gentlemen, societies, and members. The officers for the fifth session having been elected, the meeting was brought to a close.

### Paris

Académie des Sciences, August 21.—M. Faye in the chair.—M. Chabris has calculated the quantity of nitrate of ammonia yearly carried down to the soil for the nutrition of plants by means of the rain to be two pounds of nitric acid, and consequently, three pounds of nitrate of ammonia per acre.—M. Dumas contributed to the Academy a piece of bread, the provision for the army, which had been infected by *Oidium aurantiacum*. Such facts are not exceptional, principally in very hot weather, and may be detrimental to the public health, as the fungus spreads very rapidly, and it is very difficult to get rid of it. A special committee has been appointed to prevent the infection if possible. MM. Dumas, Baron Larey, Tulasne the botanist, and Pasteur, the celebrated author of so many works on spontaneous generation, are members.—M. Berthelot has examined most carefully a piece of carbon from the Cranbourne meteorite, an Australian stone, and shows by many scientific arguments that the Cranbourne carbon is quite unlike the Orgueil meteorite carbon (a French specimen). The Cranbourne carbon must have been acted upon by a high temperature in ultra-terrestrian space, and no trace of organic origin is to be found on it.—M. W. de Fonvielle sent through M. Leverrier a note establishing that meteoric phenomena analogous to the Marseilles phenomenon are not exceptional cases.—M. Bert, the former Prefect of Lille during the war, described some most interesting experiments on the effects of pressure in suffocating animals living in a confined space. The rapidity of death is not the same for every kind of animal. If the pressure is very high the death is not due to any mechanical effect or to the want of oxygen, but to the presence of carbonic acid, resulting from respiration. It is poisoned by the produce of its own lungs.—The Academy held a secret committee for the nomination of a free member. The list of candidates long delayed was at last published, and M. Belgrand is at the head. But the nomination will be contested.

August 28.—M. Faye in the chair.—M. Saint Venant, a member of the Academy, sent a rather long paper "On the Motion of the Waves," and tried to express through several groups of equations the several motions, which he calls *houles* and *clapotis*, both of which become manifest when the sea is heavy. These new calculations are in some respects grounded on a work published by Gerstner in 1804, "Theorie der Wellen."—Dr. Wurtz, a member of the Academy, sent a paper "On the Action of Chlorine on Aldehyde."—M. Dumas presented, in the name of MM. Montefiore, Levy, and Kunzel, a work entitled "Experiments on Different Alloys, and principally on a Phosphoric Brass, which can be used for casting guns."—M. Jaussen sent to the Academy a complete report "On the Aëronautical Expedition with the *Volta*." The paper is printed in full.—MM. Troost and Hautefeuille sent a very long paper "On Subchlorides and Oxychlorides of Silicon." These chemical researches were executed in the laboratory of the Normal School, and have induced the learned experimenters to explain a new chemical paradox, and to show how it may happen that silicon appears to be volatilised under very curious and peculiar circumstances.—M. Leverrier read an account of several papers sent from Florence by M. D. Muller, relating to several important questions of terrestrial magnetism. In one of these papers the learned physicist explained how a very large perturbation was observed at the very moment when the sun and the moon came into contact on December 22, 1870, and ceased just when the two discs were separated. M. Muller was one of the Italian eclipse party sent to Terra Nova (Sicily). The view of the eclipse was lost, but a most interesting fact was witnessed. The Italian Government will very shortly issue a special publication on this unexpected phenomenon. It is to be noted, moreover, that the total amount of ecliptic perturbation was diminished in proportion to the distance from the central line of total obscurity.—At the secret committee which followed M. Belgrand was elected a free member.

### New Zealand

Wellington Philosophical Society, July 1.—The president, W. T. Locke Travers, F.L.S., in his address, dwelt on the rapid extinction of the interesting subalpine vegetation of New Zealand, and stated that in a few years many plants that were not rare when he first botanised the Nelson Mountains would soon only be found in herbaria. Mr. Buchanan described the following addition to the flora: *Haloragis aggregata*, *Celmesia laterale*, *Acæna glabra*, *Rosthovia Novæ Zealandiæ*, *Danthonia monoica*, and subspecies of *Danthonia semi-annularis* and *Carex pyrania*. Dr. Knox gave the results of the dissection of the supposed Native Rat, and showed that it could not be distinguished from *Mus Rattus*. Mr. Skey announced the isolation of the bitter principle of the kernel of the Karaka berry (*Corynocarpus lævigata*) as a non-nitrogenous crystallisable resin similar to Digitaline. He proposed to name it Karakine. Dr. Hector exhibited the neck of a Moa with the skin, feathers, and tissues attached, and pointed out the similarity of the feathers to those of the Emu, while they differed from the Kivi; remarking that *Apteryx Mantelli* has the feather shafts prolonged, giving the skin a harsh bristly feel, which distinguishes it from *A. Australis*. He showed a fine series of models of Moa's eggs he had prepared for comparison with the cast of the Æpyornis egg recently received. By a series of specimens which he had obtained alive from the natives and afterwards dissected, Dr. Hector showed that the difference between *Glaucopsis Wilsoni* and *G. olivascens* are merely sexual. Captain Hutton described the following additions to the birds of New Zealand: *Hydrochelidon leucoptera*, Temm, *Procellaria fuliginosa*, *Thalassidroma marina*, *Strepsilas interpres*, and a Totanus and Laurus that have not yet been determined. He also showed evidence of the existence in New Zealand of a goose allied to the Bean goose of Europe, and stated that it had been surmised by Dr. Finsch in a paper published last year that two of the above—Strepsilas and Totanus—would probably be found in New Zealand, and also an Actitis, which has not yet, however, been obtained.

## CONTENTS



Erratum.—Vol. iv., p. 358, second column, line 20 from bottom, for "1870" read "1770."

### *NOTICE*

*We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception.*

THURSDAY, SEPTEMBER 14, 1871

## *THE ANCIENT GEOGRAPHY OF INDIA*

*The Ancient Geography of India.* I. The Buddhist Period, including the Campaigns of Alexander, and the Travels of Hwen-Thsang. By Alexander Cunningham, Major-General, R.E. With thirteen maps. (London: Trübner and Co., 1871.)

THE principal difficulty in the study of Indian antiquities has always been the absence of a chronological framework. The Indians themselves had no idea of what we mean by history. They possessed a vague regard for antiquity, but for an antiquity measured by millions of years; while an attempt to find out whether a certain event had happened fifty or a hundred years sooner or later, seemed to possess in their eyes no interest whatever. The result has been that even at present, after Sanskrit literature has been studied for nearly a hundred years, we are still completely in the dark as to the chronology of ancient Indian history. We have a date here and there, as, for instance, the date of Buddha, the great reformer, or of Pâ*n*ini, the great grammarian; but even these are dates which rest to a certain extent on the good will of Sanskrit scholars, and which it would be difficult to defend against the attacks of uncompromising sceptics. Some people still speak of the Laws of Manu as an ancient authority dating from the eighth century B.C.; others would hesitate to assign that compilation in its present form to an ante-Christian era. The dates of the Mahâbhârata and Râmâya*n*a, the two great epic poems, the dates again of the six systems of Hindu philosophy, are equally uncertain, and the Purâ*n*as which were at one time quoted as co-equal with the most ancient literary monuments of the world, are now assigned to the age of Charlemagne rather than to that of Moses.

It may easily be imagined therefore how gratefully Sanskrit scholars would receive any kind of authentic information that should enable them to draw a line somewhere, and to vindicate for certain events and certain works of literature a date that could no longer be called in question. The contact between India and Alexander the Great enabled scholars to fix the date of King *K*andragupta as the contemporary of Alexander, and through him the date of another king, A*s*oka, who had raised Buddhism to be the state religion of his realm, and had left besides some important inscriptions which we possess, and which are written in a language that is no longer Sanskrit. Unfortunately the Greek accounts of India are so meagre that they did not yield much help for determining the literary state of India, and it is a curious fact that no native writer ever mentioned the name of Alexander as the invader of India.

The next contact between India and the outer world was through Buddhism. Buddhism was a proselytising religion, and even before the beginning of the Christian era Buddhist missionaries had reached Tibet and China to preach there the doctrines of Buddha. Thus it happened that after Buddhism had been established in China, pilgrims from that country travelled to India as the Holy Land of their religion, and spent years in the country collecting relics and manuscripts, and learning the language in which the sacred books of Buddhism were written.* Some of them wrote descriptions of their travels in India, and the two most important of them, the travels of Fa-hian and Hiouen-thsang, have been preserved. It is true that Fa-hian belongs to the beginning of the fifth century A.D., while Hiouen-thsang travelled through India from 629 to 645. But even such late witnesses were not to be despised, and it is well known that the publication of Hiouen-thsang's travels by M. Stanislas Julien marked quite a new epoch in the history of Sanskrit scholarship. Here was at all events *terra firma* where historians might take their stand to look forward and backward. Cities which he had visited, buildings which he had described, kings whom he had seen, books which he had read, stood out like landmarks in the desert of Indian history; and though their date might hereafter have to be fixed as much anterior to Hiouen-thsang or Fa-hian, yet it was something to be convinced of their historical reality even at the late date of these Chinese travellers. With regard to the history of Sanskrit literature, the gain was less considerable than might have been expected, for although both Fa-hian and Hiouen-thsang learned Sanskrit, they learned it for the sake of Buddhist literature only, and cared but little for the ancient literature of the Brahmans. Yet from time to time we gain a few valuable grains. We must not forget that the time when the whole of Sanskrit literature was regarded as a forgery and the ancient language of India as a mere invention is not so very distant; and that the fact of a Chinese traveller of the seventh century giving a paradigm of the Sanskrit verb *bhû*, "to be," would have been extremely useful in silencing Dugald Stewart's scepticism. It is equally interesting that the Chinese pilgrim mentions at least one archaic form as peculiar to the grammar of the Veda—viz., *bhavâmasi*, "we are," instead of the common *bhavâmas*. The mention also of some technical grammatical terms, such as *tinanta*, verb, *subanta*, noun, *Unâdi*, and possibly *Nirukta*, are curious as showing that Hiouen-thsang still learned Sanskrit according to the system of Pâ*n*ini, and not of some later grammarians.

The most important evidence, however, that could be gathered from the works of these Chinese pilgrims was geographical. M. Vivien de Saint-Martin, in France, and Prof. Lassen, in Germany, have fully availed themselves of that evidence in their works on the Geography and Antiquities of India; and General Cunningham's new work on the "Ancient Geography of India" is, in fact, a running commentary on the travels of these Chinese priests. General Cunningham's name is well known in England as an indefatigable explorer of Indian antiquities, and he brings to his task accomplishments in which few scholars could excel him. We may quote his own words:

"My own travels," the General says in his Preface, "have been very extensive throughout the length and breadth of Northern India, from Peshawer and Multan, near the Indus, to Rangoon and Prome on the Irawadi, and from Kashmir and Ladâk to the mouth of the Indus and the banks of the Narbada. Of Southern India I have seen nothing, and of Western India I have seen only Bombay, with the celebrated caves of Elephanta and Kanhari. But during a long service of more than thirty years in India, its early history and geography have formed

* "Buddhist Pilgrims," in M. M.'s "Chips from a German Workshop," vol. i. p. 236.

the chief study of my leisure hours; while, for the last four years of my residence, these subjects were my sole occupation, as I was then employed by the Government of India as archæological surveyor to examine and report upon the antiquities of the country."

General Cunningham has divided the geography of India according to the same system which is generally adopted in the history of India, viz., into the Brahmanical, the Buddhist, and the Mohammedan periods; and he has selected the second or Buddhist period as the principal subject of his first volume. The first or Brahmanical period traces the gradual extension of the Aryan race over Northern India, and comprises that early section of their history during which the religion of the Vedas was the prevalent belief of the country. The geography of that period, as far as it can be worked out from the Vedic writings, has been treated by M. Vivien de Saint-Martin, and by Prof. Lassen in his "Indische Alterthumskunde."

The second or Buddhist period embraces the rise, extension, and decline of the Buddhist faith, from the time when Buddhism became the state religion of India to the conquests of Mahmud of Ghazni. As the beginning of the political influence of Buddhism coincides in time with the invasion of India by Alexander and the subsequent establishment of Greek dynasties on the Indian frontier, the historian of this period has, in the beginning, the advantage of the Greek accounts, while further on, from 400 to 700, he has to depend mainly on the accounts furnished by Chinese pilgrims. This period, too, has been ably treated by M. Vivien de Saint-Martin in several *mémoires*, and by Prof. Lassen in his "Indische Alterthumskunde," yet there was room left for new inquiries; and the results of these inquiries have been published by General Cunningham in the volume now before us.

The third or Mohammedan period has not yet been treated as a whole, though there are ample materials for it in the works of Reinaud, Elliot, Erskine, and others.

The chief merit of General Cunningham's work consists in his description of spots of which he can speak as an eye-witness. Here his knowledge of the actual localities has enabled him either to confirm the identifications of his predecessors, or to fix by more correct evidence the real site of the places described by Greek or Chinese geographers. He furnishes himself, at the end of his Preface, a list of the more important of his own identifications. Whenever his identifications are based on local peculiarities, his arguments seem always powerful and convincing. It is when he bases his views on the evidence of mere names that one feels occasionally inclined to withhold one's assent. The changes in local names are, no doubt, most capricious, and amenable to hardly any rules. Everything is possible here; but for that very reason nothing should be assumed that cannot be proved by historical evidence. Hiouen-thsang calls Ceylon *Seng-kialo*, which is the Chinese rendering of the Sanskrit name *Sinhâla*. The fuller Sanskrit name is *Sinhâla-dvîpa*. This passes through a chain of changes, all of which can be traced historically, from *Singal-dib* to *Sirandib* to *Zilan* and *Ceylon*.

These changes may seem violent; but they are not half so objectionable as, for instance, the simple change *Sâlâtura* to *Hâlâtura*, *Alâtur*, and finally to *Lahor*, proposed by General Cunningham (pp. 57, 58). It is true that the *s* of *Sindhu* is changed to *h* in *Hindu*, and afterwards elided in *India*, but the *s* of *Sindhu* is different from the palatal *s* of *Sâlâtura*. Besides, that dental *s* was changed into *h* in Persia, not in India, and dropped at last only in the mouths of Greeks, who first heard the name from the mouths of the Persians. The same objection applies to the proposed change of *Svetavâsa* into *Khetâs* (p. 125). The *sv* of *Sveta*, "white," would not become *Kh* in the western countries; it could do so only if the *s* were a dental *s*, which it is not.

Again General Cunningham admits occasionally formations of Sanskrit names, which are entirely against the genius of the language. On page 29, in explaining the name of *Begrâm*, he says:—"Masson derives the appellation from the Turki *be* or *bi*, 'chief,' and the Hindu *grâm* or 'city,' that is, the 'capital.' But a more simple derivation would be from the Sanskrit *vi*, implying 'certainty,' 'ascertainment,' as in *vijaya*, 'victory,' which is only an emphatic form of *jaya*, with the prefix *vi*. *Vigrâma* would, therefore, mean emphatically 'the city,' that is, 'the capital,' and *Bigrâm* would be the Hindu form of the name." A Sanskrit scholar would say at once that such a compound of *grâma*, "village," with the preposition *vi* is impossible. The preposition *vi* may be joined to a verb or verbal noun, like *jaya*, "victory," but not to a noun like *grâma*. I had, myself, derived the name of *Begrâm* from *bhaga-ârâma*, the abode of the god Bhaga, or of the gods in general; taking *bhaga* either in the sense of the Sun-god, or like the Zend *bagha*, the old Persian *baga*, in the sense of gods in general, and *ârâma* as abode. *Bhagârâma* changed to *Begrâm* would be a sort of synonym of *Behistân*, τὸ Βαγίστανον ὄρος, the place of the *Bhagas*, or of *Bhaga*, the Lord. In this conjecture I have since been confirmed by finding that Albyruny mentioned *Bhagapura*, town of Bhaga, as one of the names of Multân (Reinaud, Mémoire, p. 98).

It is well known that the name of the Kabul river, Κώφην, occurs in the hymns of the Rig Veda as *Kubhâ*, but I cannot understand on what ground General Cunningham declares that name to be non-Aryan. The etymology of proper names is never very easy, but there would be no difficulty in connecting *Kubhâ* either with *Kumbhâ*, "vessel," Greek κύμβος, or with κυφή an old Cretan word for "head" (Sk. ka-kubh), or with κυφός, "bent, crooked." *Kutilâ*, "crooked," is the name of a river, and *Kampanâ*, "the trembling," is the name of one of the rivers of Kabulistan, it may be of the Kabul river itself. As *Kubhâ*, the Kabul river, is mentioned but twice in the Rig Veda, I shall give the two passages:

Mâ vah Rasâ anitabhâ Kubhâ Krumuh mâ vah Sindhuh ni rîramat, Mâ vah pari sthât Sarayuh purîshinî asme it sumnam astu vah. ("O ye Storm-gods, let not the Rasâ with infinite splendour (amitabhâ), let not the Krumu, or the Sindhu delay you; let not the misty Sarayu surround you:—with us alone be your delight!") (Rv. v. 53, 9.)

Trishtâmayâ prathamam yâtave sagûh Susartvâ Rasayâ Svetyâ tyâ Tvam Sindho Kubhayâ Gomatîm Krumum Mehatnoâ karatham yabhih îyase. ("First joined together with the Trishtamâ for thy course, with the Susartu, the Rasâ, the Svetî, thou O Sindhu (goest), with the Kubhâ to the Gomatî, the Krumu, with whom thou proceedest together with the Mehatnu.") Rv. x. 75, 6.

This verse is not free from difficulties, and in some parts my translation may be questioned. But it is clear in the main that the poet in praising the Sindhu (the river Indus), mentions its tributaries. The first tributaries which join the Indus before its meeting with the *Kubhâ* or the Kabul river cannot be determined. All travellers in these northern countries complain of the continual changes in the names of the rivers, and we can hardly hope to find traces of the Vedic names in existence there after the lapse of three or four thousand years. The rivers intended may be the Shauyook, Ladak, Abba Seen, and Burrindoo, but one of the four rivers, the Rasâ, has assumed an almost fabulous character in the Veda. After the Indus has joined the Kubhâ or the Kabul river, two names occur, the *Gomatî* and *Krumu*, which I believe I was the first to identify with the modern rivers the *Gomal* and *Kurrum.* (Roth, Nirukta, Erläuterungen, p 43, Anm.) The Gomal falls into the Indus, between Dera Ismael Khan and Paharpore, and although Elphinstone calls it a river only during the rainy season, Klaproth (Foe. koue ki, p. 23) describes its upper course as far more considerable, and adds : Un peu à l'est de Sirmágha, le Gomal traverse la chaîne de montagnes de Solimán, passe devant Raghzi, et fertilise le pays habité par les tribus de Dauletkhail et de Gandehpour. Il se dessèche au défilé de Pezou, et son lit ne se remplit plus d'eau que dans la saison des pluies ; alors seulement il rejoint la droite de l'Indus, au sud-est du bourg de Paharpour." The *Kurrum* falls into the Indus North of the *Gomal*, while, according to the poet, we should expect it South. It might be urged that poets are not bound by the same rules as geographers, as we see, for instance, in the verse immediately preceding. But if it should be taken as a serious objection, it will be better to give up the *Gomatî* than the *Krumu*, the latter being the larger of the two, and we might then take *Gomatî*, "rich in cattle," as an adjective belonging to *Krumu.*

I have dwelt longer on this point in order to show how much has to be considered before we decide on the Aryan or non-Aryan character of local names in India. Genera Cunningham writes :—

"The name of Kophes is as old as the time of the Vedas, in which the Kubhâ river is mentioned as an affluent of the Indus ; and as it is not an Arian word, I infer that the name must have been applied to the Kabul river before the Arian occupation, or, at least, as early as B.C. 2500. In the classical writers we find the Khoes, Kophes, and Khoaspes rivers, to the west of the Indus, and at the present day we have the Kunar, the Kuram, and the Gomal rivers to the west, and the Kunhar river to the east of the Indus, all of which are derived from the Scythian *Ku* 'water.' It is the guttural form of the Assyrian *hu* in Euphrates and Eulæus, and of the Turki *su* and the Tibetan *chu*, all of which mean water or river."

The Ku in Kubhâ admits, as we saw, of a far easier interpretation. The *Go* of Gomal is the Sanskrit *go*, "cow," and the *Ku* of Kuram or Kurrum is the first syllable of *Krumu*, which is derived from "*kram*," to stride.

Although on minor points like these, and particularly on linguistic questions, some of General Cunningham's statements are open to criticism, the book as a whole is a valuable contribution to our knowledge of the ancient geography of India, and we hope that this first volume will soon be followed by others.

MAX MÜLLER

## *OUR BOOK SHELF*

*Epilogo della Briologia Italiana.* Del Dottore G. de Notaris, Professore di Botanica e Direttore dell' Orto Botanico della R. Universita' di Geneva. (Geneva, 1869 ; London : Williams and Norgate.)

DR. DE NOTARIS is so well known in this country by his numerous works on mosses and microscopic fungi, as well as by his liberality to correspondents, that it was with great pleasure that we received the noble volume before us, published at the request and expense of the Commonalty of Geneva. It was not to be expected that a country like Italy, where the borders of the Mediterranean are not rich in mosses, should present much novelty, the more Alpine parts yielding very much the same species as the Alpine or more temperate parts of the European districts. It is, however, always interesting to compare the floras of different countries, even where species are so widely spread as the lower Cryptogams, and it is no matter of surprise to find that there are here very few genera which are not amply represented in our own flora. The only genera which have not at present occurred in this country are Lescuræa, Habrodon, Anacamptodon, Fabronia, which is essentially a genus of warmer climates, Dubyella, Oreas, Pyramidium, Conomitrium, Oreoweisia, Septodontium, Angstrœmia, Trematodon, Braunia, Coscinodon, Bruchia. Most of these are genera either containing one or very few species. The following European genera, excluding those found in the British Isles, seem not to occur in Italy : Voitia, Sporledera, Pharomitrium, Eustichium, Pyramidula, Psilopilum, Anisodon, Platygyrium, Thedenia, most of which contain only a single species. The only genera of the British Isles which do not occur in Italy, are Daltonia, the single species, *D. splachnoides*, being confined to one or two localities in Ireland, Œdipodium, Discelium, Bartramidula, Anomobryum, Tetrodontium, Glyphomitrium, Hedwigidium, Anodus, which again are genera for the most part of one species only, so that Italian muscology cannot be considered as essentially different from that of other European districts. There are undoubtedly many good species which do not occur in this country, but it is probable that the number of these will be much reduced, one of the most curious, *Buxbaumia indusiata*, having been found by Dr Dickie at Aboyne in Aberdeenshire. It is much to be wished that some Italian botanist would give a similar work on Italian fungi. The truffles and puffballs of Italy have been admirably worked out by Vittadini, and something has been done for the more noble fungi by Viviani and others, but we ought to look to Italian mycologists for the identification of the fungi of Micheli. There is no doubt that any skilled mycologist would be well rewarded by the investigation of the Italian woods, which doubtless contain numerous interesting species. We must, however, look to the Italians themselves for information, as many difficulties would stand in the way of a person not intimately acquainted with the language of the peasantry. We see no reason why as perfect an enumeration of the fungi should not be given, as that of the Italian mosses now before us.

M. J. BERKELEY

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Thickness of the Crust of the Earth

ARCHDEACON PRATT has given just the answer I expected to my remarks on his defence of Mr. Hopkins. As I said at the time, I scarcely thought it possible that he could have fallen into the mistake of supposing that the disturbing forces to which precision and nutation are due act by fits and starts. But note what follows from this. His whole defence of Mr. Hopkins's method falls to the ground. The very life and soul of that defence was, in almost his own words, that the disturbing forces produced the motion due to their action before friction *had time* to act; or, in other words, that the disturbing forces gave a pull so sharply and quickly that they did their work before friction, which the Archdeacon looks on as rather a sluggard, could rouse itself and counteract them, that they were, in short, able to steal a march on friction each time they gave a pull or a push.

Now it is clear that this explanation has no meaning in it, unless the action of the disturbances is intermittent. Archdeacon Pratt admits that he never supposed that this is the case, therefore he must find some new line of argument, if he wishes to continue his chivalrous defence of his old friend and, I believe, tutor, who can no longer speak for himself. I may add that all who knew Mr. Hopkins personally, and even those who, like myself, only knew of him through common friends, will appreciate and admire Archdeacon Pratt's championship, even if they are unable to agree with him.

In his last letter (NATURE, August 31) Archdeacon Pratt has given a new and independent method, which leads him to the conclusion that the earth is solid from surface to centre, or nearly so; so that if we accept his reasoning we must admit that, whatever may be said of Mr. Hopkins's method, his results at last are right. He mainly rests his argument on the consideration that such a limp thing as an earth with a crust not more than 100 miles thick could not stand the strains with which the disturbing actions of the sun and moon are for ever trying it.

How will it be, though, if we can show cause for believing that the crust of the earth is after all somewhat of a limp thing, and yet does stand these strains? All *a priori* reasoning must give way to fact, if that fact can be established; and though we may be surprised that so thin a crust is able to hold out against the violent treatment it has to undergo, yet, if we can show good reason for believing that the crust is, after all, thin, we must cease to wonder, and try to explain the seeming anomaly.

I shall content myself now with putting forward one of the several grounds on which the thinness of the earth's crust can be, I will not say established, but rendered highly probable; and if my arguments shall prove in the end to have any weight, I have no fear that the seeming contradiction between them and the reasoning of Archdeacon Pratt and Prof. Thomson in the other direction will sooner or later be explained away.

Everyone is familiar with what is known in Geology as Upheaval and Depression, that over and over again during the earth's lifetime portions of the solid crust have been raised, and others lowered relatively to a fixed datum, such as the sea level. Very naturally the idea springs up that the displacement is produced by a thrust acting vertically upwards, or by a removal of some vertical support below. Some cases of small local upheaval may have been brought about in this way, but this is not the machinery by which nature has acted on the large scale.

The fact of upheaval is brought home to us when we find strata originally formed beneath the sea now high and dry far above its level; how the upheaval was brought about we learn by recollecting that these strata were originally horizontal, noting whether they are displaced from that position, and, if so, after what fashion the displacement has taken place. Isolated observations show us strata in some places horizontal, in others inclined at different angles to the horizon; but when we combine into one view a large number of such observations, the result is that we see that the strata have been folded into troughs and arches, that when we find horizontal beds we are on the summit of an arch or the bottom of a trough, where inclined beds appear we are on the slopes. Further, we invariably find that the crumpling up of the strata has been most violent in those parts of the earth's surface which have been raised highest above their original position, that is, on mountain chains. We come, then, to the conclusion that the way in which upheaval has been produced has been by a folding of the strata into troughs and arches. That the crust of the earth, instead of being a rigid unyielding mass, has been from time to time bent into folds, and, so to speak, crumpled up and wrinkled; that it is not unlike, what it has often been compared to, the shrivelled skin of an old dried apple.

Again, the supposition of a thin crust and an internal molten nucleus, gives a very satisfactory explanation of the way in which the crumpling is produced. As the nucleus cools it contracts, and the crust has to accommodate itself to the diminished support within; it cannot shrink, and therefore it gets crumpled, just as in the case of the apple, the inside shrinks more than the skin, and the latter in consequence wrinkles up.

Here, then, is an argument in favour of no very great thickness and a certain amount of limpness in the earth's crust, and it is not the only one of its kind; on the other side, are Archdeacon Pratt's and Sir W. Thomson's weighty reasons in favour of its rigidity. Far be it from me to attempt "tantas componere lites." I only wish to show that there are two sides, and two very good sides, to the question.

What Mr. Hopkins has done seems to me to amount to this: he has shown that with a solid earth the amount of precession would be almost exactly what it is; but he has not shown that this would not equally be the case with an earth having a thin and a *viscous* melted interior; that case he has not attempted to handle, the case he did examine being that of a thin crust and a perfectly fluid interior. If it can be conclusively proved that the thin crust and viscous melted interior are incompatible with known astronomical and mechanical phenomena, we must give them up, but till that has been done we are bound to remember that, whatever has been said against them, there is something in their favour also. A. H. GREEN

Barnsley, Sept. 8

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### Temperature of the Sun

I HAVE just seen the interesting note of Mr. Ericsson in the number of NATURE for July 13 (p. 204), and I am very glad that this question should be thoroughly ventilated.

Mr. Ericsson and others have been startled at the high degree of temperature at which I have arrived, and the appellation of *extravagant* is not spared. I beg leave, however, to observe that this conclusion does not materially differ from that obtained by Mr. Waterston. I am surprised that my opponents, satisfied with rejecting the result as extravagant, do not examine if the method is correct or not. The only objection that could be made is, that, while from the experiments of Soret, the resultant figure is 5,334,840° C., I doubled it, on account of the absorption which the radiation suffers in the solar atmosphere, whose integral effect is a great deal larger than Mr. Ericsson supposes; and it would not be waste of time to discuss the experiments which prove that the absorptive power of the solar atmosphere is very considerable. Mr. Ericsson passes over this too slightly, saying that this absorption would be only 0·01 of the whole, while I have found it considerably greater.

Mr. Ericsson refers to the explanation which I subjoined about this high temperature, that it is to be regarded as a *virtual temperature*, as if these were words *which I would not attempt to explain*. The explanation was, however, given very clearly in my own book, perhaps too shortly, since it seems not to have been understood. The word *virtual* was also employed by Mr. Waterston to indicate the degree of temperature which would be produced in a thermometer by the accumulated radiation of different transparent strata. And indeed this is not an absurd statement and incapable of conveying information, as Mr. Ericsson seems to suppose.

First of all we must admit that a gas exposed to a radiating source does not always attain the same temperature as a solid body. It is obvious, for instance, that the temperature of the free air at the top of a mountain is a great deal lower than the temperature of the thermometer exposed to the sun. This is due to the small absorptive power of the gas. Therefore, at the boundary of the solar atmosphere the temperature of the transparent gas may be a good deal lower than that of a solid thermometer (if by hypothesis it could preserve there its solidity). It is besides not incorrect to say that the different successive strata may add their own radiation, so that by two, three, or more radiating strata we could obtain a higher temperature than by a single one. At least this has been understood, even by M. Respighi, who, however, is of the same opinion as Mr. Ericsson about the exaggeration of my result. The integral effect of all the strata that contribute to this elevation would be the indication of the thermometer, which may be higher than the temperature of a single outside stratum subjected to external radiation.

Mr. Ericsson says that it is of no consequence whether the sun's photosphere belongs to the class of active or sluggish incandescent radiators. I think, however, this point to be very important. Since we cannot experimentally determine the temperature of the sun except by using its radiating power, it is very interesting to take into account this element as very substantial. Very few, indeed, will allow that which Mr. Ericsson takes for granted, that the radiating power of the solar materials may be compared to that of pure lamp-black, as he assumes at the end of the note.

Mr. Ericsson spends a great part of the note in proving that the law of the diminution of radiation according to the square of the distance is accurate, which certainly I have never questioned. The difference between his own result and mine may perhaps be due to a difference in the use or construction of the instrument; but as, unhappily, I have no information of this construction, I cannot attempt any discussion of his principles. I can only say that his table cannot be used in all seasons indifferently, since I have proved that at the same zenith distance, the absorption of solar heat is very different in summer and in winter, on account of the different quantity of aqueous vapour which is found in the atmosphere. And hence the deductions which he makes about the difference of radiation in aphelion and perihelion may be merely accidentally accurate, and not very conclusive.

On the whole, however, I see that the researches of M. Ericsson approach my results a great deal more nearly than those of M. Zöllner, who fixes the temperature of the lower stratum of the solar atmosphere in contact with the photosphere at 68,400° C. only. And this is a number sixty times less than that of Mr. Ericsson, while mine is only thirty-seven times greater.

The conclusion which spontaneously flows from such extraordinary differences is, that we are yet far from having any exact information on the subject, and I hope that this question will now be better discussed, and that I may be able to find some improvement to be made in my book.

Rome A. SECCHI

### Neologisms

I THINK the most suitable word to indicate plane-direction is "position," though the word "pose" would serve, and has, indeed, been used in that sense. The word "position" bears the same relation to the word "direction" that "Stellung" bears to "Richtung," or "set" to "righting." "Position" is often (but incorrectly) used to indicate *place*, but we may reason with Colonel Mannering, *Abusus non tollit usum*—the abuse of anything doth not abrogate the lawful use thereof. This recognised, the words "position of a plane" can bear no other meaning than that referred to by Mr. Wilson. For the purpose of indicating place, the word "location" would be convenient, but that it suggests to the Latinist a "setting to hire." Our American cousins (very wisely, I think) neglect such trifles.

By the way, is not the word "neologism" very ugly and unnecessary? We must have new words, but need we call them neologisms?

As to the invention of new words, I take it that every author who has anything new to say must sometimes want a new word, in which case he has as fair a right to invent and use such a word as to describe new ideas. If this is not the case, I must plead guilty to a grievous series of offences. In fact, I have received during the past year about a ream of letters rebuking a practice which I consider fully "in my right." You should not speak, writes one, of "a limitless expanse," but of an "unlimited expanse;" you must not say "forceful analogy," urges another, but "forcible analogy;" "not "star-cloudlet" says a third, but "nebula;" not "square to" but either "perpendicular" or "at right angles to" says a fourth, and so on. So must you write if you wish to be understood, say these critics; or rather they say, "It is indispensable for the adequate conveyance of your meaning that you should thus conform to established usage."

I am not jesting; these words have not only been employed by one of my anonymous critics, but have been seriously suggested for my own use. In some cases modes of expression are vilified: for instance, it seems you must not say of Venus that she is "nearer to the sun than the earth is," for this is inelegant; you must say that she is "nearer to the sun than the earth;" and, in like manner, for the sake of euphony, one should say of Mercury that he is "nearer to the sun than the earth," rather than that he is "nearer to the sun than to the earth." My attention has been directed to each of the expressions here corrected as characterised by a vice of style. So that, since Venus in inferior conjunction is nearer to the earth than to the sun, but nearer to the sun than the earth is, she is (when so placed) at once nearer to the earth than the sun, and nearer to the sun than the earth,—a statement which appears to me less instructive than might be desired. But possibly I am prejudiced.

It is well to keep (where one may) within dictionary precincts, nor need the writer neglect the rounding of his periods; but, in my judgment, he should set before both these things what the above quoted critic calls "the adequate conveyance of his meaning." RICHD. A. PROCTOR

Brighton, September 9

THERE remains but one point to notice in reference to the hybrid (or monster) *prolificness*. Dr. Latham pretty well exhausts its etymological bearings. There remains its phonological bearings to consider. No new word has a chance of being naturalised unless it can be pronounced as well as written; and the greater the difficulty of pronunciation the less is that chance. Now, in order to render Mr. Wallace's word acceptable, it must be pronounced as if it were written, *prollyficknuss*, in which phonetic form we almost lose the parent adjective. The reason of this is, that the syllables *ic* and *ness* will not inosculate. To use Mr. Sylvester's phraseology, there is not a perfect anastomosis, and this imperfection is remediable only by change of accent, viz., passing on the accent from *lif* to *ic*; otherwise we must sacrifice anastomosis, and write the word as a compound, *prolific-ness*, *i e*, with a hyphen to indicate the necessity of a pause in that place. Surely on all accounts *prolificence* is by far the better word.

Yoxford, September 7 C. M. INGLEBY

### The Aurora

I HAVE just read Mr. Wilson's interesting paper entitled "Some Speculations on the Auroras," published in your periodical for September 7. In the *Philosophical Magazine* for July 1870 I made a suggestion as to the origin of auroras similar to that just published by Mr. Wilson.

The periodicity in auroral displays noticed by Mr. Wilson had not attracted my attention. It would doubtless, if it were well established, be confirmatory of the views independently put forward by Mr. Wilson and myself. A. S. DAVIS

### Meteor

ON Saturday, September 2, at 8.14 or 8.15 P.M., I saw a fine meteor under very favourable circumstances. I was standing with several friends at the door of Mr. W. F. Moore's house at Croakbourne, in the Isle of Man, and we were looking up at the western sky at the moment when the meteor came. It started between, I think, $\gamma$ and $\pi$ Herculis (it was too cloudy to see those stars), descended nearly vertically, passing through Corona Borealis, and vanished a little below $\zeta$ Bootis, at about 15° above the horizon. It moved slowly but continuously, taking from two to three seconds in travelling over 45°. It broke into three, which followed one another, connected and followed by a luminous train which was visible for about one second. The first part of the three was brilliant white, and was estimated by Mr. A. W. Moore and myself independently as equal in size to $\frac{1}{4}$th of the moon's surface. It was very brilliant, being mistaken by the Rev. John Howard, who was looking in another direction, for a flash of lightning. The two latter globes were blue.

Rugby, September 6 J. M. WILSON

### The Earthquake at Worthing

IN your issue of the 31st ult. is an extract from a letter which appeared in the *Times* a day or two before, giving a very circumstantial and a somewhat sensational account of an earthquake which took place at Worthing, at 3.45 on Monday morning, the 28th of August. Is it not possible that there may be some connection between the said earthquake and the circumstances narrated as under in the *Brighton Gazette* of the Thursday following? If so, might it not be on the whole more prudent of correspondents of the *Times* or other papers, before they rush frantically into print on such subjects, just to put a question or two to some imperturbable old fisherman (if they be shaken out of their wits again at a watering place) instead of appealing to hysterical ladies and excitable old gentlemen for their notes of an event of great scientific interest?

"'What's that? An earthquake! There it is again! Now again! And now again!' These were the exclamations which paterfamilias and materfamilias and lots of juveniles, roused from their slumbers, uttered on Monday at 3.40 A.M., just before the break of day. It was a strange noise; lights flashed from windows, bells were rung violently, windows were thrown up, and cries of 'Thieves' and 'Police' were shouted. But there was no earthquake, there were no thieves, although there were the police, by whom the sounds were distinctly heard. It was some time before all was again quiet, and not even then in many a household until processions in curious garb, armed with sticks, pokers, shovels, and fire-irons, in place of fire-arms, had paraded from kitchen to garret in search of the supposed nocturnal marauders. And now the cause of all this has been discovered. It was the coastguard squadron, a few miles out at sea, having what is termed their night quarter exercise—a turn-out drill in the middle of the night, so as to fit the men for action in an emergency." E. A. PANKHURST

Church Hill, Brighton, September 11

### A Fossiliferous Boulder

DURING a visit I made in July last to a respected friend at Dinnington, Northumberland, I observed a travelled boulder in the corner of his field, and, on closer inspection, found that it contained a number of ammonites, encrinites, and the detached portions of the stems of the stone lily, usually found in the Lias in the vicinity of Whitby. The composition of the boulder, which was about two feet in length, and of proportionate breadth and depth, was basaltic or trap, and had evidently taken up the fossils when in a state of fusion; some of the ammonites being compressed or disturbed. Upon inquiry, it appeared that my friend had sunk for a well, and came at the depth of about twelve feet upon the native freestone rock, upon which this boulder was found. Of course it must have been transported to its place of deposit by ice during the glacial period of our world's history, and then covered over by the subsequent boulder-clay; but from whence was it transported? From Yorkshire or the Hebrides?

I also visited in the immediate vicinity what was formerly the site of an ancient lake of about 1,200 acres, Prestwick Car. This sheet of water was drained a few years ago into the Pont rivulet, and the bed of the old lake is now, through the enterprise and skilful industry of the farmer, covered by luxuriant crops of oats with magnificent heads, approaching six feet in height, and immense thickness of stem. The land, as might be expected, is a deep bog earth; the surface, however, is remarkably light, apparently a peat soil, and easily disturbed, or blown away by the winds. The remarkable point here was, that after the drainage had been completed, the earth solidified and put under culture, the roots and a portion of the stems of trees broken off near the roots, appeared as if rising from the earth, the prior existence of which was unseen and unknown, indicating the remains of a primæval forest: no branches appeared. The wood is that of the alder. Was not the lake originally formed by the destruction of this ancient forest by the agency of wind?

Barbourne, Worcester, Sept. 9 J. BROUGH POW

### A Vital Question

PRAY do not mind if I am alone in my venturesomeness, but in the name of Science, not that which is falsely so called, but that which depends upon evidence, let me protest against the doctrine contained in the concluding portion of Sir W. Thomson's address. Scholastic theology has for me nothing worse than the declaration, made on the strength of a mere dogma, that our dear mother earth is no mother at all, but absolutely incapable of filling any function in the production of her own children. The dogma that life can only proceed from life, appears, when analysed, like too many another dogma, but a meaningless jingle of words.

Here are three counter propositions, which I advance in all confidence of their soundness:—

1. We know nothing whatever of the nature of life to justify us in asserting its absolute difference in kind from many other phenomena, as of magnetism, chemistry, or Nature in general.

2. If, as astronomers hold, all the bodies composing the solar system are derived from the sun, they must contain identical elements. That their elements are actually identical is, moreover, indicated by the spectroscope. So that if those elements be incapable of producing life on this planet, they must be incapable of producing it elsewhere. However much reason there may be to suppose they have not produced life in any particular instance *as yet*, as, *e.g.*, in our satellite, that is no reason against their doing so in the future. If Sir William's object had been *to gain time* for existing evolutions, I could have forgiven him, but there was no hint of this.

3. To speak of life as necessary to the production of life, is to ignore all that Science has ascertained respecting the transference and convertibility of force, and to fall back upon the anthropomorphism of the theologians, only with the difference, in this case, that it is not Jupiter, but "the stone that fell down from Jupiter," whom we are to hail as our father and mother. Moreover, to speak of life as necessary to the production of life, is to assume that we already know the limits of Nature's productive power; and to assert that life is not a natural product at all, is to restrict our definition of Nature by some arbitrary limit which excludes the most important functions of Nature.

Doubtless it would be a very pretty idea to regard the planets as so many orchids in the flower-garden of the Universe, and the meteorites as their fertilising bees; but Sir W. Thomson entertains no such pleasing sentiment respecting the earth. He degrades this unhappy planet far below the meteorite.

Once upon a time when astray with a companion in a far Western wilderness, we were reduced to eating anything that we could find. On the question arising whether rattlesnakes were fit to eat, I propounded the dictum that whatever could itself live ought to be able to support life in another, and our experience, so far as it went, confirmed the saying. I venture to vary it for Sir W. Thomson's benefit, and to suggest that whatever can support life, as this earth does, can in all probability produce it.

Loving, as I do, both the world and the things which are in the world, I hope you will not refuse me a corner for this sorrowing dissent from a doctrine so depreciatory of the world, and whose enunciation cannot fail to give occasion to the many enemies of Science to blaspheme its sacred name on account of the eccentricities of its professors. EDWARD MAITLAND

Oxford and Cambridge Club

### Draining a Cause of Excessive Droughts

WILL you kindly allow me through the medium of NATURE to ask whether any of my fellow readers can give me any *actually observed facts*, to show that draining is justly considered an item in the sum of causes which have given rise to the lengthened periods of drought that we have experienced in these islands for the last few years. As a matter of reasoning I believe it is generally admitted that such is the case, for ample evidence has been produced by actual experiment to show that draining raises the temperature of the land and the air above it; and if so, it would lessen the chance of the vapour suspended in the atmosphere being condensed. Such observed facts are on record as regards the cutting of forests, *e.g.*, NATURE, vol. iv. p. 51, "Buchan's Meteorology," p. 88, and if my memory does not fail me, some information was given on both these points in a previous volume of NATURE, but I am at present unable to lay my hands upon it, though I have glanced over the pages as well as the index.

If any one will kindly furnish me with the information, which may also be of interest to others, or refer me to a work not difficult of access, I shall be extremely obliged.

THOMAS FAWCETT

### Rainbow

ON Friday, the 8th July, about four P.M., as I was driving across the Bog of Allen, about eight miles from Edenderry, I observed the most brilliant rainbow I have ever beheld either in Europe or India. It appeared in the North, and was low down on the flat horizon, being an arc of 60° with the horizon as its chord. The ends of the bow were nearly due E. and W. The spectrum was intensely vivid. A second bow, imperfect towards the centre, shortly afterwards appeared above it; in perhaps five minutes, the E. end of this upper bow faded, and immediately I perceived for a corresponding length of the true rainbow, bordering the violet, a well defined rim of sea green, this bounded by a band of almost mauve-coloured violet, which shaded off into the indigo sky.

The under-side of the opposite end of the bow (above which the portion of the upper bow was still visible) presented no such appearance.

Next day I learnt that, about the same hour, a thunderstorm burst over Edenderry, and the telegraph clerk, on going to work his instrument, was instantly struck senseless to the ground.

Now, are the two bands beyond (*i.e.* below) the violet often seen? for I never before observed them; or are they due to an unusual amount of electrical tension in the atmosphere?

And is the second incident an unusual occurrence in telegraph offices? F. G. S. P.

### Earthquake in Jamaica

On the night of the 20th inst., at twenty minutes past nine, a sharp shock of earthquake was felt throughout the island, accompanied by a loud rumbling noise. The undulations were from the north.

Robt. Thomson

Cinchona Plantation, Jamaica, August 23

### An Inquiry

Can any of the readers of Nature inform me whether Dr. Anderson, who, in the capacity of naturalist, accompanied Captain Sladen's expedition from Bhamo to Momein in 1868, published any papers upon the scientific results of the journey?

If I am not mistaken, Dr. Anderson was a candidate for the Chair of Natural History in Edinburgh last year, and died before the election.

F. R. S.

## PROF. HAYDEN'S EXPEDITION

We learn from *Harper's Weekly* that advices from Prof. Hayden's exploring expedition in the Yellow Stone Lake region have been received up to the 8th of August last, and contain a satisfactory exhibit of progress. After establishing the depôt of supplies already referred to on the Yellow Stone River, about one hundred and forty miles below the lake, the party ascended the river, and reached the lake on the 26th of July, where they made a new camp. They then began at once to survey the lake with the most approved apparatus, by the aid of a boat taken along for the purpose, and expected to be able to ascertain the exact contour, as well as the principal depths. They had already found several places in the lake where the depth reached three hundred feet, especially along the line of a certain channel-way, and they confidently expected to find soundings of at least five hundred feet.

They explored one of the islands in the lake, which they called Stevenson's Island, and found it to contain about fifteen hundred acres, densely wooded, and with thick and almost impenetrable underbrush, consisting largely of gooseberry and currant bushes, loaded down with ripe fruit. On the threshold only of the wonderful natural phenomena in the way of geysers, boiling springs, &c., described by Lieut. Doane and Governor Langford, they were satisfied that the description fell far short of the reality, which they, indeed, despaired of being able to pourtray, even with the aid of photographic views and sketches.

One of these geysers once in thirty-two hours threw up a column of water about eight feet in diameter to a height of over 200 feet. Hundreds were met with having columns of from ten to fifty feet high, some playing all the time, and others only at intervals. The hottest springs were found to vary in temperature from 188° to 198°, the boiling point at that altitude amounting to about 195°. Most of the springs were ascertained to be divisible into two principal classes, one class containing silica, sulphur, and iron, and the other silica and iron only.

The elevation of the lake was determined to be about 8,500 feet; the altitude of the surrounding peaks being, of course, very much greater. An abundance of trout was found in the waters, of excellent flavour, although much infected with intestinal worms. Game was scarce immediately around the lake; but at a short distance it was said to be very abundant. In addition to the topographical and geological collections, others were being made in all branches of natural history, for a full account of which, as well as a description of the phenomena in general, we shall look with interest to the forthcoming report of the expedition.

## MR. GEORGE HODGE

We greatly regret to record the death, at Seaham Harbour, on the 7th of September, after a short illness, at the age of thirty-eight, of this accomplished naturalist. Although from his retiring and unassuming disposition, little known beyond the naturalist circles of the north, George Hodge realised, as few do realise, the objects of a local naturalist. Living on a portion of the north-east coast, the marine fauna of which was practically uninvestigated when he first settled there, he made its patient and honest study the business of the scanty leisure left him by heavy business responsibilities. How far he succeeded is best evidenced by the Natural History Transactions of Northumberland and Durham, his favourite medium of publication for his careful observations and exquisite drawings of the lower animal forms During a temporary residence in Newcastle, he was honorary secretary to the Natural History Society, and was to the last a valued member of its committee.

Mr. Hodge was a most enthusiastic dredger; if he could get a boat to sea on a fine day (this being even more of a desideratum with him than with most men, as he was rather easily upset), he was perfectly happy. The last two dredging expeditions conducted by the Tyneside Naturalists' Field Club, with grants from the British Association, were undertaken chiefly by him in conjunction with Mr. G. S. Brady. The Echinodermata were his favourite subjects of study, but he was also specially interested in the Zoophytes, Pycnogons, Crustacea, and marine Acari, among all of which he had done good work. To his influence chiefly may be ascribed the establishment of the very useful and flourishing Natural History Club of Seaham Harbour, in whose proceedings he always took great interest.

## ELEMENTARY PRACTICAL GEOMETRY

As "A Father" has asked me by name in your columns what book I can recommend as laying a foundation for the geometry of the future, I suppose I ought to answer him, though I cannot do so by a simple reference to a book. I think the main object of early geometrical teaching should be to lay a foundation of familiar facts on which the science will afterwards be built up. This is unquestionably the true scientific method in teaching all subjects; and as yet it has never, or very rarely, been applied in Geometry. For example, no intelligent teacher of botany will begin by classifying flowers, or teaching theories about their structure; he begins by giving his class flowers to dissect, and then they will know what he is talking about; and teachers of chemistry who follow any other plan find themselves inevitably compelled to cram their pupils. The question is, *how* is this method to be applied in Geometry? I know from various sources that there is a pretty wide-spread conviction that it ought to be so applied, but there is a difficulty that meets teachers at once: there does not seem to be enough of practical geometry that is sufficiently easy for children; and practical geometry, as presented in text books, is dull and uninteresting, as well as rather hard. Still my conviction remains that to lay a foundation of knowledge of facts is as necessary in Geometry as in other sciences, though the range of facts easily observed is somewhat less, and the science becomes much sooner a deductive one. And I think it is admitted that because this observational or practical geometry is wanting in our elementary mathematical teaching, geometry is generally found so difficult, so inexplicably difficult, by boys.

It does not suffice to give a child a box of geometrical solids, and let him handle them and learn their names, though this is not useless. Nor does it suffice to give him a ruler and a pair of compasses to play with; and, in fact, the more we reflect on what is required to give an interest to the observations out of which familiarity with geometrical facts is to spring, the more inevitably, it seems to me, are we led to the conclusion that practical geometry is to be taught not *per se*, but by practical work, by interesting and varied applications of geometrical methods to measure and copy actually existing things.

And this at once suggests that the elementary teaching of practical geometry should consist in the manipulation of measuring instruments, and the calculations based on these measurements, which lie at the foundation of sound scientific work. I believe that all such measurements and calculations and practical constructions as are within the range of a boy, might be profitably laid before him as his work in elementary geometry: and I believe that this kind of training would moreover be of the very highest value in preparing him for good experimental work, and for a sound appreciation of scientific methods and results.

It will be advisable, however, to go into some degree of detail in order to explain my meaning to such as are not familiar with these parts of education; and in doing so, I must confess that I have not tested these details throughout by actual experience. For we have at Rugby to deal with older boys than those whom I am now contemplating, and though we do something of the kind with our younger boys, yet it is not what I should choose if I had the control of boys' education from an earlier age. Any one who wishes to see our actual course of practical geometry can do so by ordering Kitchener's "Geometrical Note-book" from the publisher of NATURE. But it must be understood to be a stopgap, and not a complete work. (I trust the author will forgive me for saying so.)

Let a boy be furnished with a ruler, a triangle (in plain wood), a pair of compasses, and a protractor. Let him have a hard pencil, and be taught how to sharpen it. First let him draw on card a decimetre scale, divided into centimetres, and in part into millimetres. This, of course, he must copy from some trustworthy scale. Insist on this, and on every part of his work being done with great care and perfect neatness, and therefore not in ink.

These are his tools. He must proceed to measure some figures provided him for this purpose; a few triangles, quadrilaterals, &c, in wood, or figures drawn on paper, are sufficient for this purpose. Every one of his measurements is neatly written in a suitable note-book, and the figure to which they apply is drawn (freehand) therein.

The next thing to proceed to is the measurement of angles, and the expression of the result in degrees and minutes, with exercises suggested by Euclid, i. 32, and its corollaries, properties of the circle, &c., which are to be practically verified, the observed results being written down, and compared with the theoretical results.

Then the lad may go on to the practical measurement of areas, beginning of course with a rectangle, which he divides into square centimetres and millimetres; he goes through the practical proof that the area of the triangle is half that of the rectangle of equal altitude on the same base; he proves Euclid, i. 47, and iii. 35; he finds the areas of various polygons of which drawings or models are given him.

Mensuration of solids is next approached, and here probably a few rules will have to be given, by which volumes are calculated from linear measurements. But in all cases the measurements must be made by the boy himself with his compasses and scale. Any one who pleases can show his pupils how to prove the relation between the volumes of pyramids and prisms by weighing models of suitable dimensions. The same method may easily be applied to determine approximately the area of a circle; and in this, as in some other measurements, it will be well to require an estimate of the degree of approximation attained, and a mean to be taken of several measurements.

If more applications are wanting, the use of co-ordinates to express position may be explained, and some examples may be given of their application in simple problems, such as to make a plan of a room or of a garden, the scale being specified; and to copy a drawing, such as the sun with a group of spots. More advanced work to any amount will be offered by projections. The boy would be required to draw the projections of the various regular solids given to him, and perform the usual exercises of geometrical drawing. The construction by ruler and compasses of exact copies of triangles and other figures may be introduced almost anywhere, and a clear statement given of the different data from which a triangle can be constructed.

I wonder what "A Father" and mathematical teachers say to these suggestions. It will be obvious that they do not aim at making a boy a rapid analyst, or an expert problem solver; but I hope it is equally obvious that they are really calculated to make a careful and exact worker, one who shall attach precise meaning to his words, and shall be capable of using his head and hands in combination with one another in practical problems. The method is, moreover, applicable to a class, as well as to an individual pupil, and involves a very trifling expenditure on materials.

When some such course of practical geometry has been gone through, a boy may begin any scientific or deductive geometry; he had better read whatever book is read in the school to which he is going. A boy so prepared will find Euclid easy enough, but rather unaccountably indirect and clumsy; but he may be fortunate enough to be going to a school which has adopted some better arranged text book. In a year or two there will be better modern text books than now exist. Whatever book he reads, he ought to work many examples, and do original work. It is not a bad plan to give him the enunciations alone, and let him discover the proofs as far as he can. Perhaps the best text book now existing is the "Eléments de Géometrie," par Ch. Briot.

My remarks have run to considerable length, much greater than I intended, and I can apologise for it only on the ground that many teachers are thinking of the question handled in it, and that it is only by imparting our notions and our experience to one another that we shall improve our methods. I sincerely hope that "A Father's" letter to you may elicit answers from teachers more experienced and successful than I am.

J. M. WILSON

## *ON FRESH DISCOVERIES OF PLATYCNEMIC MEN IN DENBIGHSHIRE*

IN the course of 1869 I had the good fortune to discover and explore a sepulchral cave at Perthi Chwareu, a farm about fourteen miles north of Corwen, and high up in the region of hills. It contained fifteen or sixteen skeletons, some of which were buried in a sitting posture, of ages varying from infancy upwards, and associated with the broken bones of animals that had been eaten, which belonged to the dog, fox, badger, horned sheep, Celtic shorthorn (*Bos longifrons*), roe, stag, horse, wild boar, and domestic hog. The solitary work of art left behind by man consisted of a flint flake, but there were also fragments of *Mya truncata*, and of mussel and cockle-shells. The cave had been evidently used as a place of habitation by some early race of men, and subsequently as a cemetery, and since the corpses had been deposited on the old inhabited surface, the human bones were more or less intermingled with those of the animals. The entrance had been blocked up with a barrier of large stones, and the interior was filled nearly to the roof with the fine red silt introduced through the crevices in the roof by the rain. The human remains, which were described by Prof. Busk in the essay on the discoveries published in the Journal of the Ethnological Society, January 1871, presented points of very high interest; for while the skulls were rather above than below the present average cranial capacity, some of the leg-bones were remarkable for the peculiar antero-posterior flattening or platycnemism of the shin-bones. And this flattening was caused by the prolongation of the bone in front of the inter-osseous ridge, and not in any great degree by its posterior extension, which is the distinctive feature of the tibiæ found in the caves of Cro Magnon and of Gibraltar. The fact that these platycnemic leg-bones were associated with others of the ordinary forms, and for the most part belonging to the young, and probably to females, while the skulls were of the same type, proves that the character is not one of race, as M. Broca believed, but rather one peculiar to the individual and perhaps to the sex.

Subsequently, I was able to bring this interesting sepulchral cave into relation with remains of man from other parts of Denbighshire, through the courtesy of Mrs. Williams Wynn, in whose possession were a skull and several long bones obtained some years ago in a cave at Cefn, and of the same type as those from Perthi Chwareu. They were found along with the remains of sheep or goat, pig, fox, badger, and stag, and four flint flakes.

A chambered tomb at Cefn, explored in 1869 by Mrs. Williams Wynn, under the care of the Rev. D. R. Thomas and myself, and consisting of a chamber 5ft. wide and 9ft. long, which gradually contracted until it joined a passage 6ft. long and 2ft. wide, contained considerably more than twelve human skeletons buried in the sitting posture, of various ages, and presenting in some cases platycnemic tibiæ. The skulls were of the same type as those from Perthi Chwareu, and some were possessed of peculiar upturned nasal bones that pointed unmistakeably in the direction of a *nez retroussé*. A few small broken flint pebbles were the only foreign matters in the tomb, which was built of large rough slabs of limestone placed on edge, and covered with capstones, and finally buried under a carnedd of loose fragments of limestone. A second chambered tomb with a passage was discovered by the Rev. D. R. Thomas in this carnedd in 1871, which was full of human remains of the same kind as those which I have mentioned, and in addition a few remains of dog, pig, sheep, and roe-deer were found. A broken flint and a round stone were also met with.

The remarkable correspondence of the human remains in the carnedd and the Cefn Cave with those of Perthi Chwareu, proves that the race of men who buried their dead in the tombs is the same as that which used the caves for its last resting-places. The stone chambers, with their low entrance and narrow passage, are indeed caves artificially made, and it is very possible that the idea of making "Ganggraben," or gallery graves, is derived from the ancient custom of living in and burying in caves.

It becomes an interesting question to ascertain the relative age of these cave dwellers and carnedd builders, who have so completely passed out of remembrance that their very name has perished. The evidence offered by the flint flakes may be at once dismissed as being valueless, because they were buried with the dead at least as late as the Roman occupation of Britain, and they merely indicate an antiquity not less than that of the conversion of the Romano-Celts to Christianity, a date which is very hotly contested at the present time. Nor does an appeal to the remains of the animals help us very much. The domestic animals are nearly the same as those still kept in the district, and were introduced into Europe during the Neolithic age. The dog, however, so far as I know, was not usually eaten in Britain in Roman times, although it was an article of food in the Neolithic age in Switzerland and in Yorkshire. The sitting posture also of the corpses points in the Neolithic direction, as well as the correspondence of the skulls with those termed "river bed" by Prof. Huxley, and others which are undoubtedly of the newer stone age. On this evidence, therefore, the Neolithic date of these ancient dwellers in Denbighshire might be inferred with a high degree of probability.

All doubt, however, on the point has been removed by my discovery of a second cave some 300 yards removed from the first, during the exploration carried on by Mr. Lloyd, of Rhagatt, at the end of last August. Like the first it ran nearly horizontally into the rock, and was blocked up with earth and large masses of stone, and it contained the broken bones of the same animals associated with skeletons of the same type. The corpses had been buried in the sitting posture. During the first day's digging we obtained a beautiful polished axe made of greenstone, and with the edge uninjured by use, which had evidently been interred for some motive or other along with the dead, as well as a few splinters of flint, and one well-defined scraper of the same sort as those which the Eskimos use inserted into a handle of bone or antler. We added also the bear, *U. Arctos*, to the list of animals. And subsequently we met with a remarkably fine flint flake rather over three inches in length, which was in juxtaposition to a small heap of human bones belonging to one skeleton, and rested on the ancient floor of the cave that was indicated by a mortar-like mass of decayed stalagmite. There were also many fragments of a rude black hand-made pottery, composed of clay, worked up with small fragments of stone to prevent fracture while it was being subjected to the fire. Some were nearly an inch in thickness; while others ranged from a quarter to half an inch. It is of the same kind as that which is commonly met with in caves, occurring alike in Kühloch and Gailenreuth, and in Kent's Hole, being very frequently discovered in association with Neolithic remains. After clearing out the horizontal passage for a distance of 10ft. from the entrance, we found that it expanded into a chamber, of the dimensions of which we are unable to form an idea, as it was nearly full up to the roof with *débris*. The floor underneath the decomposed stalagmite consists of a tenacious gray clay, which has never yet yielded any remains either in Yorkshire, Wales, or Somerset, and is probably the result of the melting of the glaciers, the traces of which are abundant in the neighbourhood.

A third cave, running into the rock parallel with the last at a distance of 12ft., contained similar remains of man and the animals, as well as a fourth, which stands about half-way between Perthi Chwareu and those of Rhos-digre.

The interest of this discovery consists in the fact that the group of caves which has been used by a race of herdsmen in long-forgotten times as habitations and burial places, and the tombs at Cefn, must be referred to the Neolithic age. And we can now be certain that those people who have manifested the peculiar flattening forwards of the shin in Denbighshire belong to that age. It is a point also well worthy of note that the cranial capacity of these Neolithic men was not inferior to that of the average civilised man, although the ridges and processes for muscles indicated a greater physical power.

The clue to this remarkable series of caves was afforded by a small box of bones forwarded by Mr. Darwin, and obtained from the *débris* of a refuse heap in a neighbouring ridge, on which the Neolithic men happened to hold their feasts. We have by no means yet exhausted the evidence of a social state now unknown in Europe, which is presented by the caves and tumuli of Denbighshire.

W. Boyd Dawkins

## METEOROLOGY IN AMERICA*

THE attempt to presage great weather phenomena is nothing new. From time immemorial civilised society has sought after a plan for averting the violence of the storm and tempest as anxiously as it has sought to resist the deadly approach of the pestilence and the plague.

The Great Plague of London, historians tell us, carried off in a year about 90,000 persons. This was, however, in the rude and undeveloped condition of medical science, when the metropolis of England had but few hospitals,

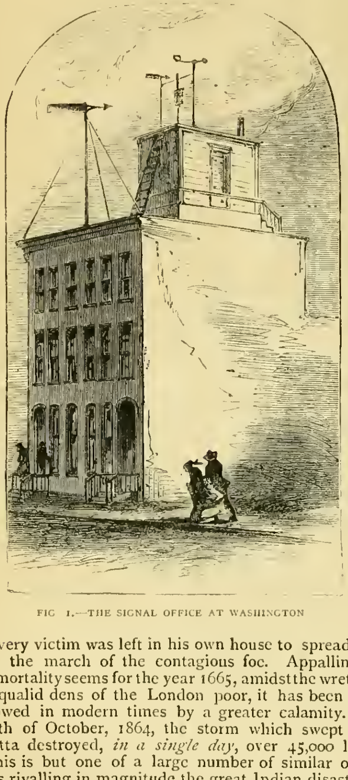

FIG 1.—THE SIGNAL OFFICE AT WASHINGTON

and every victim was left in his own house to spread and speed the march of the contagious foe. Appalling as such mortality seems for the year 1665, amidst the wretched and squalid dens of the London poor, it has been overshadowed in modern times by a greater calamity. On the 5th of October, 1864, the storm which swept over Calcutta destroyed, *in a single day*, over 45,000 lives! Yet this is but one of a large number of similar occurrences rivalling in magnitude the great Indian disaster.

To give forewarning of approaching tempests on the coasts of the Adriatic, the Italian and old Roman castles, as described by an antique writer, had on their bastions pointed rods, to which, as they passed, the guards on duty presented the iron points of their halberts, and whenever they perceived an electric spark to follow, they rang an alarm-bell to warn the farmer and the fisherman of an approaching storm. It is interesting to note that this ancient Italian custom was widely spread over the earth in former ages.

A new element of science has been introduced—the electric telegraph—an invention whose mission of usefulness is destined to unlimited enlargement.

In November 1854, while the Anglo-French fleet was operating in the Black Sea against the stubborn walls of Sebastopol, the tidings flashed across the wires that a mighty tempest had arisen on the western coast of France, and, by the warnings of the barometer, was on its way eastward. The telegram was sent by the French Minister of War, Marshal Vaillant, from Paris, and reached the allied fleet in good time to enable them to put to sea before the cyclone could travel the five hundred leagues of its course, and disperse or destroy the most splendid navies that ever rode those waters. The storm came with a fatal punctuality to the predicted hour. The Crimea, shaken, ravaged, scourged by its fury, presented everywhere a scene of havoc and ruin in the allied camp more fearful than any the fire of all the Russian forts combined could have inflicted. It is perhaps not too much to say that, but for that telegram and its timely storm warning, the congregated navies, far from home and shattered to pieces, could not have sustained the besieging armies, and the event of the great Eastern war might have been different from what it finally was.

So happily, in this instance, did theory (too often despised) blend with fact, that the French War Minister said, "It appears that, by the aid of the electric telegraph and barometric observations, we may be apprised several hours or several days of great atmospheric disturbances, happening at the distance of 1,000 or 1,500 leagues."

So far as we have been able to learn, the first idea of making use of the telegraph for conveying information in regard to the weather, with a view of anticipating changes at any point, occurred to Prof. Henry, the eminent secretary of the Smithsonian Institution, in the year 1847, as in the report of the Institution for that year, page 190 (presented to Congress on the 6th of January, 1848), we find the following paragraph:—

"The present time appears to be peculiarly auspicious for commencing an enterprise of the proposed kind. The citizens of the United States are now scattered over every part of the southern and western portion of North America, and the extended lines of telegraph will furnish a ready means of warning the more northern and eastern observers to be on the look-out for the first appearance of an advancing storm."

Additional references to this subject were made in the reports of 1846 and 1849, in the latter of which we are informed that "successful applications have been made to the presidents of a number of telegraph lines, to allow, at a certain period of the day, the use of their wires for the transmission of meteorological intelligence." Although subsequent reports referred to the intention of the Institution to organise a telegraphic department for its meteorological observations, it was not until 1856, as far as we can ascertain, that observations were actually collected and posted. In the report for 1857 we find that "the Institution is indebted to the national telegraph lines for a series of observations from New Orleans to New York, and as far westward as Cincinnati, which were published in the *Evening Star*."

In the report of 1858 it is announced that "an object of much interest at the Smithsonian building is the daily exhibition, on a large map, of the condition of the weather over a considerable portion of the United States. The reports are received about ten o'clock in the morning, and the changes on the maps are made by temporarily attaching to the several stations pieces of card of different colours, to denote different conditions of the weather as to clearness, cloudiness, rain, or snow. This map is not only of interest to visitors in exhibiting the kind of weather which their friends at a distance are experiencing, but is also of importance in determining at a glance the probable changes which may soon be expected."

* We are very glad to avail ourselves of the courtesy of the Editor of *Harper's New Monthly Magazine*, who has allowed us to reprint, in a modified form, an important article on this subject by Professor T. B. Maury, in which a complete picture of what is being done in America is given. It will be seen that in many points our own Meteorological system is inferior to that now in operation in the States. We should add that the woodcuts have also been placed at our disposal by the Editor of the Magazine referred to.

The report for 1859 contains a list of thirty-nine stations from which daily weather despatches are received, and the report for 1860 refers to forty-five stations. In the report for 1861 Prof. Henry announces that the system has been temporarily discontinued, in consequence of the monopoly of the wires by the military department, and in 1862 it seems to have been again resumed.

It is very evident that to America belongs the credit of first initiating and carrying into successful operation the systematic use of the telegraph for the abovementioned object.

In the year 1857 Lieutenant M. F. Maury, then Superintendent of the National Observatory at Washington, appealed to the public and Congress, through the press, urging the establishment of a storm and weather bureau, and at the same time made an extensive tour through the north-west, addressing the people with a view of rousing public attention to the vast importance of this meteorological system.

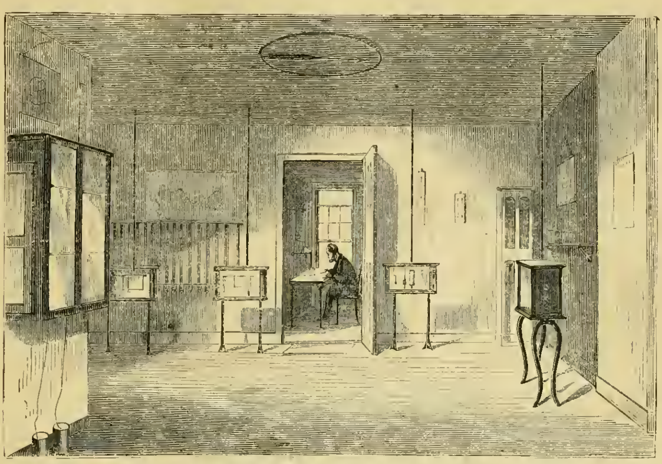

FIG. 2.—INTERIOR OF INSTRUMENT ROOM IN OFFICE OF CHIEF SIGNAL OFFICER

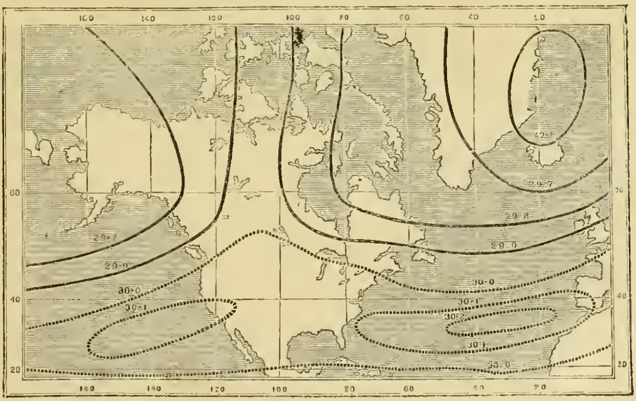

FIG. 3.—MEAN ANNUAL ISOBAROMETRIC LINES FOR THE UNITED STATES

In the Journal of the American Geographical and Statistical Society for 1860, we read that "As long ago as 1851 we find the Superintendent of the National Observatory at Washington urging the extension to the land—for the benefit of farmers, the shipping in our ports, and the industrial pursuits of the country generally—of that system of meteorological co-operation and research which had been so signally beneficial to commerce and navigation at sea. The Brussels Conference endorsed this recommendation. Much stress, in these appeals to Congress and the people, has been laid upon the value of the magnetic telegraph as a meteorological implement; for it was held that by a properly managed system of daily weather reports by telegraph warnings of many, if not most, of the destructive storms which visit our shores or sweep over the land, might be given sufficiently in advance to prevent shipwreck, with many other losses, disasters, and inconveniences to both man and beast" (page 6). The same journal states that the Meteorological Department of the London Board of Trade, under Admiral Fitzroy, was established to co-operate with the suggestion of Lieutenant Maury, which statement is confirmed by the report of the English Board for 1866 (page 17), and also by Admiral Fitzroy himself, in his Weather Book, where he tells (page 49), "from personal knowledge, how cold Maury's views and suggestions were received in this country (England) prior to 1853." The great meteorologist, Alexander Buchan, Secretary of the Scottish Meteorological Society, in his recent work, strikingly states the indebtedness of Europe to the United States for this system: "The establishment of meteorological societies during the last twenty years must be commemorated as contributing in a high degree to the advancement of the science. In this respect the United States stand preeminent."

Less than three years after the occurrence of the famous "Black Sea storm," just mentioned, there appeared for the first time, and in an American paper, a formal proposition for the establishment of a general system of daily weather reports by telegraph, and the utilisation of that great invention for the collection of meteorologic changes at a central office, and the transmission thence of storm warnings to the sea-ports of the American lakes and our Atlantic sea-board.

"Since great storms," says Mr. Thomas B. Butler in his work on the "Atmospheric System and Elements of Prognostication," "have been found to observe pretty well-defined laws, both as respects the motions of the wind and the direction of their progress, we may often recognise such a storm in its progress, and anticipate changes which may succeed during the next few hours. When it is possible to obtain telegraphic reports of the weather from several places in the valley of the Mississippi and its tributaries, we may often predict the approach of a great storm twenty-four hours before its violence is felt at New York."

On the coasts of the kingdom of Italy mariners are forewarned that a storm threatens them by a red flag hoisted on all the towers and light-houses of the principal localities, ranging from Genoa to Palermo, and thence up along the Adriatic. On the most dangerous points of the coast of England, where the fishing-boats and small craft that perform the service of the coast are exposed to formidable gales even during the most promising season, barometers put up by the Meteorological Bureau are at hand to warn the seamen of bad weather. A striking illustration of the importance of storm weather signals was recently furnished (March 8), when a tornado swept over St. Louis, destroying several lives and 1,000,000 dols. worth of property.

In former publications the writer has demonstrated at length the fire-sprinkled paths and tracks of these storms, some of which are generated in the torrid zone, and sweep over the Gulf of Mexico, and thence up the valley of the Mississippi; or, shooting off from the bosom of the Gulf Stream, strike upon the Atlantic coast, and thence commence their march upon the sea-board and central States of the Union. In these published papers the view taken of these tropic-born cyclones is, with some modifications, that announced in 1831, and then substantially demonstrated by Mr. William C. Redfield, of New York, viz. that they rotate round a calm centre of low barometer, in a direction contrary to the hands of a watch in the northern hemisphere, and with the hands of a watch in the southern hemisphere.

The writer was aware, when this view was first publicly sustained by himself, that it was not accepted by all meteorologists.

The observations, of the most reliable and extended character, made within the last few years, go far to show that the storms which descend on low latitudes of the earth from high polar regions are, as the storms of the tropical regions, likewise of a rotary or cyclonical character.

One of the most beautiful illustrations of the law which governs these atmospheric disturbances may be found in the gale which is so celebrated as that in which, on the 25th of October, 1859, the noble steamship *Royal Charter* went down, and several hundred lives were lost, in sight of the island of Anglesea, on the coast of Wales. "The *Royal Charter* gale, so remarkable in its features, and so complete in its illustrations," as Admiral Fitzroy has well remarked, "we may say (from the fact of its having been noted at so many parts of the English coast, and because the storm passed over the middle of the country), is one of the very best to examine which has occurred for some length of time."

The peculiarity of this gale which swept over the deck of the *Charter* was its *intense coldness*, being a *polar* current.

The phenomena of the *Royal Charter* gale, as detailed in Fitzroy's Weather Book and the publications of the day, are important because they furnish the reader with the type to which most American storms, and, indeed, all storms, more or less strictly conform, as geographical or orographical circumstances permit or prevent.

Storms similar in their conditions to that of the *Royal Charter* not unfrequently occur in the United States, especially in the winter, when the conflict of the two currents, the polar and the equatorial, in high latitudes, is marked by sudden transitions in January from mild, moist, and balmy weather to a sudden and fearful cold, below zero. The great snow-storm which visited Chicago on Friday, the 13th of January last, was from the great polar current, and, as is the wont of westerly storms (from the orographic peculiarity of the country), made its way to the Atlantic along the lakes and through the valley of the St. Lawrence.

"With daily telegrams from the Azores and Iceland," Buchan says, "two and often three days' intimation of almost every storm that visits Great Britain could be had." The Iceland telegram would give tidings from the polar air current, and that from the Azores would advertise the movement of the tropical current.

It is highly important that the United States should have telegrams from the Pacific, and from the valley of the Saskatchawan, or some point in British America on the eastern slope of the Rocky Mountains. The importance of reports from the south-west was also fearfully demonstrated in March, during the already mentioned interruption of the Signal Service.

It is due to the cyclone theory, or "law of storms," here and heretofore advanced by the writer, to say that many of the storms which seem to be deviations from the cyclonic law are modified by *interfering cyclones*. This view was formally adopted by the committee of the Meteorological Department of the London Board of Trade. Mr. Stevenson, of Berwickshire, England, as quoted by Fitzroy in the Board of Trade Report for 1862 (p. 33), has some striking observations, founded on his own invaluable labours: "The storms which pass over the British Isles are found generally to act in strict accordance with the cyclonic theory. In many cases, however, this accordance is not so obvious, and the phenomena become highly complicated. This is a result which often happens when two or more cyclones interfere—an event of *very frequent occurrence*. When interferences of this description take place, we have squalls, calms (often accompanied by heavy rains), thunder-storms, great variations in the direction and force of the wind, and much irregularity in the barometric oscillations. These complex results are, however, completely explicable by the cyclonic theory, as I have tested in several instances. A very beautiful and striking example of a compound cyclonic disturbance of the atmosphere at this place was investigated by me in September 1840, and found to be due to the interference of three storms." Mr. Stevenson gives a number of instances of interfering cyclones which confirm this view. The points of *interference*, where two cyclones strike and revolve against each other, are best marked by a peculiarly and *treacherously* fine rain.

It may not inappropriately be added here that the cyclone theory, so strikingly illustrated by the hurricanes of the West Indies, has been demonstrated by Dove to apply to the typhoons of the Indian Ocean and China Seas. And Mr. Thorn has long since shown that the theory holds good for the storms of the Indian Ocean, south of the equator.

## EXHIBITION AT MOSCOW

THE Society of Arts has been exerting itself to ensure that England shall take part in the International Exhibition to take place at Moscow next year. At a recent meeting of the Council a deputation was received, consisting of M. Philip Koroleff, Conseiller d'Etat Actuel, Director of the Moscow Agricultural Academy and President of the Educational Department of the Exhibition, MM. Lvoff, Nicholas Saenger, Secretary of the Society of Friends of Natural Science, and the Rev. Basil E. Popove.

M. Koroleff stated that on June 11, 1872, the Society of Friends of Natural Science, Anthropology, and Ethnography, attached to the Imperial University at Moscow, proposes, with the permission of his Imperial Majesty, to celebrate the 200th anniversary of the birth of Peter the Great, falling on that day, by the opening of a Polytechnic Exhibition in Moscow.

This exhibition, which is intended to form the foundation of a Central Polytechnic Museum in the old capital of Russia, and to present, as far as possible, a complete view of the present relations of Natural Science and Technicology to arts and commerce in Russia, as well as of the progress made by the Russian nation in applied sciences throughout a period of two centuries, since the time of Peter the Great, will, in the opinion of Russian naturalists, form a most suitable tribute to the genius of this great historical character, and communicate a more elevated and especially interesting feature to the festival in his honour.

This exhibition is not, strictly speaking, an international one, for, in accordance with its immediate object, it is proposed to limit the number of nations represented in it. The co-operation of German, French, Belgian, and Dutch exhibitors is hoped for, but the desired sympathy and aid is more particularly requested from England, which has attained, in comparison with other nations, such vast and unsurpassed results in that particular sphere, comprising the applications of science to art and commerce, within the limits of which it is proposed to keep the exhibition.

The Applied Natural Sciences and Technicology will form the two great divisions in the exhibition. It is in these two branches of social life that England has given so great an impulse to its own people, and is able to do the same in the case of other nations.

The exhibition is not a commercial undertaking. Its idea has been started, and is being carried out, by men devoted to science and art, who have accordingly based it, not on the principle of competition, but on that of previous invitation, and selection by competent judges.

In view of the proposed formation of a Polytechnic Museum in Moscow, the Committee will also take the necessary measures that articles considered essential to form parts of a systematic collection in it, should be, if possible, secured for the museum.

## SOLAR RADIATION TEMPERATURES

IN NATURE of August 24th, page 325, you quote the sun and shade temperatures published by Mr. H. Steward and Mr. F. Nunes, of Chiselhurst, and conclude with the following sentence—"Surely there must be an error somewhere. The maximum temperature of Mr. S. or Mr. N. differ by 40° and 50°! Who is to teach or correct amateur meteorologists?" With your permission I will endeavour (1) to explain the possible cause of these discrepancies, (2) to show that it is to "amateur meteorologists" *alone* that we are indebted for (α) all published information on the subject, and (β) for the inauguration of a system of strictly comparable observations on the temperature of the sun.

The difference between a thermometer in sun and shade may I suppose be roughly defined as due to the excess of the heat rays which penetrate the former beyond those with which it can part. A bright, clear, glass bulb filled with mercury is evidently a mirror; it therefore reflects nearly all the heat rays which fall upon it, and therefore reads nearly the same in full blaze of the sun as in perfect shade. Hence it is useless as a measure of solar heat, and so long back as 1835 it was supplanted by a thermometer of which the bulb was blown in black glass. The next improvement was placing the thermometer inside a glass jacket, which was suggested about the year 1860. The reason for this arrangement was very simple; the naked black bulb thermometer varied with every change of force in the wind, and no two instruments were comparable, because it was impossible to secure precisely similar currents over both thermometers. The glass shields have greatly diminished, but not removed, this source of error. The next improvement was to substitute a dull coating of black for the glassy surface which still acted as a partial reflector. Lastly, it was found that the unblackened stem of the thermometer reduced slightly the temperature of the bulb. Hence we arrive at the present form of instrument, a maximum thermometer, with its bulb and part of the stem dull blackened, enclosed in a glass shield or jacket. Most of them are at present made with nearly all the air exhausted from the shield (whence the term vacuum thermometers), but experiments are in progress with non-exhausted jackets, and that point must therefore be left open.

The difference between one of the earliest and one of the latest form of instruments will reach 60° or 70°.

It was supposed that position did not affect these improved instruments, and so (for example) we have that at Greenwich lying on grass, that at Oxford "in a niche in the west front of the observatory about five feet from the ground."* Some experiments made by myself in 1867 showed that the temperatures on grass depended on the state of the grass, whether succulent or parched, and on its length. Hence it was evident that here again comparability was gone. After many experiments by the Rev. F. W. Stow and others, one of his suggestions was adopted, and the thermometer placed on a post at the same height (4 ft.) as everybody (except the Meteorological Committee) places their shade thermometers.

Having thus epitomised the progress of solar temperature observations, I proceed very briefly to the points already mentioned.

(1.) Explanation of the discrepancies.

TEMPERATURE IN SUN DURING AUGUST, 1871

| Observer . . | Mr. Steward | Mr. Nunes | Roy. Ob. | Mr. Symons | |
|---|---|---|---|---|---|
| Locality. . . | —— | Chiselhurst | Greenwich | Camden Square | |
| Mode of Observation . . . | Vac. | On Grass | On Grass | On Grass | 4ft. above ground. |
| | deg. | deg. | deg. | deg. | deg. |
| August 7 . . | 113 | 148·0 | 136·9 | 127·4 | 124·5 |
| ,, 8 . . | 113 | 147·0 | 130·5 | 120·2 | 123·3 |
| ,, 9 . . | 110 | 151·5 | 142·9 | 128·5 | 122·0 |
| ,, 10 . . | 112 | 148·0 | 142·3 | 124·5 | 122·3 |
| ,, 11 . . | 119 | 150·7 | 140·0 | 124·3 | 118·4 |
| ,, 12 . . | 115 | 146·5 | 146·5 | 126·4 | 122·5 |
| ,, 13 . . | 125 | 147·0 | 151·0 | 128·0 | 126·2 |
| Mean . . . | 122·4 | 148·4 | 141·4 | 125·6 | 122·7 |

The instruments and their position at Chiselhurst,

* Radcliffe Met. Obs. 1867, page 4.

Greenwich, and Camden Square, are identical, but the Camden Square grass is by far the most "velvety," and hence partially its much lower temperature. Another and more powerful influence is smoke. Although neither photographers (*e.g.* Mr. F. Bedford) nor artists, (*e.g.* Goodall, R.A., and Cousens, R.A.) deem this a smoky quarter, it is certainly more so than the Royal Observatory, which again is more so than Chiselhurst. Adding 5° to my own readings for the succulence of the grass, we have the following mean values:—Camden 130°6, Greenwich 141°5, Chiselhurst 148°4, whence there appears a regular increase with decrease of smoke. If Mr. Steward's instrument is in the heart of the City, the explanation is complete; but it may differ from the others in construction.

(2.) As this article has become longer than I intended, I will not enter into proof respecting the share in unravelling the inconsistencies of sun temperatures which is due to amateurs, but if required am ready to do so.

Lastly, it is solely to an amateur, the Rev. F. W. Stow, that we are indebted for establishing a small corps of observers in all parts of the British Isles, and some foreign countries, who use only thermometers compared *in the sun*, and mounted on posts so as to be free from terrestrial influences. This is what the private observers are doing, while the public observatories either ignore the subject *in toto*, or follow each its own traditions, and the meteorological societies print indiscriminately readings of thermometers on grass and on posts in jackets, and out of them. G. J. SYMONS

## NOTES

THE arrangements connected with the Eclipse Expedition are making fair progress. The committee have telegraphed to America inviting Prof. Young to take part in the observations. Prof. Zöllner, of Leipzig, has also been asked to join the expedition. We are glad to know that the committee have received the most generous and valuable aid from the directors of the Peninsula and Oriental Steam Navigation Company, and of the British India Telegraph Company. This is as it should be.

IT is hoped that the spectroscope will be brought to bear on Encke's comet this autumn, as the positions will be about as favourable as it is possible for them to be for brightness and a dark sky ground. Mr. Hind informs us that he thought he glimpsed it in Mr. Bishop's refractor a few nights ago.

MR. HIND has communicated a very interesting letter to the *Times* on the solar eclipses of the next twenty years, which we hope shortly to reproduce with some additional facts.

WE have received from the Royal Society of Victoria a prospectus of the proposed Eclipse Expedition from that colony. It states that the Eclipse will be visible as a total Eclipse over a zone about eighty miles wide, passing across the peninsula of Cape York, the Gulf of Carpentaria, and Arnheim's Land to the north of Port Darwin. For the purpose of enabling scientific men in the Australian Colonies to observe the phenomenon, the Royal Society of Victoria proposes to charter a commodious and powerful steamer to carry a party to Cape Sidmouth, or such other point within the limits of totality as may be found most suitable. It is not proposed that the party should be limited to members of the Royal Society, but that it shall be open to the public generally in that and the other colonies. To secure however that no ineligible persons are admitted to the party, the names of all who are desirous to join must be submitted to the Committee appointed for the purpose by the Royal Society. Communications have already been made to the neighbouring Colonies, and many favourable answers have been received. It will be necessary for the expedition to start not later than the last week in November, and it will occupy about three weeks. If possible, arrangements will be made to visit Feejee on the return voyage.

AMONGST the most recent additions to the Zoological Society's living collection, are two specimens of the man-of-war-bird, or fregate-bird (*Fregata aquila*), a well-known denizen of the seas of the Tropics, but one that has never previously reached this country alive. Five of these birds were taken from a breeding-place of this species in the Bay of Fonsecain, Central America, by Captain John M. Dow, C.M.Z.S., of the Panama Railway Company's service, and presented to the society, and two of them have reached the Regent's Park Gardens in excellent health and condition, and may now be seen in one of the compartments of the large Western Aviary. The *Fregata* is an aberrant form of the Pelecanoid type, remarkable for its great powers of flight, and with its structure modified accordingly.

THE new Aquarium at Brighton is now making rapid progress towards completion, some of the tanks being nearly ready to receive their contents. The building is on a very large scale, and will contain upwards of fifty large tanks. Unfortunately, however, no one with any practical knowledge of the working of a large aquarium seems to have been consulted as regards the plans, and there are consequently certain defects in the mode of construction which are likely to interfere with the efficiency of the establishment.

L'ABBÉ MOIGNO, the well-known editor of *Les Mondes*, proposes the establishment of what he terms a "Salle du Progrès," in which an education shall be given which he considers the universities do not supply,—elementary, within the compass of any intelligent mind, and yet of the highest description as to quality. The main feature in the instruction thus given is to be the abundance of experiments and illustrations, whether in any branch of physical or natural science or in art. The illustrative diagrams he proposes to be reproduced on glass by photography, so that they can be packed conveniently in a small box, and then magnified on a large screen by the magic lantern. Admission to the courses at the Salle du Progrès is to be at as low a price as possible, and for the working classes it is to be entirely gratuitous. Under the title of "Daily Bread" (*le pain quotidien*), L'Abbé Moigno proposes also the establishment of a daily journal of religion, politics, science, industry, and literature, intended to promote the regeneration of France by the cultivation of a higher standard than that acknowledged by the bulk of French literature. We wish the Abbé every success, and believe he may do much good by his efforts in these directions. How long are we to wait for scientific lectures for the people in London?

AN International Exhibition of Fruit, open to growers in this and other countries, is to be held in the grounds of the Royal Horticultural Society, at South Kensington, on October 4.

THE recent numbers of the *Révue Scientifique* contain an admirable summary of the most important papers read at the recent meeting of the British Association.

DR. MORTIMER, late head-master of the City of London School, whose death is just recorded, numbered among his pupils, according to the *Pall Mall Gazette*, several men very eminent in science, including Mr. Earnest Hart, and three senior wranglers, Mr. Aldis, Mr. Purkess, and the late Mr. Numa Hartog.

IN Mr. Robert Russell of Pilmuir, who died on the 3rd inst., Scotland has lost one of her most painstaking and scientific meteorologists. A Scottish farmer by birth and training, his whole life was bound up in the agricultural profession. On his favourite study of meteorology, and other subjects connected with scientific agriculture, he was a frequent contributor to various journals, was the author of a work on the Climate and Agriculture of North America, and from 1860 to 1866 was editor of the "Transactions of the Highland Society." He was present at the recent meeting of the British Association, where he read a paper on a branch of meteorology, and was engaged in researches on this subject almost to the time of his death.

THE death is announced of Mr. William P. Turnbull, of Philadelphia, at the age of forty-one. This gentleman was born in Scotland, but had resided for a number of years in Philadelphia, where he was well known as an ornithologist of considerable eminence. He occupied himself for a time in collecting a very complete library of works relating to American ornithology, and also in securing manuscripts, letters, and original drawings of Alexander Wilson. As an author he was known by the publication of two works; the first, a list of the birds of East Lothian, published in Glasgow; the second, a list of the birds of East Pennsylvania and New Jersey, both of them noted for the beauty of their typography and the accuracy of their indications. He was for many years an active member of the Academy of Natural Sciences in Philadelphia, and his loss will be much felt by that institution.

A THIRD enterprise of the Coast Survey of the United States is that of a hydrographic reconnaissance of the Aleutian Islands and the adjacent coast of Alaska, under the direction of Mr. William H. Dall, so well and favourably known for his previous labours in that country, as embodied in his work entitled "Alaska and its Resources." Mr. Dall is now in San Francisco, and expects to leave in a short time for the field of his operations, to be absent a year or more. He is accompanied by Mr. M. W. Harrington, of Ann Arbor, as astronomer, and goes prepared to carry on the work in all its details, including the preparation of charts, soundings of the bottom, determinations of temperature, the chemical constitution of the water, the deep-sea fauna, &c.

*Galignani* says that the French expedition to the North Pole with the *Boreal* is about to be carried out, notwithstanding the death of Captain Lambert. The new enterprise has been undertaken by the Geographical Society of Paris. The vessel is at Havre, quite ready to start, and the new chief of the expedition is also, curiously enough, named Lambert.

MR. OCTAVE PAVÉ, a gentleman of French extraction, and, it is said, formerly a resident of New Orleans, has been lately in San Francisco preparing for his proposed visit to Wrangell's Land—an island of which we have already made mention as having been discovered several years ago by Captain Long, to the north-west of Behring Straits, off the coast of Siberia. Mr. Pavé proposes to go to Cape Yokam as the nearest point, and to embark thence in an India-rubber boat for the region referred to. This boat is so arranged as to serve as a sledge on land and a boat in the water, and much is expected from its performances. Should Wrangell's Land be reached, the subject of proceeding still farther to the north-west will be entertained, with the idea that possibly a route to the pole may be found in that direction.

A LENGTHENED abstract of Messrs. Hull and Traill's paper on the relative ages of the rocks of the Mourne Mountains, appears in the *Geological Magazine* for September. In the report read at the recent meeting of the British Association, epitomised in our issue for August 24, the granite of Slieve Croob is made to consist of quartz, orthoclase, albite, and mica, instead of quartz, orthoclase, and mica. In the report of the Conversazione in the number for August 17, Dr. Gladstone is spoken of as exhibiting experiments on the crystallisation of metals by electricity, instead of experiments on the crystallisation of silver; the crystalline growth of gold, silver, copper, tin, lead, and zinc by electricity was exhibited under the microscope by Mr. P. Braham.

PROF. HOPPE-SEYLER has recently made an important contribution to our knowledge of the processes of putrefaction and disinfection, and his experiments have a significant bearing on Pasteur's researches. The ferments operated upon were entirely such as are formed of chemical insoluble substances. Liebig's altered views on fermentation, putrefaction, and cremacausis are criticised, and Pasteur's assumption that because living organisms are invariably present in putrefying and fermenting fluids, *therefore* those organism are necessary to, and the cause of, the changes going on, is controverted. It is true, he says, that the organisms may contain the ferment, but it is not the less necessary to separate the ferment from the organism in order to form a correct estimate of the question at issue. The article appeared originally in the *Medizinische-chemische Untersuchungen* for July, and an abstract is given in the *Lancet* for August 26th.

A SENSATIONAL story has been reprinted in the English papers from the *Swiss Times*, with respect to the disappearance of several persons while bathing during the present season in the Lake of Wallenstadt, a circumstance attributed to fishes of enormous size in the lake. Dr. Frank Buckland, while not placing implicit faith in the story, suggests that the obnoxious fish may perhaps be specimens of *Silurus glanis* which have strayed from their accustomed habitat in the Lower Danube, or descendants of the monstrous Kaiserlautern pike mentioned by Conrad Gesner, or perhaps huge carps or mythical creatures existing only in the brains of enthusiastic tourists. More explicit information would be very desirable This is not the only marvellous fish story. The *New York Tribune*, of August 24, says that a fish mystery is troubling Council Bluffs. Spoon Lake, a placid sheet of water near that city, has never been known to contain fish "to any extent" until recently, when its waters not only swarmed with myriads of finny monsters, but the surrounding shores are alive with fish. They have come in such enormous numbers that the waves wash them high and dry on the shore, where they lie knee-deep, dead and putrefying. The fish trade in Omaha and Council Bluffs has become prodigious. The fish seem to be greatly astonished at their new surroundings, and stick their heads from the water and open their mouths as if they wanted air. A little boy takes a flat board and wades into the water, and in ten minutes throws out as many fish as a waggon can carry, varying in weight from two to five pounds. People who have lived in the neighbourhood for years declare the phenomenon unprecedented, and various wild theories are put forth in explanation. The prevalent belief is that the swarm came into the lake by a subterranean passage during a late storm, while a few venerable observers contend that the Missouri overflowed its banks and flooded the lake with catfish and perch.

A REPORT upon the Bombardment of the Muséum d'Histoire Naturelle of Paris, by the German Army in January last, is reprinted from the Bulletin de la Société Botanique de France, by M. Aug. Delondre, and possesses a certain historical interest. It contains details of the destruction wrought among the collections, and a list is given of the stove plants which were destroyed either by the direct agency of the shells or by the effect of the cold to which they were exposed by the destruction of the glass and damage to the houses. The Orchids, Pandaneæ, and Cyclantheæ are among the families which have sustained the most serious injury.

A FIELD-DAY in connection with the Newbury District Field Club is to be held on Tuesday, September 19. Highclere and Kingsclere are to be visited, and a lecture will be given during the day by Prof. Rupert Jones, on the Geology of the Kingsclere Valley. The programme includes the visiting of many places of local and antiquarian interest. The first volume of the Transactions of this society is in active preparation, and will shortly be issued.

## THE BRITISH ASSOCIATION MEETING AT EDINBURGH

### SECTION A.

OWING to the large number of papers still remaining in Section A, the Section was divided on Tuesday into two divisions.

*Thermo-Electricity*, by Prof. Tait. The principal object of this research was to verify certain deductions which the author had made from the principle of the Dissipation of Energy. But it had also a practical bearing, viz., to obtain a means of measuring high temperature, such as the melting point of iron or rocks. Sir W. Thomson's great investigation had pointed out for the first time that the whole subject of thermo-electricity depended on something more than the ordinary and well-known Peltiet effect; in fact, he predicted and subsequently verified by ingenious experiments the existence of what he called the specific heat of electricity, which, taken in conjunction with the Peltiet effect, supplied a complete explanation of the known phenomena consistent with the Conservation of Energy. His experiments further showed, if the relation between the electromotive force of a thermo-element and the temperature be represented by a curve in which the abscissæ are proportional to the temperature and the ordinates to the electromotive force, that this curve is symmetrical about an ordinate. But they did not show the form of tbe curve. Some preliminary experiments made by the author of the present paper had shown the curve to be so closely parabolic that he considered the subject worthy of a careful investigation. The difficulty of the research lay not so much in the experiments themselves as in obtaining wires of the infusible metals. He tried circuits of almost every metal which could be obtained in suitable wires, platinum, palladium, iron, &c., and on plotting the results, all the curves appeared to be parabolas. The form of experiment employed was this: As mercurial thermometers were inapplicable to temperatures above 300° C., the simultaneous indications of two separate thermo-electric circuits, the junction wires of which were immersed in the same hot and cold baths, were plotted one as abscissa the other as ordinate. This method gives a very delicate test of the parabolic law; for if the two curves obtained by plotting the two systems separately with the temperatures as abscissæ be exact parabolas, then the curve obtained as above will also be an exact parabola; but, if either of the former differ from the parabolic form, the latter will differ much more. All the results laid down by this method have as yet given very satisfactory approximations to parabolas. The consequences of this parabolic law are curious. If it be admitted, it proves the truth of the author's deduction from the principle of the dissipation of energy, viz., that Thomson's specific heat of electricity, like thermo and electric resistance, varies directly as the absolute temperature. The Peltiet effect is expressed as a parabolic function of the temperature. Another method of combining two thermo-electric circuits is to make the circuits, the junctions being in the same baths, act on a differential galvanometer in opposite directions. It is obvious, from the equations to the parabolas representing the relation of electromotive force to the temperature, or from considering the analogous case of the paths of two projectiles baving the same horizontal velocity, and projected from the same point, that, by properly adjusting once for all temperatures the resistances of the two circuits, the term depending on the square of the temperature may be eliminated, and the galvanometer indication will be proportional to the difference of temperatures of the two baths.

Sir W. Thomson, speaking of the importance of Prof. Tait's research, pointed out that its results were in direct contradiction to the statements made in books on the subject.

*A New Reflector for Lighthouses*, by T. Stevenson.

The novelty of this lantern, which was a holophotal apparatus with a spherical and approximately paraboloidal mirror, combined with a Fresnel's lens, consisted in replacing the speculum which usually forms the mirror by plate-glass facets, silvered, like a looking-glass, at the back.

The paraboloidal mirror consisted of three annular facets formed by the revolution of a circular arc about a horizontal axis, the circular arc osculating the generating parabola to which it was required to approximate.

The facets were prepared at Messrs. Chance's works near Birmingham, by first bending plate glass into the approximate form, and then grinding and polishing the facets in the same way as the prisms of dioptric apparatus are ground and polished. The joints between the facets should be made good with Canada balsam; they can then be hardly seen, as the refractive index of the balsam is nearly the same as that of the glass.

It is sometimes necessary to construct a holophote which shall illumine a given arc of the horizon instead of the whole, as in the ordinary dioptric fixed light, or a very small portion alone, as in the revolving light; in fact, to send out a wedge-shaped beam diverging horizontally. This is usually effected by passing a portion of the light through a second system of prisms to deviate it into the required direction, after being rendered parallel in the usual way. But such a beam could be produced by a single reflection from a suitable surface, and, though it would be impossible to construct such a surface in speculum metal, facets of glass could be made osculating the surface, and be silvered at the back. Mr. Stevenson had asked Prof. Tait for formulæ to calculate the form of surface required.

Prof. Tait exhibited the formulæ he had obtained, and pointed out that such questions could be easily solved by quaternions, and that this calculus was peculiarly adapted to solve the problems of geometrical optics.

*A Method of Estimating the Distance of Fixed Stars*, by Mr. Fox Talbot.

The author did not know whether he had been anticipated in the proposal of the following method, and he left that to be determined by those better acquainted with practical astronomy.

The principle of the method may be seen from a simple example. Suppose the plane of the orbit of a binary system to pass through the sun, *i.e.* that the observer is in the plane of the orbit, and that in the spectra of the individual stars there are lines belonging to the same element. The spectra of the two stars taken through the same slit should be observed and compared. When the stars appear in the same straight line, it is clear that their velocities relative to the earth are the same, since both are moving perpendicularly to the line of vision; the lines from the two stars will therefore coincide. But when their apparent distance from each other is greatest, the difference of their velocities relative to the observer is equal to the velocity of either star in its orbit about the other. This difference of relative velocity will produce a displacement of the lines, which displacement may be observed and even measured. This will give us the value of that velocity. But we also know the periodic time. We have then at once the circumference and thence the diameter of the orbit. We know the greatest angular distance between the stars; we have then the distance of the stars from the earth.

*Report of the Committee on Underground Temperature*, by Prof. Everett, D.C.L.

The intended boring at the bottom of Rosebridge Colliery has not been executed, recent occurrences in a neighbouring pit having given the manager reason to fear an irruption of water in the event of such a boring being made. Careful observations of temperature have been taken by the engineers of the Alpine tunnel under Mont Frejus (commonly called the Mont Cenis tunnel). The highest temperature in the rocks excavated was found directly under the crest of the mountain, which is quite a mile overhead. The temperature was 85·1° Fahr., the mean annual temperature of the crest over it being estimated, from comparison with observed temperatures at both higher and lower levels (San Theodule and Turin), at 27·3° Fahr. Assuming this estimate to be correct, the increase of temperature downwards is at the rate of 1° in 93 feet, which, by applying a conjectural correction for the convexity of the surface, is reduced to about 1° in 81 feet as the corresponding rate under a level surface. This is about the rate at Dukenfield Colliery, and is much slower than the average rate observed elsewhere. The rocks are extremely uniform, highly metamorphosed, and inclined at a steep angle. They contain silica as a very large ingredient. They are not faulted to any extent, and are very free from water. It is proposed to sink two bores, to the depth of from 50 to 100 feet, at the summit, and another point of the surface over the tunnel, with the view of removing the uncertainty which at present exists as to the surface-temperature. Mr. G. J. Symons has repeated his observations at every fiftieth foot of depth in the water of the Kentish Town well, between the depths of 350 and 1,100 feet, the surface of the water being at the depth of about 210 feet. The observations which have been repeated are thus completely free from the disturbing effect of seasonal changes. The results obtained agree closely with those previously found, and show between these depths a rate of 1° in 54 feet, which, from the estimated mean temperature of the surface of the ground, appears to be also very approximately the mean rate for the whole 1,100 feet. The soil, from 325 to 910 feet of depth, consists mainly of chalk and marl, and shows a mean rate of 1° in 56 feet. From 910 to 1,100 feet, it consists of sandy marl, sand, and clay, and shows a mean increase of 1° in 54 feet. The former of these is in remarkably close agreement with very trustworthy determinations made by Walferden from observations in the chalk of the Paris basin. These are as follows:—Puits de Grenelle, Paris, depth, 400 metres; rate, 1° F. in 56·9 feet. Well at Military School, Paris, depth, 173 metres; rate, 1° F. in 56·2 feet. Well at St. André, 50 miles west of Paris, depth, 263 metres; rate, 1° F. in 56·4 feet. General Helmersen, of the Mining College, St. Petersburg, informs the secretary that, in sinking a well to the depth of 540 feet at Yakoutsk in Siberia, the soil was found to be frozen, probably to the depth of 700 feet. The rate of increase from 100 to 540 feet was 1° F. in 52 feet. A new pattern of thermometer has recently been constructed for the committee, which promises to be of great service. It is a maximum thermometer on Negretti's principle, adapted to be used in a vertical position with the bulb at the top. The contraction in the neck prevents mercury from passing into the stem when the instrument receives moderate concussions. Before taking a reading, the instrument must be gently inclined so as to allow all the mercury in the stem to run together into one column near the neck. On restoring the thermometer to the erect position, the united column will flow to the other end of the tube (that is, the end furthest from the bulb), and it is from this end that the gradations begin. It is set for a fresh observation by holding it in the inverted position, and tapping it on the palm of the hand. This instrument, like that heretofore used by the committee, is protected against pressure by an outer case of glass, hermetically sealed.

## SECTION G.

At the opening of this Section on Monday, after disposing of Mr. Symons' Report of the Rainfall Committee, a paper *On a New Form of Steam Blast*, was communicated by Mr. W. Siemens, F.R.S. The new blast is employed for the movement of air in the pneumatic tubes connected with the central telegraph station in London. It is said to cost only 40*l.*, and will do the same work as an engine which costs 2,000*l.*

Mr. Stevenson then read a paper describing what he termed a *Thermometer of Translation*. It consists of an expansible body, with needle point at its upper end, and when expanded by the sun is fixed at its upper end, by a needle point catching into fine teeth cut in a sheet of glass or other material of small expansibility pressed below. The end being fixed, the contraction raises the centre of gravity at the bar. In this way the daily march or creep of the bar chronicles the changes of temperature.

Mr. Michael Scott, in a paper *On Improved Ships of War*, proposed to construct a ship of war, which should combine cruising and fighting qualities, by adopting the turret system for the guns, and having the ship so designed that the free board could be reduced by sinking her deep in the water through the filling of certain cisterns. The masts were to be of telescopic construction.

Captain Jenkin, C.B., said this was a subject of grave importance to the country. He thought, however, it would be necessary before giving an opinion on this paper, that they should be able to understand what, in the proposed ship, was intended to be below the water. All he (the speaker) could see was what was to be above the water, and consequently he could not give an opinion in a professional point of view on the plans produced. While Mr. Scott was entitled to their thanks for what he had produced, he thought it would be necessary for him to bring forward models of the ships he proposed to construct.

A paper *On an Apparatus for Working Torpedoes* was read by Mr. Philip Braham. The author proposed to propel or "shoot" torpedoes against the enemy's ship by means of compressed air, and under the surface of the water.

On Tuesday, the reports on the treatment and utilisation of sewage read in the Chemical Section on Monday having been submitted, a paper on *The Carbon Closet System* was read by Mr. E. C. C. Stanford, F.C.S. In the most populous places the carbon system, he held, was the most practicable, the most healthful, and the most profitable means that could be used in getting rid of the sewage; and he thought that the system of the future must be some modification of the dry system such as that which he had brought before the section. An interesting discussion followed the reading of this paper.

The committee appointed to consider the various plans proposed for legislating on the subject of Steam-boiler Explosions, with a view to their prevention, presented an interim report, in which they stated that the Parliamentary report having been so recently published, there had not been time for its due consideration, or for the committee to meet and confer thereon, and they had postponed entering into the subject on the present occasion.

Mr. Lavington E. Fletcher, C.E., in a paper on *Steam-boiler Legislation*, stated that the Parliamentary Committee had arrived at the three following conclusions, viz.:—(1) That the majority of explosions arise from negligence, either as regards original construction, inattention of users or their servants, neglect of proper repairs, and absence of proper and necessary fittings; (2) that on the occurrence of explosions, a complete investigation of the cause of the catastrophe should be promoted by the appointment of a scientific assessor to assist the coroner; and (3) that reports of each investigation should be presented to Parliament. These three conclusions, it was considered, formed a foundation from which a superstructure would spring in course of time which must eradicate steam-boiler explosions. What the precise character of that superstructure should be is a question on which opinions may differ. Some—among whom are the Parliamentary Committee as already explained—prefer a system of pains and penalties to be inflicted on the steam user in the event of his allowing his boiler to give rise to an explosion. Others prefer a system of direct prevention by the enforcement of inspection on the following general basis:—They would recommend a national system of periodical inspection enforced but not administered by the Government, that administration being committed to the steam users themselves, with a due infusion of *ex officio* representatives of the public. For this purpose they propose that steam users should be aggregated into as many district corporations as might be found desirable, boards of control, empowered to carry out the inspections, and levy such rates upon the steam users as might be necessary for the conduct of the service, being appointed by the popular election of the steam users in each district, the different boards being affiliated by means of an annual conference, in order to promote the harmonious working of the whole system. Its advocates consider that in this way a system of national inspection might be mildly, but, at the same time, firmly administered, and that it would then not only prevent the majority of steam boiler explosions, but prove of great assistance to steam users in the management of their boilers. That it would be the means of disseminating much valuable information. That it would promote improvements. That it would raise the standard of boiler engineering, and prove a national gain. It frequently happened, the paper went on to say, that on the occurrence of disastrous explosions, boiler owners were quite unable to compensate those who had been injured. Such was the case last year at Liverpool, where an explosion occurred at a small iron-foundry, in October, killing four persons, laying the foundry in ruins, smashing in some of the surrounding dwelling-houses, and spreading a vast amount of devastation all round. The owners of the boiler, which had been picked up second-hand, and was a little worn-out thing, were two working men, who but a short time before the explosion had been acting as journeymen. They were possessed of little or no capital, and were rendered penniless by the disaster. Another very similar case, though much more serious, occurred at Bingley in June 1869, where as many as fifteen persons were killed, and thirty-one others severely injured by the explosion of a boiler at a bobbin turnery. In this case the user of the boiler was only a tenant, and, judging from the ruined appearance of the premises after the explosion, any attempt to gain compensation for the loss of fifteen lives and thirty-one cases of serious personal injury would be absolutely futile. The plan of imposing a fixed minimum penalty would tend somewhat to meet this difficulty, as the surplus of one would correct the deficit of another, and in this way a compensation fund might be established for the benefit of the sufferers. This definite minimum penalty would tend to meet the present tendency of boiler owners to seek to purchase indemnities from insurance companies in the event of explosions, rather than competent inspection to prevent these catastrophes, since, if the penalty were made sufficiently high, it would pay an insurance company as well to make inspections and prevent explosions, as to adopt comparatively little inspection, permit occasional if not frequent explosions, and pay compensation.

A short discussion took place, in which it was argued that increased security for the proper inspection and manufacture of boilers should be obtained.

The last paper read in this Section was *On the Rainfall of Scotland*, and contributed by Mr. Buchan. The paper was illustrated by a map of Scotland, showing the average annual rainfall of about 200 places, many of the averages being deduced from observations carried on through a long series of years. The map brought out the large rainfall in the west as compared with the east, a difference which was strongly marked even in the group of the Orkney Islands. The average rainfall in the west, at stations removed from the influence of hills, was from thirty-six to about forty inches; but on the east coast, in similar situations, the rainfall was as low as from twenty-four to twenty-eight inches. In casting the eye towards the watershed of the country running north and south, it was seen that in ascending towards it from the west, there occurred a rapid, but by no means uniform, increase; and in descending from it towards the east, a rapid, but by no means uniform, decrease. The largest rainfalls occur almost wholly among the hills forming the watershed north of the Forth and Clyde. The places characterised by the heaviest rainfall are, so far as observation has yet enabled us to determine, the following:—Glencoe, immediately under Rest-and-be Thankful, 128 inches annually; Ardlin, head of Loch Lomond, 115 inches; Bridge of Orchy, 110 inches; Tyndrum, 104 inches; Glen Quoich, in the south-west of Invernesshire, 102 inches; and Portree, Skye, 101 inches. At no great distance from several of these places the rainfall is by no means excessive, thus pointing out an enormous difference of climate between places not far apart. Along the watershed of that part of Scotland which lies south of the Forth and Clyde, no such excessive rainfall occurs, the highest hitherto observed being only 71 inches, Ettrick Pen Top, 2,268 feet high. This diminished rainfall in the south as compared with places similarly situated further north is doubtless due to the mountains of Ireland to the south-west, which partially drain the rain-bringing winds of their moisture before they arrive at these parts of Great Britain. The distribution of the rainfall is very instructive in many districts, as in the valley of the Forth, from the head of Loch Katrine to North Berwick, where the amount varies from ninety-one to twenty-four inches; in Clydesdale, where the quantity is greatest at the head and foot of the valley respectively, being considerably less at intermediate places; and along Loch Linnhe and through the Caledonian Valley, where the variations of the rainfall were excessive. In all these districts, as well as elsewhere, many cases might be referred to, showing that the amount of the rainfall is very far from being determined by mere height. In truth, it is to local considerations we must chiefly look for an explanation of the mode in which rain is distributed over any district; and hence, in estimating the rainfall, particularly in hilly districts, no dogmatic rule can be laid down which can approximate in accuracy the result arrived at by one skilled in such matters and who is at the same time well acquainted with the district. If those districts were shaded off in which the rainfall did not exceed 30 in annually, the great grain-producing district of Scotland would be indicated; and it was interesting to note that in those districts which produced the best wheat the rainfall was lower than elsewhere, being in many places as low as 24 in. There are about fifty places at which observations have been made for periods varying from twenty to forty-five years. On comparing the average of the three driest years at each of these places with its average, the amount of the deficiency is found to vary exceedingly, being as much as one-third in some places and as little as one-ninth in others.

Mr. Bateman expressed his great satisfaction that Mr. Buchan had stuck to facts instead of theorising. He went on to say that the correction given for the three dry years was just as fallacious as the correction for altitude. A proposed rule had been laid down of an increase in the rainfall of one and a half per cent. for each 100 feet of elevation, but subsequently that one and a half had been changed to two and a half. Mr. Bateman said that the formula for the three dry years was that for the height. In point of fact it was impossible to lay down any rule that would give accurate results.

Mr. Symons drew attention to the fact that the ratio of the three dry years depended very much on the length of period involved, and showed that the epoch of dry years in the instances quoted varied almost precisely with the length of those averages.

Mr. Bateman and Mr. Symons spoke strongly in favour of the work performed by Mr. Buchan, and of the excellent operations of the Scottish Meteorological Society.

Mr. Milne Home made a few appropriate remarks in reply.

## *MAXIMUM VELOCITY OF METEORIC STONES REACHING THE SURFACE OF THE EARTH*

IN Prof. Nordenskjöld's account of the Aerolitic Shower which took place near Hessle, in Sweden, on the 1st of January, 1869, he mentions, as a remarkable fact, that stones weighing two pounds, which struck the ice of the Larsta-Viken, failed to penetrate, making holes only three or four inches deep in the ice and rebounding. (Vide the *Academy*, Dec. 15, 1870.)

The small velocity retained by these stones at the time of striking the earth is, doubtless, owing to the resistance of the air, and, consequently, is not an indication of the velocity which they had upon entering the atmosphere.

Stones thus penetrating the atmosphere from interplanetary space, would be moving in a resisting medium under the joint influence of their original velocity of translation and the constant action of terrestrial gravity. In the case of small masses, the resistance of the medium would very speedily produce retarded motion; and before traversing twenty or thirty miles of air, they would probably move with a velocity approximating uniformity, and under the action of gravity alone. In other words, they would gradually lose their original velocity of translation, and, descending, nearly or quite vertically, under the action of gravity, would ultimately attain a maximum velocity under the opposing influences of the resisting and accelerating forces, and then descend to the earth with this uniform velocity.

Thus, for example, a rifle bullet shot obliquely into deep water, would very soon lose the horizontal component of its velocity, while the vertical motion would be so rapidly retarded that, at a comparatively short distance below the surface of the water, it would begin to descend vertically with the very moderate uniform motion resulting from the resistance of the liquid and the constant action of gravity.

In like manner, no matter how great the velocity with which a meteoric stone enters the atmosphere, the enormous resistance which it encounters must operate ultimately to produce a similar result, although, in some cases of oblique incidence, the horizontal component of velocity is not entirely lost before reaching the earth.

It is well known that this maximum or limiting velocity of a falling body is attained when the required velocity is such that the resistance is at each instant equal to the weight of the moving body. In the case of small masses moving in the air, it may be shown that this velocity is quite moderate.

The principles of dynamics furnish the means of determining the resistance of a given body moving in a medium of given density when the size of the body and its velocity are known.

Let $A$ = area of cross-section of body at right angles to direction of motion.
$D$ = weight of unit volume of medium (air).
$v$ = velocity of moving body (meteoric stone).
$g$ = acceleration by gravity in a unit of time.
$k$ = a constant co-efficient, deduced from experiment, depending upon the shape of the moving body.

Then we have,

$$\text{Resistance} = k \times A \times D \times \frac{v^2}{2g}$$

Hence, by the conditions under which, as above given, $v$ becomes a maximum, if $w$ = weight of the moving body or stone, we have,

$$k \times A \times D \times \frac{v^2}{2g} = w \therefore v = \sqrt{\frac{2g \times w}{k \times A \times D}} \qquad (a)$$

Applying this formula ($a$) to the case of a meteoric stone weighing two pounds moving in the air:—let us assume it to be a cubical mass, having a specific gravity of 3 in relation to water as unity. Then its volume = 18·482 cubic inches, and the area of one of its faces = 6·9903 square inches = 0·048544 square feet. Hence, assuming the resistance to act at right angles to the face of the cube, and taking the pound and foot as units, we have—

$A$ = 0·048544 square feet.
$D$ = 0·080728 pounds = weight of cubic foot of air at 0° C.
$w$ = 2 ,, = ,, ,, stone.
$g$ = 32·1928 feet per second.
$v$ = required maximum velocity in feet per second.

Hence by formula ($a$):—

$$v = \sqrt{\frac{2 \times 32.1928 \times 2}{k \times 0.048544 \times 0.0807288}} = \sqrt{\frac{128.77}{k \times 0.0039189}}$$

$$= \sqrt{\frac{32859}{k}}.$$

Or $v = \frac{181.27}{\sqrt{k}}$ in feet per second.

Assuming $k = 1.3$,* we obtain,

$v = 158.99$ ($= 159$) feet per second, as the maximum velocity attained by such a stone in falling to the earth. This velocity does not exceed *one-tenth* of the initial velocity of a rifle bullet. And, as the *penetrating power* of a given projectile is proportional to the *square* of its velocity, its power of penetrating the ice would only be *one-hundredth part as great* as that of a projectile of similar mass and dimensions moving at the rate of a rifle bullet. Hence *we need not be surprised that the ice was not penetrated more than three or four inches.*

If the same mass of stone (two pounds) were *spherical* in form instead of *cubical*, its diameter would be 3·2803 inches = 0·27336 feet, and $A = 0.058689$ square feet. In this case, we may assume $k = 0.7$ † Hence, by the formula ($a$), we obtain,

$v = 197.05$ feet per second: so that in this case likewise its velocity would be quite low, and its penetrating power *very insignificant.*

Of course, in the case of *large* meteoric stones the value of $v$ would be much greater.

JOHN LE CONTE

## ASTRONOMY

### The Solar Eclipse of 1868 ‡

ADEN was chosen as the observing-station because from the general nature of its climate it was thought that a satisfactory view of the phenomena that took place during the eclipse might fairly be expected, and also because, as it was far removed from the stations of the French and English expeditions, any observations taken there would prove of considerable importance. The observers were Prof. Edmund Weiss (the leader) Prof. Oppolzer, and J. Rziha, already known for his observations of the eclipse of 1867.

On the morning of the eclipse (Aug. 17) the state of the atmosphere proved unfortunately anything but favourable for astronomical investigation, owing to the presence of a great amount of cloudiness. According to Oppolzer the beginning of the totality was $18^h\ 29^m\ 30^s.0$ (Aden mean time), the end $18^h\ 33^m\ 24^s.6$.

A few moments before the total disappearance of the sun, Weiss saw on it a beautiful carmine red border or streak, in the middle of which arose a similarly-coloured complicated prominence (No. 1) which lasted for a few seconds. Half a minute later ($18^h\ 30^m\ 2^s$) a second prominence (No. 2) appeared, long, thin, and in shape resembling a slightly bent finger; nearly two minutes later ($18^h\ 31^m\ 58^s$) he noticed a third smaller, hill-shaped, or conical prominence (No. 3). Just at the end of the totality another beautiful red border appeared, on the outer edge of which was a gleam of deep blue, most intense at the point of junction with the red, and rapidly fading away on the outside into the background.

Some English officers stationed a short way off also noticed the first two prominences (which they say were visible to the naked eye) and the red border at the end of the totality, but they failed to see prominence No. 3, perhaps for want of sufficiently powerful or properly adapted instruments.

Oppolzer's observations coincided with those of Weiss, except that he failed to see prominence No. 3 on account of the interference of passing clouds, though he suspected its presence from a certain red appearance at the spot indicated by Weiss. Satisfactory observations of the corona were rendered impossible by the state of the atmosphere.

Rziha's part was confined to the spectrum, and his account is that simultaneously with the disappearance of the last sunbeam. Fraunhofer's lines entirely vanished, the spectrum became continuous and remained so to the end of the totality. All his efforts to detect any reversal of the lines proved ineffectual. Just before the reappearance of the sun, thin clouds intervened and hid the greater part of the corona, so that the principal sources of light were the red border and the prominences. At this moment the more refrangible rays from the green disappeared gradually, and only the red end of the spectrum remained, consisting of deep red, carmine, orange, feeble yellow, and the faintest possible tinge of green, at the same time this remaining part became discontinuous owing to the appearance of dark lines in it, which did not, however, coincide with any of the principal lines of the ordinary spectrum. The disappearance of the dark lines, Rziha seems to think, would connect the corona with a solar atmosphere; and he suggests that the lines or streaks which appeared afterwards were due to absorption by the water-vapour of our own atmosphere.

## PHYSICS

### On a Quantitative Method of Testing a "Telegraph Earth," by W. E. Ayrton*

THE method used up to the present time for testing a telegraph earth has been *qualitative* only. As, however, the electrical condition of every "earth" is of great practical importance, it is necessary that some accurate *quantitative* method should be devised, in order that every telegraph office may ascertain whether the resistance of their earth is higher or lower than the maximum resistance allowed. The principal difficulty met with is that, if the resistance between two earths be measured successively with positive and negative currents, the same result is not obtained. Consequently the ordinary law for a Wheatstone's Bridge, or Differential Galvanometer, would not hold true. This difficulty, however, has been overcome in this paper, and formulæ are developed suitable for a Wheatstone's Bridge, a Differential Galvanometer, or a Galvanometer of which the law of the deflections is known.

The details of some experiments are also given, and a particular instance is mentioned in which a much better "earth" was obtained by burying the plate in the upper stratum of soil than by burying it much deeper, on account of a bed of sandstone that existed at about fifteen feet below the surface.

## SCIENTIFIC SERIALS

THE *American Naturalist* for September commences with an article by Mr. W. J. Hays, entitled "Notes on the range of some of the Animals in America at the time of the arrival of the white men." The moose, now almost entirely driven out of the United States, was, at the time of the first European settlement, found as far south as New York city; the range of the carriboo was not more extensive then than it is now, although fossil remains have been found as far south as the Ohio; the musk-ox is not mentioned by the early travellers; but the common deer (*Cervus virginianus* and *C. campestris*) was everywhere represented as existing in incredible numbers. The Wapiti deer was found all along the coast from Canada to the Gulf of Mexico; the bison (improperly called the buffalo by the early settlers), also ranged along the coast from the valley of the Connecticut to Florida, and roamed over the entire country now known as the United States, and extending as far north as the sixtieth parallel in British America. Mr. Hays reckons that at the present time not fewer than half-a-million bisons are annually destroyed by the hand of man. The red fox existed in America before the advent of the white man, in addition to the gray species, notwithstanding assertions to the contrary; wolves were everywhere abundant, as also was the beaver; the jaguar, not now found east of Texas, occurred in the mountains of North Carolina as recently as 1737; the dog was found in all parts of the country; and, from the descriptions, must have been of the same species as those now found with the Indians of the plain. The only other original article in the number is "On the Food and Habits of some of our Marine Fishes" by Prof. A. E. Verrill.

THE most important paper in the *Journal of Botany* for September is an article by Mr J. G. Baker "On the Dispersion of Montane Plants over the Hills of the North of England." Mr.

* For *cubes* moving in *water* the experiments of Du Buat and Duchemin give $k = 1.28$.

† For *spheres* moving in *air* the experiments of Robins and Hutton give for velocities:—

$$v = 3.28,\ 16.4,\ \ 82,\ 328 \text{ feet per second.}$$
$$k = 0.59,\ 0.63,\ 0.67,\ 0.71.$$

‡ *Astronomische Nachrichten*, No. 1836-1837: "Account of the observations of the Austrian Expedition sent to Aden to watch the total solar eclipse of 1868."

* From the Proceedings of the Asiatic Society of Bengal.

Baker divides the sub-mountainous regions of the North of England into four distinct ranges:—the Porphyritic Hills, including the Cheviots; the Carboniferous Hills, or that portion of the Pennine chain which falls between the Tyne and the Wharfe; the Slate Hills of the Westmoreland and Cumberland Lake district; and the Oolitic Hills of North-east Yorkshire. The range of each indigenous species of sub alpine plant is traced, and a comparative table given of the number of species found in each range; those in the Slate and Carboniferous districts more than doubling those in the Porphyry and Oolite. Dr. Trimen contributes a description, with plate, of *Siler trilobum*, an alleged new British plant, the genuinely indigenous character of which is, however, questioned.

## *SOCIETIES AND ACADEMIES*

### Paris

Academie des Sciences, September 4.—M. Faye in the chair.—M. Bertrand read a long note on the theory of the moon. The learned member supported the same theory as the one advocated by M. Biot, and contended that the third of the three great lunar inequalities had been discovered by Ptolemy. M. Sedillot, a learned Arabic scholar, is of the contrary opinion, and his views were successively supported by M. Leverrier and M. Chasles.—Father Secchi sent from Rome the result of observations made with the same instruments as those he had previously made, and which, having been executed up to the 26th of August, during a period of magnificent weather, are of special interest. An engraving, which is necessary for their comprehension, is sent for publication in the *Comptes Rendus*. It shows the sun as it was observed on the 23d of July from 8.30 to 9.40 at Rome; protuberances are seen, as exhibited by spectroscopic observations. They are very great in number as well as in dimension. Father Secchi says that he is now engaged in making special observation, to ascertain if variations observed in the number and form of protuberances are not connected with variations in the photosphere, and, consequently, with the diameter of the sun itself. Father Secchi states, moreover, that it is very difficult to account for the differences between several accurate observers, which amount to two seconds, without some elements of the kind. He said that he will very soon send a special paper on this important matter. M. Faye, in review of the paper, said that great discoveries might be expected very shortly relatively to the constitution of the sun, and that the labours of various contributors to this subject might be very shortly rewarded.—M. Chasles presented to the Academy a book sent by M. Quetelet, Director of the Royal Brussels Observatory, entitled "Anthropometry; or Measurement of the different Human Faculties." The author tried to find curves, exhibiting not only muscular force and vitality, but also the vices and virtues, representing the period of life at which the proclivities are the strongest for murder, robbery, love, &c. &c.—M. de Tastes sent a paper "On the Atmospheric Currents of the Northern Hemisphere," which, if grounded on facts, may help to prognosticate the weather. He supposes that the polar regions are not disturbed by storms, but are regions of calm. In order to support his theory he quotes a letter sent to the Academy in July 1870, in which he wrote these words, "the next winter, 1870-71, will be one of the coldest in the whole century."—M. Dumas read a note from MM. Troost and Hautefeuille founded on the memoir published by M. Morren on the spectrum of carbon in the Ann. Phys. et Chemie (4th ser., vol. iv., p. 365), and several other accurate spectroscopic determinations. The authors endeavoured to show that the spectra of carbon, boron, silicon, titanium, and zirconium may be derived from each other by special and gradual modifications indicative of certain secret affinities or rather analogies in the form of the molecules. An analogous series was established by M. Ditte for the spectra of sulphur, selenium, and tellurium. M. Dumas suggested whether each natural chemical family cannot be expected to show some spectroscopic affinities for its different members.

### Melbourne

Royal Society of Victoria, April 17.—Mr. Foord read some notes on the enhydros or water stones, and described the result of experiments upon a sample weighing over 900 grains, which he had obtained through Mr. Ulrich from Mr. Dunn, the mineralogist, who was the discoverer of these stones in Victoria. The sample had for its largest section a form closely approaching an equilateral triangle. It clearly included two separate chambers; in fact, during the experiment it was cloven into two separate water stones; one of which appeared to be quite filled up with quartz crystal; the other containing, besides an inner lining of quartz, a mobile fluid and a bubble of air. To extract the fluid, a fragment was broken from one of the corners of the stone. This disclosed a fine opening or pore in the quartz lining connected with the inner gravity. The fluid was perfectly pellucid, but contained a few minute angular transparent fragments. The fluid was water, slightly mineralised. A single drop evaporated on glass left a slight residue, forming a gummy annular outline, but affording distinct evidence of crystallisation when examined under the microscope. When fifteen drops of the fluid were evaporated on a watch-glass over oil of vitriol, in vacuo, the fluid froze, giving out air bubbles, which vesiculated the icy crust; the ice gradually disappearing left a small residue, nearly white in colour, now crystalline and wrinkled on the surface. A few small crystals and some large ones were observed in the mass. A small crop of beautiful microscopical crystals were obtained on resolution and spontaneous evaporation. Among them were recognised cubic crystals and crystals pertaining to the cubic system. On dissolving the crystals a delicate impress of their form was left, white on a delicately pale yellow ground, as though a deposit of colloidal ferruginous silica remained, with colourless cavities where the crystals had occupied position. On testing the re-dissolved saline matter, it gave a distinct white flocculent precipitate with nitrate of silver, immediately soluble in ammonia. It also gave a granular precipitate with chloride of barium. With ammonia and oxalate of ammonia a very slight granular precipitate was obtained after some time, and with ammonia, chloride of ammonia, and phosphate of soda, relatively abundant crystalline precipitate tufts, or stellate groups of acicular crystals, were obtained. A drop of the fluid examined in the microscope showed vividly the sodium double line, but no indication of potassium, lithium, calcium, nor indeed of any other metal, was apparent. Having thus described the result of his experiment, Mr. Foord endeavoured to show that the wall of the enhydros owed its plane form to crystalline silica deposited along with the amorphous silica, the two together forming the chalcedony. It was also attempted to be shown that there was every gradation from agate, in which the deposit was on the wall of the cavity like a varnish, up to enhydros, in which the cavity was interlaced by planes dividing it into angular chambers. Specimens of thin laminæ were shown, in which the crystalline character of quartz was distinctly observable, resembling the geometric carpet pattern. The President again brought under the notice of the Society the proposed expedition to Cape York in December next, to view the Total Eclipse of the sun, to the preparations for observing which we have already alluded.

## BOOKS RECEIVED

English.—Phrenology; and How to Use it in Analysing Characters: N. Morgan (Longmans).—Hints on Shore Shooting: J. E. Harting (Van Voorst).—Modern Scepticism: C. J. Ellicott (Hodder and Stoughton).—The Phœnix; vol. i., and vol. ii., No. 13.

## CONTENTS

THURSDAY, SEPTEMBER 21, 1871

## THE SMITHSONIAN INSTITUTION

AN excellent article in the *New York Journal of Commerce* makes us acquainted with several points in the organisation of the Smithsonian Institution—that most cosmopolitan of our existing scientific organisations—to which we are anxious to draw attention. It is too much to hope for a similar institution in this country, but it is, nevertheless, interesting to watch the development of the American one under the wise direction of Prof. Henry.

Many years ago the Institution established what is known as the "Smithsonian system of exchanges," whereby, in exchange for those of America, the scientific publications of societies and individuals throughout the civilised world are placed without cost within easy access of the student of science in this country.

This system, promising at its inauguration all that could be expected, rapidly expanded, and during the past few years has yielded an abundance of fruit in the way of a knowledge of the progress of science in every part of the world, far exceeding the anticipation of its most sanguine supporters. Indeed, so eminently beneficial, not only to the scientific, but to the general interests of the country, has the system proved, that Congress a few years since directed the establishment, on a similar plan, of an international exchange of official publications, to be placed under the especial charge of the Institution, and voted, as a basis of operations, for distribution, fifty complete sets of the documents of the Fortieth Congress. These will shortly be ready for transmission by the Institution, in the name of the United States, to such foreign Powers as have either requested to be included in the list of exchange, or in some other way announced their approval of the plan, and are, therefore, known to be prepared to return similar publications of their own Governments respectively. Thus, in time, will be added to the great Congressional library a fund of knowledge which can hardly fail to be of vast importance to this Government.

It is mainly through its system of exchanges that the Institution has accumulated, and will continue to increase, that vast storehouse of scientific truths denominated the "Smithsonian Library," which, numbering about 70,000 volumes (inclusive of pamphlets, &c.), contains, besides complete series of the Transactions of many of the older societies of England and France, which it would now be difficult, if not impossible, to replace, hundreds of works which, like those of the societies in question, can be obtained in no other way than by exchange. On account, however, of the limited space provided for its proper accommodation in the Smithsonian building, but chiefly owing to danger from fire, the Institution a few years ago transferred its library to the Capitol, where, in company with the library of Congress, it still continues to occupy fire-proof roomy quarters. With regard to the library, the secretary of the Institution, in his last printed report, remarks: "The transfer of the Smithsonian Library still continues to be approved by all who have attentively considered the advantages it affords the Institution, the Government, and the public;" . . . that while, "by its transfer the Smithson fund has been relieved of a serious burden in the cost of binding and cataloguing the books . . . it has enriched the library of Congress with a class of valuable works which could scarcely be procured by purchase, and has facilitated the use of the books by collecting them in one locality, under the same system, readily accessible to the public." Again, Prof. Henry remarks: "The library of Congress, or, as we think it should now be denominated, the 'National Library,' contains about 180,000 volumes" (1868). . . . This library is, emphatically, a library of progress, for while it continues to increase by purchase in its own series of standard works at all times, its additions through the contributions of the Institution include the Transactions of the principal learned societies of the world, or the works which mark more definitely than any other publications the actual advance of the age in higher civilisation;" adding, in order to counteract the impression that the Institution, since the transfer of its library, no longer desires to receive books, "that no change in this respect has taken place in the policy of the Institution." It is gratifying to learn that this is the case; and to know that while Congress uses the books, it carefully cares for them.

On account of the large expense attending the transmission of a few packages when the Institution first put in operation its now great system of exchange, but owing much more to the greater expense that it was anticipated would have to be met in connection with return transmissions for American addresses; and, moreover, since the total charges for transportation both to and from the United States would have to be defrayed almost entirely by the Institution, while the results of its efforts would be placed at the free command of all, both organised bodies and individuals, it was soon found necessary to attempt to secure reduced rates for its ocean freight. In addition, it was absolutely requisite that foreign ports should be opened to the entry free of duty of the packages of a scientific character bearing the Smithsonian label; and to the accomplishment of both these ends the eminent head of the Institution bent every energy. Upon a proper presentation of the subject, the leading steamship lines plying between the United States and foreign countries, besides several companies sailing in waters exclusively foreign (with merely representative houses in this country), one after another became impressed with the importance of scientific intercommunication between the Old and the New World, as developed under the Smithsonian system, until now all, with great liberality, grant free freights to books and specimens interchanged under the auspices of the Institution.

With the ports of the rest of the world open to the free entry of scientific truth, the continuance on the part of Italy, year after year, to withhold from her people this right, was long deemed sufficient cause for the suspension, by the Institution, of inter-communication in the line of transmission of books and specimens. But not till three years ago did such suspension take place, and then solely because of the great expense to the Institution, on account of taxes levied at Italian ports on parcels whose contents, while purely of a scientific character, were intended as presents to that people. The suspension, however, was but for a brief period. A knowledge of the cordial welcome which American contributions to science had always met with at the hands of the principal scientists and learned bodies of Italy, was sufficient to convince the Institution that a cessation of intercourse would last but a short time, while it would terminate beneficially to both parties. The result was as predicted. Shortly after the stoppage of transmission, there was manifested by the scientific portion of the Italian people a strong desire that it should be resumed. Negotiations were soon begun, and after two years Italy acceded free entry to parcels bearing the familiar endorsement of the Institution. The decree guaranteeing this right may properly be said to mark an epoch in the history of the Smithsonian Institution, as well as in that of the advance of scientific education. The Institution has a printed catalogue of foreign correspondents, numbering nearly 1,600 learned bodies, and, in addition, an extended manuscript list of individuals with whom it is in correspondence abroad, which embraces the names of the most eminent *savans* of the Old World. This shows that outside the United States, the policy of the Institution is everywhere highly endorsed; while fresh evidence of the fact is continually being received from new organisations, having for their object the advancement of science, in the form of applications for enrolment in the Smithsonian list of correspondents.

With no desire for a knowledge of the terms of the bequest, and satisfied as to the correctness of their own opinion that Smithson's gift was solely for the people of the United States, many Americans do not approve of extending the benefits of the said gift beyond the narrow limits of the land in which they themselves reside. In so enlightened an age, and with Smithson's will easily accessible, the error of such an opinion is unpardonable.

The mistake made by Congress, however, shortly after the bequest was accepted, is in a measure to be excused. The trust was of a novel character, while the instrument conveying it to the care of the United States made known but briefly the design of the giver. The life and character of the testator ought to have been closely investigated in order to arrive at a proper appreciaion of the true spirit of the terms upon which the money was given and accepted. It would appear that without an investigation of this kind, or certainly without a knowledge of Smithson's intention, Congress directed the management of the interest from the fund, for a few years immediately subsequent to its acceptance by the United States Government, in such manner as to divert the bequest for a long time almost entirely from its legitimate purpose. Several hundred thousand dollars yielded by a fund left for the "increase and diffusion of knowledge among men," was sunk in "brick and mortar." Had the amount expended in the erection, not to speak of the large sums paid out for the maintenance and repair, of the stupendous structure known as the Smithsonian building, been added to the principal of the original fund, the Institution would have been enabled to realise to a much fuller extent than has been done the anticipations of the generous foreigner whose name it perpetuates. While many were convinced of the fact at the time the fund was accepted, it is now universally admitted that for the "increase and diffusion of knowledge," brains and the printing press are the essential requisites, and that for the accommodation of these a building of moderate size and of small cost is all that is needed. The Smithsonian Institution, however, the especial object of which is that just set forth, continues to occupy a structure which in point of dimensions is vastly more extensive than is needed for its operations. The cost of maintaining such an edifice is very great, while, owing to its peculiar style of architecture, contingent repairs are frequent as well as expensive. It is to be hoped that the building will, before long, become the property of the Government, and the purchase money be added to the present Smithson fund. This vast edifice is suitably adapted to the exhibition of a Museum on a scale worthy the capital of the nation. The nucleus of such an establishment is already cared for by the Institution, but, although belonging to, is not maintained entirely at the expense of, the Government. The purchase in question of this building would be an acknowledgment of the intention of the United States to correct, as far as possible, the errors committed when the trust was accepted, and would prove an earnest to the people of other lands (for whom, equally with ourselves, the gift is designed) that the trustees of the munificent liberality of Smithson intended hereafter to carry out his wishes according to the letter and spirit of his will.

The efforts of the distinguished head of the Institution so to conduct the establishment as that the greatest good may eventually result to the greatest number, are appreciated far and wide, while his untiring devotion to the cause of education has rendered his name familiar to the most distant portions of mankind.

## *PHYSIOLOGICAL RESEARCHES AT GRATZ*

*Untersuchungen aus dem Institut für Physiologie und Histologie in Gratz.* Herausgegeben von Alexander Rollet. Zweites Heft. (Leipzig: Wilhelm Engelmann, 1871. London: Williams and Norgate.)

THIS record of Histological and Physiological research in Styria contains a series of interesting and important papers.

The first of these is one by the editor on the classification of tissues. Nearly every author who has written on Histology generally has put forth some classification or other. Amongst these, that of Dr. Beale in Todd and Bowman's "Physiological Anatomy and Physiology" (Ed. 1866, p. 70) is cited as one of the worst and most illogical. It is a difficult question to determine what are and what are not elementary units of histological structures. Dr. Rollet founds his classification, as far as possible, on the data afforded by the history of the development of the tissues. Thus, endothelial structures are classed with connective tissue, and separated entirely from epithelial, as being developed in the pleuroperitoneal cavity of the embryo. The classification arrived at is as follows:—

1. Sencocytes.
2. Red blood corpuscles.
3. Elementary parts of connective tissue.
4. Elementary parts of fatty tissue.
5. Elementary parts of muscular tissue.
6. Elementary parts of nerve tissue.
7. Elementary parts of epithelial tissue.

This classification seems to be an excellent one. For its further development we must refer to the paper itself.

A paper on the septic glands of the stomach by Dr. Rollet follows the former, and is most exhaustive in character, and the fact that the methods by which the results have been arrived at are most carefully described is especially to be commended. A new carmine solution is recommended which we have tried with excellent results. It has the advantage of being neutral, and of allowing of the addition of a certain amount of acid without suffering precipitation. It is prepared by boiling for five hours 35 grains of carmine with 270 cc. of dilute sulphuric acid (one volume of concentrated acid to fifteen volumes water), the volume being kept constant by the addition from time to time of water. The resulting solution is filtered and diluted with four times its volume of water. The sulphuric acid is then neutralised with carbonate of barium, and the solution quickly filtered. As soon as the filtrate has run off, a fresh quantity of water is poured on the precipitate, and comes through strongly coloured. Four or five filtrates may thus be obtained. The first two do not keep well, the third, fourth, and fifth do. From these solutions may be obtained what is called by Dr. Rollet carmine-red, which is soluble in distilled water.

It is too much the fashion amongst English histologists to aim at staining the nuclei only of the cells of tissues, whereas what is far more valuable is a clear definition of the boundaries of the cell itself. This result is in most cases only to be obtained by using a perfectly neutral solution of carmine such as the one just described. Dr. Rollet has found it yield very good results in cases where carminate of ammonium had failed. It would probably be found very good for silver preparations.

In a short notice it is impossible to do justice to such a paper as this. Dr. Rollet describes the glands of the rabbit, cat, dog, ox, sheep, pig, hedgehog, and other animals. He has also compared the appearance presented by the glands of the hybernating and active bat. The journal contains also an account of a "Commutator for Batteries in Physiological Laboratories," invented by Dr. Rollet; a paper on the "Development of Spermatozoa," by Dr. Victor V. Ebor, of great importance; another, on the "Glands of the Larynx and Trachea," by Dr. Mathias Boldyrew, who describes glands in all respects resembling pyers glands, as occurring occasionally in the larynx of the dog; and "Remarks on the effects of the administration of small quantities of curare in successive injections," by Julius Glase. The results are very remarkable. The animal becomes at each injection more and more sensitive to the poison, and finally reaches a state in which an extremely small quantity produces immediate convulsions and even death. Moreover, the injections may be intermitted for days and yet the animal remain as sensitive as before. The author believes that the system becomes adapted to the poison in such a way as to absorb it more rapidly, or that an actual change in some of the nervous centres occurs. Of course we cannot consider this a case of so-called cumulative poisoning, since the animal remains apparently perfectly healthy between the doses. The last paper is one on the "Ciliated Epithelium of the Uterine Glands." The author, Dr. Gustav Sott, has observed cilia in motion in the uterus of the cow, sheep, pig, rabbit, and moose. H. N. M.

## OUR BOOK SHELF

*A History of British Birds.* By the late William Yarrell, V.P.L.S., F.Z.S. Fourth Edition, revised by Alfred Newton, M.A., F.Z.S. Parts 1 and 2. (London: Van Voorst, 1871.)

"YARRELL'S British Birds" is without doubt one of the best known and most widely appreciated books on Natural History ever published in this country, and has probably done more than any other work to excite and augment an interest in one of the most attractive branches of zoology. At the same time, "Yarrell's Birds" is neither cheap nor popular in the ordinary sense of these terms, and the fact of three large editions of it having been sold, and a fourth being now called for, is a sterling proof of its extraordinary merits. The third edition of the work was issued in 1856, a few months before the author's death. For the editorship of the present (fourth) edition the publisher has secured the services of Prof. Newton of Cambridge, than whom no one is better qualified for the undertaking. Moreover, what is of still greater consequence, it may be added that, so far as we can judge from the parts of the work that have as yet reached us, Prof. Newton has set about the task entrusted to him in a very thorough way. As has been observed in the prospectus of the new edition, the literature of the subject has been nearly doubled within these last thirty years—that is, since the date of the publication of Mr. Yarrell's original work, while even since the issue of the last edition an extraordinary augmentation has been made of our knowledge of British Birds. "Very many of the species respecting which little was actually known in 1856 have been traced by competent observers to their breeding-quarters, and their habits ascertained, and in some instances minutely recorded." Mr. Yarrell's later editions having been little more than reprints of the original, with the intercalation of certain species recorded from time to time in the "Zoologist" and similar periodicals as "new British birds," it follows that a good deal of alteration and addition was necessary to bring the work up to the present standard of ornithological knowledge. This the new editor has apparently determined to effect, in spite of the vast amount of labour involved in so doing, which, on the whole, will fall little short of that of preparing an entirely new work on the subject. Such articles as those on the Griffon and Egyptian Vultures and the Greenland and Iceland Falcons in the first number require to be entirely rewritten, while material additions have to be made to the history of even the commonest species, particularly as regards their geographical range and their representation by allied forms.

The woodcuts of the present edition are mostly the same as those prepared for the original work.

It is certainly a decisive proof of the present popularity of ornithology, so far at any rate as regards the knowledge of our native species, that while Mr. Gould's "Birds of Great Britain" is still unfinished, and Messrs. Sharpe and Dresser have lately begun an entirely new work occupying nearly the same ground, a fourth edition of Mr. Yarrell's "History of British Birds" should be commenced with every prospect of permanent success.

P. L. S.

WE have lately received the last published Report on the progress of Entomology prepared in connection with the *Archiv für Naturgeschichte.* In the space of 226 pages it includes a review of all the works and papers published in 1867-68 on the subject of Entomology, taking that word in what may be called its Linnean sense, namely, as embracing the study of Insects, Arachnida, Myriopoda, and Crustacea. Of these reports, commenced by Erichson, continued by Schaum, and after his illness by Gerstäcker, it is impossible to over-estimate the value, for although the information contained in them upon the species and systematic matters is rather less detailed than in the English "Zoological Record," the notices of anatomical and physiological papers are fuller, and the student will always find indications of the direction in which to look for information on other subjects. The conductors of these useful Reports have always been in the habit of delaying their publication until the literature of each year could be analysed as completely as possible, and in the present issue we have only the particulars of the contributions to entomological knowledge published during the years 1867-68. The Insecta proper are reported upon by M. F. Brauer of Vienna, whilst Prof. Gerstäcker confines his labours to the Myriopoda, Arachnida, and Crustacea.

W. S. D.

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Science and Art Department

BELIEVING your columns to be at all times open for the discussion of matters connected with Science and Art teaching, I venture to offer a few remarks on the administration of the abovenamed Department.

Since the arrival of the results of the last May examination, the teachers have been enabled to make up their claims for payment, and there has been a great outcry from all parts of the country on account of the serious reduction that has taken place in the amounts the teachers are entitled to claim for their work. This reduction arises from the operation of the last alteration by the Department of the scale of payments on results, the full effect of which was not felt until this year.

Many objections were raised at the time the minute was issued (in the latter part of 1869), especially to its being introduced after the commencement of the work of the session, and in deference to these remonstrances it was modified in its application to the May examinations of 1870, and its full operation deferred till the May just past. When I say just past I am speaking after the manner of the Department, for they are so far behind that the Annual Report for the session of 1869-70 has not yet been received by the committees here.

Now I venture to submit that this reduction in the scale of payments is likely to have a very injurious effect on the spread of education in Science. In many cases, especially in small towns, and where a teacher has a class in only one or two subjects, the amount of payment to be received is so small as to give rise to the apprehension that such classes will be given up, because of the utter inadequacy of the payment to remunerate a teacher for the time and trouble spent in the work of instructing them. To give an illustration, I have just been informed of an instance where a teacher has had to proceed fourteen miles by rail to give his lectures to a class, and the result of the recent examination is that he received absolutely nothing from the Department in respect of this class, all those of the students who passed being persons of the middle classes.

When in conjunction with such facts as these we read that the payment for instruction in Science and Art in 1870 was 17,000*l.* less than in the previous year, there seems great reason for the complaints of the teachers, and one must, I think, come to the conclusion that the Department has proceeded too far in the direction of economy to be conducive to efficiency or to the continued spread of scientific instruction.

If the present scale is to be retained, I think the suggestion of a writer in the *Daily News* recently, that the system of payment by results alone should be modified, is worthy of consideration. The teacher often has the greatest amount of trouble with those students who just miss obtaining a certificate, and in these cases the master receives nothing at all for the labours of the session. It would be more encouraging to the teacher if a small payment was made for those students who have attended the required number of class lectures, although they may not be able to pass the examination for proficiency. Of course, there should be safeguards provided against the abuse of such a rule, say, by excluding from its operation all such as are unable to attempt a stated proportion of the questions propounded.

Even under the present arrangements I have seen many students sit idle the greater part of the evening, or leave the examination room as soon as permitted to do so by the regulations, through not being able to attempt to answer more than two or three questions.

With regard to another matter of which complaint has been made, viz., the recent minute of the Department imposing fines on Committees who ask for a larger number of papers than they require, I must say I cannot see the reasonableness of the complaint. At the last May examination in Plymouth, one school alone (according to the printed list issued by the department) sent up a requisition for 714 papers in the various subjects of Art and Science, while the total number of papers worked by pupils of that school, and by strangers whose papers were asked for through its committee, was only 339—less than half. If this case is at all a sample of what was occurring in the country generally, and the issue of the minute leads me to think such must have been the case, I consider it was quite time for something to be done to prevent such wholesale waste. Of course in all schools there will be a certain number who will shrink back at the last moment, after having given in their names for the examination, and this being fairly provided for, I do not see anything in this regulation that efficient schools and committees should complain of.

A LOCAL COMMITTEEMAN

Plymouth, Sept. 14

### Elementary Geometry

I HAVE to thank the Editor of NATURE for inserting my letter, and Mr. Wilson for writing so fully. I was not aware when I wrote, that Mr. Wilson had himself published an elementary book on geometry He has modestly omitted to refer to it; but I have seen it, and it appears to me a suggestive book for a teacher. He acknowledges himself unable to recommend one suited to boys, for laying the foundation of geometry.

Mr. Wilson's advice seems rather suited to teachers of geometrical drawing than of mathematics. Of course it is essential that a boy should know what measuring means; but scales of measurement have no essential connection with geometry. Nevertheless, I entirely agree with him that much trouble must be taken to teach the metrical system, especially as it is not likely to be popularly used in England, at least while our children are living. To purchase a simple metrical rule is by no means an easy task. They are not kept, that I can find, at any *ordinary* instrument makers in London, Oxford, or Cambridge.

With your permission I will say what I think is required in a book for boys. It must, to a certain extent, be suitable for being committed to memory, as Euclid is. No child is capable of taking in a subject, especially if it involves logical thought, except by very slow degrees; and must at the beginning commit much to memory which he does not comprehend. What I write will appear, to those who do not know children, to involve a most vicious principle in teaching; but it is, nevertheless, a fact. Our new book must, therefore, contain all the steps of every proof in full, and no symbols must be used. In the next place, it must not be artificial It seems agreed that the use of compasses ought to be of the nature of a postulate, which would at once get rid of such propositions as the second and third of Euclid. There can be no reason for excluding the idea of the motion of a point, since in practice no figures can be drawn except by moving the pencil's point. It appears then that, having once got over the difficulty of defining a point, a line should be "the path of a point," a definition which would easily lead on to the doctrines of curvature and tangents; and a straight line would be that which does not alter its direction (virtually Euclid's definition); and, as in Mr. Wilson's book, parallel straight lines, those whose directions are the same.

It seems to me that it would be better to retain Euclid's definition of proportion, only converting it into a *test* of proportion; because the constructions founded upon it are so convenient for geometrical purposes; and if it be superseded by the algebraical mode of treating the subject, reasoning on incommensurable quantities must be introduced.

In fine, the book which is to supplant Euclid appears to be at present a desideratum. When it appears, it probably must be the work of more heads than one, if it is to be generally accepted among teachers.

A FATHER

THE circumstances attending my own introduction to geometry lead me to doubt whether a long course of practical geometry is the best beginning. I believe that, on the contrary, unless the demonstrative and deductive principles of the science are soon introduced to the student's notice, he is likely to acquire a distaste for the subject.

I was learning, under an infliction of practical geometry (at school), to detest the very sight of a box of mathematical instruments, when a fortunate illness kept me at home for two or three years. I believe that Euclid, as it would have been introduced to me at school, would have rendered my dislike for mathematics complete. But becoming possessed of a Simson's "Euclid," and *reading* it instead of learning it for "class," I found geometry the most enjoyable of subjects. In a very few months I came to the end of the book, and I have never lost the liking for geometry which I had by that time acquired.

Let it not be supposed, however, that I advocate the claims of Euclid as a text-book. The first, third, and sixth books might indeed be retained—with certain omissions and modifications; but the second and fourth books (setting aside a few propositions) are monstrosities of clumsiness.* The fifth, eleventh, and twelfth could never be generally used in their present form. But whether a totally new text-book be adopted or Euclid be modified, I am convinced that until the demonstrative and deductive nature of the science is recognised the interest of the student will not be excited.

While, however, my own experience will not permit me to believe that a course of practical constructions is a suitable introduction to geometry, I certainly agree with Mr. Wilson in regarding careful constructions as of the utmost importance to the learner. But, in my judgment, the processes of construction should accompany, not precede, the study of some demonstrative and deductive treatise.

I believe the chief difficulty under which we labour at present, resides in the fact that, owing to the small encouragement given to the study of geometry at our Universities. we have, even among our ablest mathematicians, very few able geometricians. One cannot read the Cambridge text-books of mathematics—written though these are, in many instances, with singular ability—without becoming convinced of this. So soon as a geometrical construction is introduced we recognise the clearest traces of inaptitude. The fact is still more clearly evidenced in treatises professedly geometrical. I take up an edition of Euclid, prepared by a very eminent mathematician, a senior wrangler, and, opening at random the portion relating to deductions, I find the following problem:—"Required to draw a circle through a given point to touch two intersecting lines;" and to solve this obvious third-book problem the aid of the sixth book is called in.

But it is hardly to be wondered at that university mathematicians are, as a rule, not strong in geometry, for the study of geometry is very little encouraged by university tutors. Indeed, an aptitude for geometrical methods is generally regarded as more mischievous than useful in the Tripos. I can remember the hints I myself received on this point. A few instances may perhaps interest your readers.

The first hint was given me in the lecture-room by a high wrangler (an excellent geometrician). The following proposition had been submitted—"A ball is placed on a horizontal plane, above which is a luminous point; show that the length of the minor axis of the ball's elliptic shadow depends only on the height of the luminous point above the plane." I wrote for answer that the fact is obvious, because two sloping planes touching the ball, and with a horizontal intersection through the luminous point, must clearly have the same slope wherever the ball is placed. The proof was accepted, and even regarded (to my infinite surprise) as ingenious; but I was warned not to leave the safe track of analysis.

The next hint was given me by my private tutor, a senior wrangler with fine (but untrained) geometrical powers, on the score of my solving geometrically some problems relating to epicycloidal and hypercycloidal areas.

The third hint was given me by a vacation tutor, also a senior wrangler, and was perhaps the best deserved of the three. He had set me a problem relating to a curve which chanced to be a projection of the four-pointed hypercycloid, and the problem was meant as an exercise in analytical processes. Knowing very little about these, I ventured to proceed *more meo*. I first projected the curve back again (so to speak), established the property in the case of the quadricuspid hypercycloid, and reprojected all my constructions on the original plane of the curve. I shall never forget the solemnity of the warning I received.

The last case I shall refer to relates to a probability problem (the last in Todhunter's "Integral Calculus") about a messenger and a shower of rain, the messenger's "expectation" under certain stated conditions being expressed in the following pleasing form:—

$$\frac{v}{u}\left\{\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v^2}\log\frac{u+v}{u}\right\}$$

From the day that I gave a geometrical solution of this problem (the logarithm coming out as a hyperbolic area) I was given up as a bad job. No wonder, indeed, for as a problem in the Integral Calculus it can be solved in half-a dozen lines.

So little encouragement is given to geometrical work, that I know instances where men who have taken very high degrees could not solve the easiest geometrical problems. Many indeed in my time (I believe Mr. Wilson would confirm this) in their second or third year at Cambridge, scarcely know what has to be done with such problems—that is, even how to try to solve them. I wrote a little pamphlet four or five years ago, to show how such problems should be attacked, proceeding on the following plan:—I took the case of a beginner dealing with easy geometrical problems, and considered his difficulties and false steps, as well as the true demonstration ultimately evolved. I did this because I had found it the only effectual course with pupils. To give problems, and on the pupil failing to solve them, to show him the solution, is utterly useless. One must listen to his reasoning, wrong or right, to the purpose or not—show him why it is wrong, or not effective towards the solution of the problem; and so gradually guide him towards the correct solution. In the pamphlet I employed a corresponding method.

Unfortunately (for me at any rate) Messrs. Longmans submitted this pamphlet to "a competent mathematician," who immediately misunderstood my plan; took the imagined difficulties for real difficulties of my own, and solved for my behoof an immensely difficult problem—the first worked-out example in Potts's Euclid. This achievement (*par parenthèse* my pamphlet also) was then submitted to another competent mathematician, and he, excited to emulation, suggested another solution of a problem which a boy of twelve might safely attack. Finally, these labours were submitted to Messrs. Longmans and (signatures removed) to myself. So my pamphlet has remained in my desk; for I thought better of it than to send it begging.

We want geometricians more than text-books just now. If our universities would give geometry a reasonable position among the subjects for mathematical examination, we should probably soon have both. At present a man with geometrical tastes must either turn from his favourite subject during his university career (with small chance, perhaps, of resuming it) or must be content with but a small share of university success.

Brighton, September 15 R. A. PROCTOR

* All the propositions of Book II, save four, may be established (usually) in three lines from the first two, of which they are in fact little more than corollaries. The main objection to the fourth book relates to the inscription and circumscription of the regular figures; but throughout the book the heaviness of Euclid's method is much felt.

### Captain Sladen's Expedition

IN reply to F.R.S.'s inquiry in your issue of September 14, I may state that the last number of the "Proceedings of the Zoological Society of London" (1871, part 1) contains several articles by Dr. John Anderson relating to discoveries made during Capt. Sladen's expedition to Yunan, and that the next number (1871, part 2), which I am now preparing for the press, will contain others.

It was Dr. *Thomas* Anderson, Curator of the Botanic Garden at Calcutta, whose untimely death we have recently to lament. Dr. *John* Anderson (his brother) is, I am happy to say, in good health at his official post as Curator of the Indian Museum and Professor of Comparative Anatomy at Calcutta, or was so, at all events, at the date of his last letters to me, about a month since.

11, Hanover Square, Sept 16 P. L. SCLATER

### Deschanel's Physics

AS regards the particular passage in my edition of Deschanel which I am challenged to defend by your Reviewer (NATURE, vol. iii p. 343), his charge, which is somewhat obscured by rhetorical embellishment, seems to be that in the factor $\frac{H-h}{760}$ it has not been indicated that $H$ and $h$, as well as 760, denote so many millimetres of mercury *at zero*. I think this was scarcely necessary, as the question whether the observed or reduced heights of the mercurial columns should be employed, is not one on which a doubt could occur to the mind of any intelligent reader, reduced heights being invariably employed in the chapter in which the passage occurs, and in the book generally.

The charge of inconsistency which the Reviewer urges with so much gusto, is based on the following passage in my Preface, at the beginning of Part I :—

"There is great danger in the present day lest science-teaching should degenerate into the accumulation of disconnected facts and unexplained formulæ, which burden the memory without cultivating the understanding. Prof. Deschanel has been eminently successful in exhibiting facts in their mutual connection; and his applications of algebra are always judicious."

Which, the Reviewer thinks, justified the expectation that I would omit as many as possible of Deschanel's applications of Algebra. It is not surprising that a writer accustomed to this style of inference should have an aversion to exact reasoning, and should characterise the solution of problems by the application of a little algebra as "intricate formulæ, which burden the memory without cultivating the understanding"

I may remark, with reference to my former discussion with W. M. W. in your pages, that the adoption of concrete units of mass, and derived units of force, has now received the official sanction of Sections A and G of the British Association, who have appointed a committee to frame a system of nomenclature on this basis. J. D. EVERETT

### Newspaper Science

KNOCKED up with work, I reluctantly followed the advice of my medical man, and crossing the Channel so as to be more out of the way, resolved to eschew everything scientific for the next few weeks at least, in order to recruit before the winter's labours commenced. Even here, however, I soon found that the desired result was not so easily attainable as I had imagined, for the first thing this morning, on entering the reading room of the bathing saloon, a French acquaintance, placing the *Globe* (of Monday evening, September 11) before me, directed my attention to its leading article on Prussian Artillery, adding significantly—"*Violà, mon ami*, a specimen of English scientific opinion!"

I must confess that it was not without a feeling of shame that I read an article, of which the following extracts will suffice to give a correct idea.

"Although the unchequered course of the late war was due to many causes, still it is now admitted on all sides that when the Krupp guns were brought into the field the conclusion was practically foregone." "The first public exhibition of what is now known as the Krupp gun was the gigantic specimen of a breechloading steel gun sent to our Exhibition of 1851. The steel of which this gun was made differs entirely from our *Sheffield gun metal* or from Bessemer metal, and is a *composition invented by Krupp*, and the result of a special process. The iron is alloyed *with certain clays and also with a preparation of plumbago.* There are 100,000 '*creusets*' of this metal always in active employment in the factory, and each '*creuset*' contains from twenty to forty kilogrammes. The metal in a fluid state is poured into large cylindrical moulds, where it remains for two hours till it has completely hardened. But the chief difficulties of the process lie in subjecting it to the steam hammer. For years the hammer of greatest power in the factory had a force of 25,000 *kilometres*," &c.!

The italics are mine, and any one conversant with such subjects will perceive that no further comments are required. It only remains for me to express my astonishment at seeing such rubbish appear in the leading article of any newspaper of standing, and I am sure your readers will agree with me that it is high time that journals specially devoted to science should protest energetically against such representations being conveyed to the public at home and abroad as expressions of English technical or scientific opinion. DAVID FORBES

Boulogne-sur-Mer, September 13

## *THE NEW GANOID FISH (CERATODUS) RECENTLY DISCOVERED IN QUEENSLAND*

AT the beginning of last year news reached Europe that a large "Amphibian resembling *Lepidosiren*" had been discovered in Australia, and the curiosity of naturalists was still more excited when it was stated that this creature was provided with teeth extremely similar to the fossil teeth (from the Jurassic and Triassic formations) known under the name of *Ceratodus*.

The interest attached to such a discovery will be easily understood, if we review briefly the history of *Lepidosiren*, and show the advance made by zoology in consequence of our acquaintance with this animal.

The discovery is due to the well-known Austrian traveller, Natterer, who sent two examples from Villa Nova on the Amazon River and the Rio Madeira to the Vienna Museum in the year 1837. Fitzinger, then Curator of the Collection of Reptiles, gave a somewhat superficial description of it under the name of *Lepidosiren paradoxa*, referring it without hesitation to the class of Reptiles. Nearly at the same time a very similar animal was found by Mr. Th. C. B. Weir, in Senegambia; he presented two small examples to the Royal College of Surgeons; and Prof. Owen, then Curator of the Hunterian Museum, published a full description of them under the name of *Lepidosiren annectens*, in the year 1839, explaining the reasons which induced him to regard this creature as a Fish. This view elicited further examination of the internal structure of the American species by Profs. Bischoff and Hyrtl, the former inclining to the opinion expressed by Fitzinger, the latter confirming, to the satisfaction of nearly all zoologists, the correctness of the conclusion arrived at by Owen.

Before the discovery of *Lepidosiren*, zoologists distinguished the class of Reptilia from that of Fishes by the organ of respiration, the former being provided with membranous lungs extending into the abdominal cavity, the latter breathing by gills only. Although the Batrachian reptiles were known to breathe by external gills, as fishes, during the early stage of their metamorphosis, and although some of them retain those gills through the whole period of their life, yet the development of lungs in the adult state and the co-existence of these organs with gills in the Perennibranchiates, were considered to be sufficient indications of their class-distinctness from fishes, among which no air-breathing organ was known. It is true Harvey and Hunter had pointed out that the air-bladder of the fish was homologous with the lung of higher vertebrates; but functionally it could not be compared to it, as it receives arterial blood like any other abdominal organ, returning it in a deoxygenised condition.

Now *Lepidosiren* was found to be provided with gills, and a most perfect paired lung communicating by a ductus pneumaticus and glottis with the œsophagus, receiving venous blood by strong arteries, and sending it back directly to the heart in an oxygenised condition. Therefore, in this respect it did not differ from an Amphibian, and dogmatical believers in the stability of our zoological systems felt themselves quite justified in referring this creature to the Reptilians.

Nevertheless, the presence of certain other peculiarities of structure indicated rather an ichthyic than a reptilian affinity. The notochordal skeleton, and the apophyses arranged as in many fishes, and not as in Amphibians; the organ of hearing enclosed in the cartilaginous capsule of the skull; the dentition extremely similar to that of a Chimæra; the intestinal tract traversed by a spiral valve; peritoneal outlets near the vent; no nasal canal to conduct air; finally, the skin covered with scales, the fins supported by fin rays. All these are characters not found in Batrachians, and connect *Lepidosiren* with the class of Fishes; but it was admitted that it makes the nearest approach in that class to the Perennibranchiate Amphibians.

The question had next to be settled, what place in the class of Fishes should be assigned to *Lepidosiren*; and as the view entertained by Joh. Müller is that adopted by the majority of zoologists, we think it sufficient to refer to it alone. Having determined that all Ganoid Fishes agree with the Sharks and Rays in having an additional muscular division of the heart at the origin of the aorta, named *bulbus* *arteriosus*, and provided with transverse series of valves in its interior, he found that such a *bulbus arteriosus* was likewise present in *Lepidosiren*, but with a very different valvular arrangement. This peculiarity, combined with the development of a lung, he considered to be sufficient to distinguish *Lepidosiren* as the type of a separate subclass, which he named *Dipnoi*, and placed at the head of the entire class.

Thus, then, *Lepidosiren* was finally placed among the Fishes; but from the time of its discovery dates the tendency of zoologists to subdivide the assemblage of cold-blooded animals *not only where the development of a lung ceases, but also where the development of gills begins.* Or, in other words, systematists became more and more convinced that the old division of Reptiles and Fishes was insufficient, and that three classes of living cold-blooded Vertebrates should be distinguished, viz., Reptiles, Amphibians, and Fishes, some regarding the second as even more closely allied to the third than to the first.

When we find a group of animals represented by a very small number of forms in the existing Fauna, we look to Palæontology to fill up the seeming blanks; but *Lepidosiren* did not appear to have any fossil representatives. Prof. Owen stated (in 1839) that its teeth resembled "in their paucity, relative size, and mode of fixation to the maxillæ, those of the *Chimæra* and some of the extinct cartilaginous fishes, as *Cochliodus* and *Ceratodus*;" but no further inference was made from this fact as regards affinity. And Prof. Huxley (in 1861), when drawing attention to analogous structures in *Lepidosiren* and certain Devonian fishes, still maintained the entire absence of the Dipnoous type in the fossil state.

The discovery of a "gigantic Amphibian allied to the genus *Lepidosiren*, from rivers in Queensland," and named *Ceratodus Forsteri* by Mr. Krefft, promised to mark another step in the advancement of our knowledge, and to lend additional aid in determining the natural affinities of these animals. As soon as Mr. Krefft had recognised the importance of this discovery, the trustees of the Australian Museum of Sydney took steps to secure well-preserved examples. They sent a collector into the district where the animal was known to occur; and, with their usual liberality, they despatched to the British Museum, for examination, the first specimens they could spare, by which I was enabled to present a full account of its organisation to the Royal Society. It is not my intention to enter here into the details of the results of this examination; I must be satisfied with giving a short description of it, pointing out some of the bearings which this discovery has upon the advancement of science.

The fish (for this it proved to be, and even more so than *Lepidosiren*) appears to be not uncommon in some districts of Queensland; specimens have been obtained from the Burnett, Dawson, and Mary rivers, some high up in perfectly fresh water, others descending into the lower brackish portions. It is said to grow to a length of six feet, the largest example sent to the British Museum being about three and a half feet long. The flesh is excellent eating, and of salmon colour, hence it is called by the squatters Burnett or Dawson salmon. Its food consists of the decaying leaves of myrtaceous and other plants, with which the stomach and intestine are crammed. Probably now and then it swallows, perhaps accidentally, some aquatic animal; but it is rather doubtful whether it can be caught by using living animals as bait. It is also stated that it is in the habit of going on land, or at least on mud-flats; and this assertion appears to be borne out by the fact that it is provided with a true lung. On the other hand, we must recollect that a similar belief was entertained with regard to *Lepidosiren*, of which now numerous examples have been kept in captivity, but none have shown a tendency to leave the water. I think it much more probable that this animal rises now and then to the surface of the water, in order to fill its lungs with air, and then descends again until the air is so much deoxygenised as to render a renewal of it necessary. When we recollect that the animal evidently lives in mud or in water charged with the gases which are the product of decomposing organic matter, the usefulness or necessity of such an air-breathing apparatus, additional to the gills, becomes at once apparent. Further we shall see that the limbs of this unwieldy and heavy animal are much too feeble and flexible to be of much use in locomotion on land; they may assist it in its crawling, in water, over the muddy bottom of a creek; but the chief organ of locomotion is the compressed, broad, and flexible tail, denoting by its shape and structure that the fish can execute rapid swimming motions. However, it is quite possible that it is occasionally compelled to leave the water, although I do not believe that it can exist without it in a lively condition for any length of time. It is said to make a grunting noise, which may be heard at night for some distance. This noise may be produced by the passage of the air through the œsophagus, when it is expelled for the purpose of renewal.

It deposits a great number of small eggs, which are impregnated after deposition. Nothing is known of their development; but we may infer that the young are provided with external gills, like those of some other Ganoid Fishes.

The *Barramunda* (we will use the name given to it and other similar fishes by the natives) is eel-shaped, but considerably shorter and thicker than a common eel, and covered with very large scales. The head is flattened and broad, the eye lateral and rather small, the mouth in front of the broad snout and moderately wide. The gill openings are a rather narrow slit on each side of the head. There are no external nostrils. The tail, which is of about the same length as the body without the head, is compressed, and tapers to a point, but it is surrounded by a very broad fringe, supported by innumerable fine and long fin-rays. There are two fore and two hind paddles, similar to each other in shape and size, and very different from the fins of ordinary fishes; their central portion being covered with a scaly skin, and the entire paddle surrounded by a rayed fringe. If we were to cut off the hind part of the tail of a fish, the piece would bear a strong resemblance to one of the paired paddles. The vent is situated in the median line of the abdomen between the paddles.

In order to obtain a view of the inside of the mouth, it is necessary to slit it open, at least on one side. We then notice that there are a pair of nasal openings within and on each side of the cavity of the mouth. The palate is armed with a pair of large, long, dental plates, with a flattish, undulated, and punctated surface, and with five or six sharp prongs on the outer side, entirely similar to the fossil teeth described under the name of *Ceratodus*. Two similar dental plates of the lower jaw correspond to the upper, their undulated surface fitting exactly to that of the opposite teeth. Beside these molars the front part of the upper jaw (vomer) is armed with two obliquely placed incisor-like dental lamellæ, which have no corresponding teeth in the lower jaw. As we know the kind of food taken by the Barramunda, the use of their teeth is apparent. The incisors will assist in taking up, or even tearing off, leaves, which are then partially crushed between the undulated surfaces of the molars.

The skeleton consists of a cartilaginous basis, in the form of a long tapering chord for the body and tail, and in that of a capsule for the head. No segmentation into separate vertebræ is visible in any part of the notochord but it supports a considerable number of apophyses, the abdominal of which bear well developed ribs, all being solid cartilaginous rods, with a thin sheath of bone. In the same manner no part of the brain-capsule is ossified, but it is nearly entirely enclosed in thin bony lamellæ. This is also the structure of the appendages of the skull, as the mandible and the hyoid and scapulary arches. From a study of the skull, it becomes apparent at once why in fossil teeth of *Ceratodus* nothing or very little of the bone attached to them has been preserved. Those teeth rest on cartilage as well as on bone, the latter being a very thin and porous layer which could not be preserved, unless the progress of stratification had been going on with as little disturbance as in the Solenhofen Schiefers; but the matrix in which fossil Ceratodont teeth are found shows that it was formed under very different conditions, and it is certainly not of a nature to permit the supposition that thin porous lamellæ of bone would have been preserved entire.

The structure of the skeleton reminds us much of that of the sturgeons, Chimæra, and especially of *Lepidosiren;* and of all the modifications by which it differs from these types, perhaps none is of greater interest than that observed in the paddles. The central part of the paddle, which we have found externally to be covered with scales, is supported by a jointed axis of cartilage extending from the root to the extremity of the paddle; each joint bears a pair of three- or two- or one-jointed branches. This is the case in the hind as well as fore paddles, and we are justified in supposing that those extinct Ganoids of which impressions of paddles with scaly centres have been preserved, were provided with a similar internal skeleton. Professor Huxley, some years ago, drew attention to the analogy existing between the filamentary limbs of *Lepidosiren* and the lobate fins of certain extinct Ganoids, and the correctness of this view is fully borne out by the discovery of *Ceratodus*, inasmuch as the *Lepidosiren* limb proves to be typically the same as that of *Ceratodus*, but reduced to the jointed central axis.

The gills are perfectly developed, four on each side. They are broad lamellated membranes, free from each other, but attached to the outer walls of the gill-cavity. One can hardly doubt that, in water of normal composition, they are sufficient for the purpose of breathing. A lung, however, is superadded to them, a true lung, which receives blood from a branch of the aorta, and returns it directly to the heart by a separate vein. Whilst the Barramunda is in water sufficiently pure to yield the necessary supply of oxygen, the function of breathing rests with the gills alone, and the lung receives arterial blood, returning venous blood, like all the other organs of the body; under this condition it does not differ from the air-bladder of other fishes. But when the fish is compelled to sojourn in thick muddy water, charged with noxious gases, which must be the case very frequently during the droughts which annually exhaust the creeks of tropical Australia, it commences to breathe air in the way indicated above; under this condition the pulmonary vein carries purely arterial blood to the heart, where it is mixed with venous blood and distributed to the various organs of the body. If the medium in which the fish happens to be is perfectly unfit for breathing, the gills cease to have any function; if only in a less degree, the gills may still continue to assist in respiration. In short, the organisation of the Barramunda is such as to justify us in the assertion that it can breathe by either gills or lung alone, or by both simultaneously.

With regard to the structure of the lung, it shows a nearer approach to the air-bladder of other living Ganoid fishes than that of *Lepidosiren;* it is not paired, but consists of a single long sac extending nearly to the end of the abdominal cavity. Yet the interior of the sac shows a symmetrical arrangement of the right and left side, being subdivided into numerous cellular compartments, by which the respiratory surface is much increased in extent.

The next organ of importance for determining the systematic affinities of the Barramunda is the heart. Considering the great resemblance this fish has shown in other respects to *Lepidosiren,* I fully expected to find this organ agree also with the Dipnoous type; but this is not the case. Instead of the two longitudinal valves of the Dipnoous heart, the *bulbus arteriosus* is provided with two or three transverse series, of which one only is fully developed; or, in other words, *Ceratodus* proved to be a Ganoid fish. But, as *Ceratodus* and *Lepidosiren* are in all other points too closely allied to be separated in two distinct sub-classes or even sub-orders, we must arrive at the conclusion to drop the *Dipnoi* as a sub-class, and to refer *Lepidosiren* also to the Ganoids, which will then be characterised, not by transverse series of valves, but by the presence of a muscular, contractile *bulbus arteriosus* with valves, transverse or longitudinal, in its interior—a structure which they have in common with the sharks and rays (*Plagiostomata*).

The intestinal tract is a large straight sac with an internal spiral valve, as in the Ganoids and Plagiostomes. The kidneys are paired, the ureters enter a very small urine bladder or cloaca at the back of, and partly confluent with, the rectum.

The organs of propagation show some noteworthy peculiarities. They are paired, in long bands. The male organs have no visible outlet, although a seminiferous duct has been found traversing the substance of the testicle through nearly its whole length; no outward opening could be discovered, and it is not known how the semen is discharged. The ova are small, very numerous, and attached to transverse laminæ of the ovaries; when mature, they fall into the abdominal cavity, as in the salmon tribe, and would appear to be expelled through two wide slits behind the vent. Yet each ovary is accompanied by a long oviduct, as in the sturgeon or *Lepidosiren*, though it probably has no function, and is only indicative of an approximation of this remarkable fish to higher types. Such are some of the principle features of the organisation of the Barramunda; and it remains now to add some remarks on its affinities and its place in the system.

A. GÜNTHER

(*To be continued.*)

## *ON EXOGENOUS STRUCTURES AMONGST THE STEMS OF THE COAL-MEASURES*

IN a memoir recently read before the Royal Society, I propounded a new classification of the vascular cryptogams, and at the late meeting of the British Association at Edinburgh I brought the same subject forward, when my views were opposed by Mr. Carruthers, Dr. M'Nab, and Prof. Dyer, as reported in the columns of NATURE for Aug. 31. I was well aware that when I disturbed existing and time-honoured systems of classification I should meet with such opposition; but, being thoroughly convinced that my views are sound, and that they will ultimately be adopted, it only remains for me to face the conflict, and persevere with my demonstrations of what I believe to be true. My present object is to do what was impossible in the hurried and unsatisfactory discussions that frequently arose at the meetings of the British Association to accomplish, viz.: to take care that there shall be no misunderstanding as to the real points at issue. My opponents seek to interpret the gigantic arborescent stems of the coal-measures by the light of the dwarfed and degraded examples of vascular cryptogams which constitute their living representatives. I, on the other hand, claim to interpret the latter by the former, some of which, the Lycopods, for example, instead of being feeble things trailing in the grass, had stems three feet in diameter, and rising a hundred feet into the air. Instead of merely constituting a verdant carpet for forests of noble exogens and endogens, they were the forest; here, consequently, we might expect that whatever characteristic features they possessed would be developed and displayed in their utmost perfection.

Mr. Carruthers' fundamental argument is, that I, in my classification, elevate the vegetative organs at the cost of the reproductive ones. I reply I am merely applying principles already adopted by botanists throughout the world. They are those of DeCandolle, of Endlicher, of Lindley, of Brongniart, and of Balfour. These writers, in common with most others, recognise primary distinctions that are purely vegetative. Not only are those which separate vascular from cellular plants of this character, but the further ones of exogens, endogens, acrogens, and thallogens are of the same nature. The fact of the closest resemblance of the inflorescence, and of the formation of the embryo in the embryo-sac in the two groups, does not prevent the separation of the flowering plants into exogens and endogens. Turning from the phanerogamous to the cryptogamic plants, we find that nearly every writer of importance adopts vegetative features as the basis of his classification. DeCandolle divides his *Acrogens* into those which have and those which have not vascular tissues. Endlicher's primary term *Cormaphyta* refers to a vegetative feature, viz., the possession of a stem, whilst his secondary divisions of *Acrobrya*, *Amphibrya*, and *Acramphibrya* all refer to the mode of growth and not to fructification. Lindley again distinguishes his flowerless plants according as they are acrogens or thallogens; whilst Balfour characterises them primarily as acrogens or cormogens and thallogens. In thus dwelling upon the vegetative element of the cryptogams, I am merely treading in the steps of nearly every writer of note who has written on these subjects. So much, therefore, for the primary point.

In many of the discussions which have taken place, my opponents have made the mistake of supposing that I was trying to prove these fossil coal-plants to be dicotyledonous exogens. Whereas what I have throughout contended for is that they are true cryptogams with an exogenous woody axis. Mr. Carruthers says, "The plants were true cryptogams, and in their organisation agree in every essential point with the stems of Lycopodiaceæ" (NATURE, p. 337). With this I of course agree, but I contend that we must interpret the lower forms by the higher and not the higher by the lower. In Carboniferous ages, these plants became superb forest trees, and consequently their stems attained their full development, growing year after year, from their almost microscopic condition when they burst from a microscopic spore, until they became stately trees, such as were revealed at Dixon Fold, and such as are illustrated by specimens now in the Manchester Museum. In the course of their magnificent development the stems were gradually fitted to sustain an enormous weight of branch and foliage. This was done by the development, within those stems, of a vascular woody cylinder, which grew thicker year by year; *such thickening being the result of additions to the exterior of the previous growths.* We here come to a definite issue. Do my opponents intend to deny the existence, in these arborescent carboniferous plants, of these thick ligneous cylinders, or to dispute that they grew in the way described? I think they cannot possibly contemplate doing so. Dr. M'Nab says botanists are agreed in this, that "Lepidodendra and their allies are closely related to other Lycopods. Now we know that the Lycopods, *like the Ferns, have closed fibro-vascular bundles; bundles which can only grow for a certain time, and then, all the cambium being converted into permanent tissue, growth must cease.*" The italics in the preceding paragraph are my own. With the above remark, so far as Ferns are concerned, I thoroughly agree. The facts so correctly stated by Dr. M'Nab constitute one of the fundamental bases of my proposed classification. The vascular bundles *are* closed in all the small ferns, and they remain equally so in the Cyatheas and other arborescent ferns which attain to stately dimensions. The development of this type into a lofty tree has not materially modified the structure of the stem which recurs in the most dwarfed species. But when Dr. M'Nab applies the above general statement to the Lycopods, facts do not sustain him. The huge lepidodendroid carboniferous plants give it a direct contradiction. They have *not* closed vascular bundles, and their growth did *not* cease after a limited time, but was obviously continued, being sustained by a cambium layer, until the plants assumed the magnificent dimensions which their fossil remains now exhibit. That the large vascular cylinder of the fossil forms is a development of what is seen, not only in *Lycopodium chamæcyparinus* referred to by Dr. M'Nab, but in every one of the numerous Lycopods of which I have examined sections, I have never denied. Quite the contrary. But I repeat we must interpret the significance of the *least* developed form by that which is *most* developed. Consequently we must regard the irregular vascular bundles which exist, commingled with cellular tissues, in the axis of each living Lycopod, as a degraded wood cylinder, whose nature can only be understood by reference to what it once was when its parent tree was one of the glories of the primæval forest. The race as a whole has become degenerate, and the stem being no longer called upon to sustain a lofty superstructure, its structure has become equally degenerate.

I will not enter in detail into the question of the nomenclature of the various parts of these exogenous cryptogamic stems, but reserve that subject for some other occasion, after my detailed memoirs now in the hands of the Royal Society have been published. I will merely express my conviction that Mr. Carruthers, who differs widely from me on the subject, assumes the very question in debate between us.

He holds that we can draw no parallel between the conditions existing in the stems of Cryptogams and those of Phanerogams. This is precisely what I contend we can do, and I trust to be able, as my self-appointed task proceeds to its conclusion, to demonstrate to the botanical world that I have abundant reason for so doing. This is a question wholly resting upon facts, and until those facts are fairly before the world, I object to the adoption of any *a priori* conclusion on the subject. Consistently with his views Mr. Carruthers objects to my applying to the stems in question such terms as medulla and medullary rays; especially objecting to the application of the term medulla to a structure containing vessels, *i.e.*, a vascular medulla; but Nepenthes has a vascular medulla, as well as some other phanerogamous plants, and no one presumes to deny the medullary character of such a tissue, because it happens to have vessels in it. The medullary character of the structure does not rest upon the basis of its being wholly devoid of vessels, neither does their occasional presence militate against its being a medulla.

In the preceding remarks I have confined myself substantially to the task of making clear the points at issue between my opponents and myself. In adopting my views of the exogenous structure of the stems in question, I am but following in the steps of some of the ablest of living botanists. M. Adolphe Brongniart, than whom no higher authority can be named, not only adopts the exogenous theory, but is so deeply impressed with its force that he denies the probability of many of the plants in question having been cryptogamic. He places them amongst the gymnospermous exogens. Recent events, however, have shown that though exogenous they are true cryptogams. How absurd, then, to apply to such stems the term acrogen or acrobrya! This controversy must be ultimately settled by the logic of facts, not by vague opinions, and to these I confidently appeal. The details of my proposed classification can only be discussed when all the facts are before the public. When this is the case, I hope to show that my proposition not only does no violence to the true affinities of living cryptogams, but that, in bringing the ancient and modern types into a philosophical relationship, it accomplishes what, under existing systems of classification, it is impossible to do.

W. C. WILLIAMSON

## *METEOROLOGY IN AMERICA* *

### II.—Organisation of the United States Signal Service.

THERE are probably few departments of the Executive of the United States which have been of such essential practical value as the Signal Service; and among those who have been instrumental in establishing it, we cannot avoid mentioning the names of the Hon. Halbert E. Paine of Wisconsin, the Hon. Henry L. Dawes of Massachusetts, and the Hon. William W. Belknap, Secretary of War.

It may be added that, without distinction of party, the whole people of the country, the press, both Houses of Congress, and the President, have earnestly sustained and advanced this important branch of the public service.

The military system is one of the most valuable features in the constitution of this Signal Service for the benefit of Commerce. The advantages of having the whole corps of weather observers in the army are manifest and manifold. Each observer feels the responsibility of a sentinel at his post, which begets in him a sentiment of devotion to duty the strongest of which men are capable, and which has often led the soldier to imitate the example

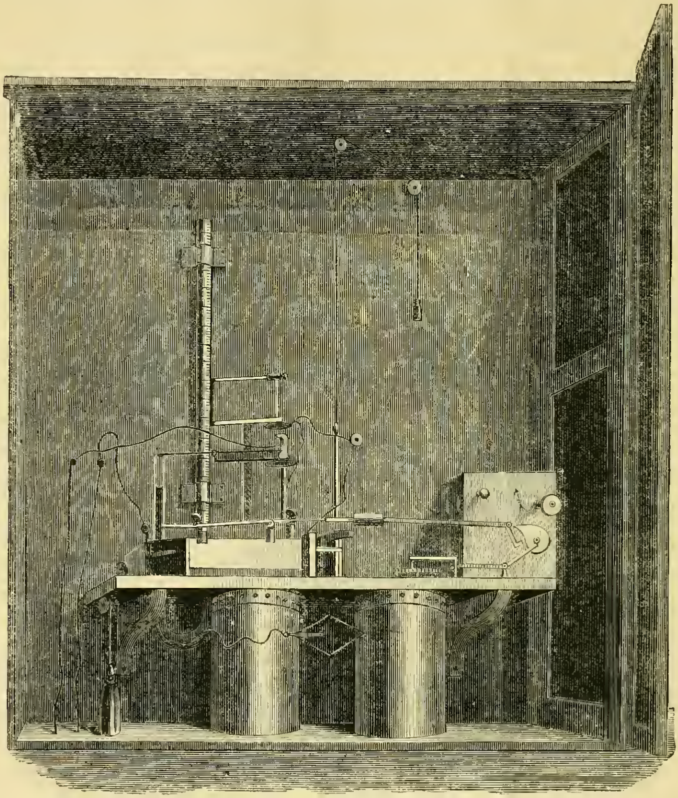

FIG. 4—PROF. HOUGH'S NEW PRINTING BAROMETER

of the Roman guard at Pompeii, who, after nearly eighteen centuries, was taken from its ruins in his martial position, showing that he had not fled before the molten flood from Vesuvius. Experience has proved what the sense of the Government originally suggested, that observations would be most punctually and scrupulously taken at the different stations by men accustomed to the discipline and obedience, even in minutest details, of army subalterns.

They are required to work out no difficult problems in meteorology, but simply to observe and record the indications of their instruments, and to transmit the same without delay or inaccuracy. In doing this work, they have become by tri-daily practice as expert and exact in reading the glasses as any of our veteran scientific men—indeed, as much so as a Fitzroy or a Leverrier could be.

Regarding the Signal Corps scattered through and over all parts of the country, we may compare it to a regiment on drill three times a day, the telegraph instantly revealing to the commanding officer, General Albert J. Myer, at Washington, the slightest failure in any observer.

By this now widely spread and magnificently organised system, the United States army, engaged under the chief

* We are again indebted to *Harper's Weekly* for the continuation of the article by Prof. Maury, and the woodcuts which we reproduce this week.

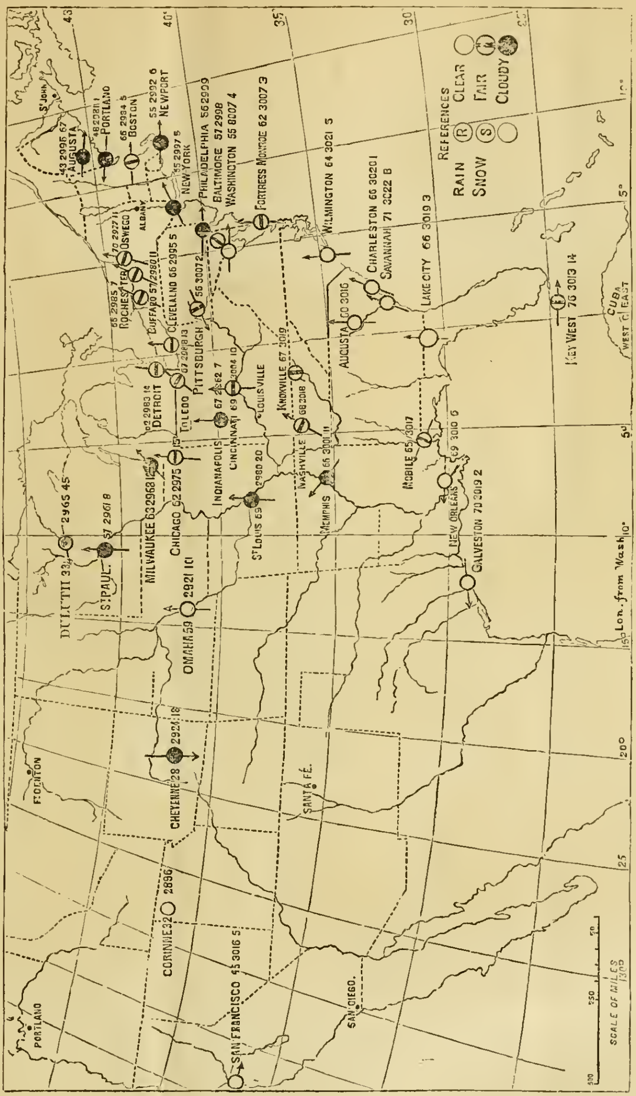


FIG. 5.—WAR DEPARTMENT WEATHER MAP (SIGNAL SERVICE, U.S.A.), SATURDAY, APRIL 8, 1871, 7 35 A.M., WASHINGTON
*The Numerals denote: 1st, the State of the Thermometer; 2nd, that of the Barometer; and, 3rd, the Force of the Wind.*

signal officer, is in time of peace undergoing a thorough training in the art of telegraphy and signalling, at the same time that it is passing through a most thorough discipline, is being educated to science, and also serving one of the most important ends ever devised for the benefit of commerce.

At Fort Whipple, Virginia, every man is taught to use the telegraph, and to become a skilful operator. He thus has a profession at all times lucrative to himself wherever he may be afterwards thrown. The training, skill, and habits of exactness acquired by the Signal Corps in time of peace will be of the greatest value to the army in time of war. The telegraph is capable of indefinite utilisation. General Von Moltke, it is well known, conducted the late operations of the German army on the battle-fields of France sitting in the rear with his map before him, and his telegraphic operator at his side, keeping him in communication with all parts of the field. It has been frequently said by distinguished military men that the telegraph will be one of the most effective weapons in any war that may now occur How necessary for the Government to keep up the efficiency of such a corps as that of which we have spoken!

As the organisation under General Myer now exists, the President and Secretary of War have a responsible military man at every important post in the country. If a warlike expedition appears on any part of our coast, causing a panic or stampede, there may be a thousand wild rumours of frightened message-senders. The Government, however, is in the receipt every eight hours (and can be in the receipt every hour if it wishes) of a reliable

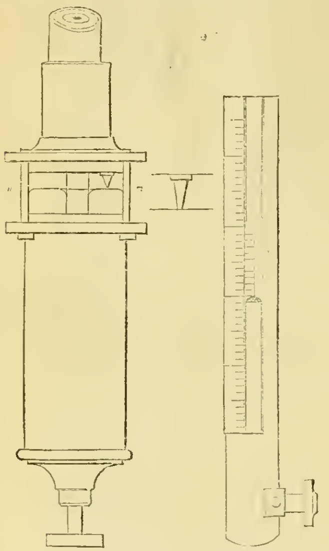

FIG. 6.—SECTION OF GREEN'S STANDARD BAROMETER

message from its own agent, who reports on his responsibility what he saw and knows to be true; and this observer will not leave his post until ordered to do so. As a mere Government police, therefore, the Signal Corps would be worth to the nation far more than it can ever cost, even if its operations should be more widely extended, as will speedily be done.

Each sergeant is sent to the Signal Service school for instruction at Fort Whipple, Virginia, where he is immediately supplied with Loomis's "Text-book of Meteorology," Buchan's "Hand-book of Meteorology," Pope's "Practical Telegraphy," and the "Manual of Signals for the United States Army," together with all the instruments necessary for practical instruction. The books he must thoroughly master. He is required to cite once daily didactically, and to practise a certain time with the instruments. He is required to remain under tuition until considered by the instructor competent to take charge of a station and perform the necessary duties, when he is ordered before a board, consisting of three army officers, for examination, when, if considered incompetent, he is returned to Fort Whipple for further instruction and practice.

If, after a rigid examination, he is found capable, he is assigned to a station, and the necessary stationery and instruments furnished him (the latter consisting of the barometer, thermometer, hygrometer, anemoscope, anemometer, and rain-gauge), and instructions to make three observations daily, viz., at the time corresponding with 7·35 A.M., 4.35 P.M., and 11.35 P.M. Washington time, so that every observer at each station should be reading his instruments at the same moment, and in the following order, viz, 1st, barometer; 2nd, thermometer; 3rd, hygrometer; 4th, anemoscope; 5th, anemometer; and 6th, rain-gauge.

In addition to the duties discharged by the officers of the Examining Board, Colonel Mallery, A.S.O., has the general charge of the very large correspondence of the office; Captain Howgate has charge of the statistics and all observations of the service; and Lieutenant Capron has the difficult post of instructor of sergeants at Fort Whipple.

Where a single person has been required to do the work of a station, receiving full reports from all stations, the labour occupied twenty hours out of the twenty-four. But the rule now adopted is to provide each station with two men—one a sergeant in charge and the other a private soldier as assistant. The observer stationed on Mount Washington has been alone on the mountain most of the time, and always responsible for the work.

In addition to a number of officers who form the Board of Examination, General Myer is also ably assisted by Major L. B. Norton, the property and disbursing officer of the Signal Service.

Prof. Cleveland Abbé, long known as an officer of the Cincinnati Observatory, and as an eminent meteorologist, is employed chiefly in the work of making out the daily synopsis of the weather, and deducing therefrom the weather "probabilities," which are given to the public by telegram through all newspapers desirous of furnishing them to their readers.

To the conspicuous ability of all of these officers is attributable the success of the enterprise.

The ordinary barometer in use by Signal Office observers is that of Mr. James Green (the well-known scientific instrument maker of New York)—an instrument adopted by the Smithsonian Institution, and also by the American navy, as the most perfect to be obtained.

This barometer has its cistern furnished with a small glass index, which shows when the mercury is at the right height in the cistern. This is adjustable by a screw which works through the bottom of the instrument against the flexible bottom of the cistern. The instrument is ready for use when the mercury touches the little V-shaped index in the cistern. So simple and complete is this barometer that any one can use it, and it ought to be in the hands of all business gentlemen, and all who are interested in watching the mutations of the weather.

Latitude and longitude on the earth's surface mark very conspicuous differences in the mean barometrie pressure, as will be seen by a study of the Isobarometric Chart for the United States, which we gave last week.

The barometer has a slight fluctuation also under several influences. It rises when the moon is on the meridian in some places. It has a diurnal oscillation, amounting on the equator to more than one-tenth of an inch, but in the latitude of New York to only 0·05 inch, the greatest height being about ten A.M., and the least about four P.M. The nocturnal variations are much less. In the latitude of Philadelphia and New York the north-east wind causes another variation of one-fourth of an inch, due to the meeting of two atmospheric waves giving a still higher wave, and hence a higher barometer. There is also the variation due to the height of the observer's station above the sea. This is, of course, of the first importance. The other fluctuations are comparatively unimportant, and do not blind an observer to those ominous fluctuations which precede the storm, the tornado, and the hurricane. The oscillations which indicate a storm are very marked. The tornado which recently ravaged St. Louis was preceded by a gradual fall of the mercury in the barometer, for thirty hours previous, of an entire inch. At Boston, within thirty-seven years, the barometer has ranged from 31·125 inches to 28·47 inches, the difference being 2·655 inches. At London it has ranged through more than 3·5 inches; but in the tropics not so much.

During the passage of a cyclone the mercury oscillates rapidly. The most noticeable fall occurs from four to six hours before the passage of the storm centre. This fall is often over an inch, and sometimes two inches.

Great changes are usually shown by falls of barometer exceeding half an inch, and by differences of temperature exceeding fifteen degrees. If the fall equals one-tenth of an inch an hour we may look out for a heavy storm. The more sudden the change the greater the danger. But it is too often forgotten that the fall of the mercury is a forewarning of what will occur in a day or two, rather than in a few hours.

A variation of an inch is certain to be followed by a tornado or violent cyclone. In the tropics "the glass" has been known to show a fall of more than an inch and a half in one hour!

The following guides in predicting weather changes are selected from the "Barometer Manual" of the London Board of Trade, and are suggestive:

I. If the mercury standing at thirty inches rise gradually while the thermometer falls, and dampness becomes less, N.W., N., or N.E. wind; less wind or less snow and rain may be expected.

II. If a fall take place with a rising thermometer and increasing dampness, wind and rain may be expected from S.E., S., or S.W.; a fall in winter with a low thermometer foretells snow.

III. An impending N. wind before which the barometer often rises may be accompanied with rain, hail, or snow, and so forms an apparent exception to the above rules, for the barometer always rises with a north wind.

IV. The barometer being at 29½ inches, a rise foretells less wind or a change of it northward, or less wet. But if at 29 inches a fast first rise precedes strong winds or squalls from N.W., N., or N.E., after which a gradual rise with falling thermometer, a S. or S.W. wind will follow, especially if the rise of the thermometer has been sudden.

V. A rapid barometric rise indicates unsettled, and a rapid fall stormy weather with rain or snow; while a steady barometer, with dryness, indicates continued fine weather.

VI. The greatest barometric depressions indicate gales from S.E., S., or S.W.; the greatest elevations foretell wind from N.W., N., or N.E., or calm weather.

VII. A sudden fall of the barometer, with a westerly wind, is sometimes followed with a violent storm from the N.W., N., or N.E.

VIII. If the wind veer to the S. during a gale from the E. to S.E., the barometer will continue to fall until the wind is near a marked change, when a lull may occur. The gale may afterward be renewed, perhaps suddenly and violently; and if the wind then veer to the N.W., N., or N.E., the barometer will rise and the thermometer fall.

IX. The maximum height of the barometer occurs during a north-east wind, and the minimum during one from the south-west; hence these points may be considered the poles of the wind. The range between these two heights depends on the direction of the wind, which causes, on an average, a change of half an inch; on the moisture of the air, which produces in extreme cases a change of half an inch; and on the strength of the wind, which may influence the barometer to the extent of two inches. These causes, separately or conjointly with the temperature, produce either steady or rapid barometric variations, according to their force.

PRESENT OPERATION OF THE SERVICE

Although the Signal Service is yet in its infancy, and must be patiently nursed and cherished by the people for some years before it can expect to do and discharge its full mission, under General Myer's indefatigable care and skilful management it has already achieved much good, and more than compensated the public for the expense of its establishment. Since it was instituted last summer, "the chief signal officer has," to quote the words of the *New York World*, "thoroughly organised and equipped a system which now embraces in its scientific grasp every part of the land from Sandy Hook to the Golden Gate of California, and from Key West to the Dominion of Canada."

Three times every day synchronous observations are taken and reports made from the stations—one at eight A.M., one at four P.M., and the third at midnight. These observations are made by instruments, all of which are perfectly adjusted to a standard at Washington. They are also all taken at the same moment exactly, these observations and reports being also timed by the standard of Washington time. The reports from the stations are transmitted in full by telegraph. By a combination of telegraphic circuits, the reports of observations made at different points synchronously are rapidly transmitted to the different cities at which they are to be published. They are, however, all sent of course to the central office in Washington. These reports are limited to a fixed number of words, and the time of their transmission is also a fixed number of seconds. These reports are not telegraphed in figures, but in words fully spelled out. There are now about forty-five stations for which provision has been made, and which are in running order. These have been chosen or located at points from which reports of observations will be most useful as indicating the general barometric pressure, or the approach and force of storms, and from which storm warnings, as the atmospheric indications arise, may be forwarded with greatest despatch to imperilled ports.

These stations are occupied by expert observers furnished with the best attainable instruments, which are every day becoming more perfect, and to which other instruments are being added.

The reports of observers are as yet limited to a simple statement of the readings of all their instruments, and of any meteorological facts existing at the station when their tri-daily report is telegraphed to the central office in Washington.

Each observer at the station writes his report on manifold paper.* One copy he preserves, another he gives to the telegraph operator, who telegraphs the contents to Washington. The preserved copy is a voucher for the report actually sent by the observer; and if the operator is careless and makes a mistake, he cannot lay the blame on the observer, who has a copy of his report, which must be a fac-simile of the one he has handed to the operator. The preserved copy is afterwards forwarded by the observer-sergeant to the office in Washington, where it is filed, and finally bound up in a volume for future reference.

When all the reports from the various stations have been received they are tabulated and handed to the officer (Prof. Abbé) whose duty it is to write out the synopses and deduce the "probabilities," which in a few minutes are to be telegraphed to the press all over the country.

* Thin paper with black carbon paper between the sheets. The pen is a dry *stylus*, and being pressed on the upper sheet, it makes a similar mark on the sheets beneath it.

This is a work of thirty minutes. The bulletin of "probabilities," which at present is all that is undertaken, is made out thrice daily, in the forenoon, afternoon, and after the midnight reports have been received, inspected, and studied out by the accomplished gentleman and able meteorologist who is at the head of this work.

The "probabilities" of the weather for the ensuing day, so soon as written out by the professor, are immediately telegraphed to all newspapers in the country which are willing to publish them for the benefit of their readers.

Copies of the telegrams of "probabilities" are also instantly sent to all boards of trade, chambers of commerce, merchants' exchanges, scientific societies, &c., and to conspicuous places, especially sea-ports, all over the country.

While the professor is preparing his bulletins from the reports just furnished him by telegraph, the sergeants are preparing maps which shall show by arrows and numbers exactly what was the meteorologic condition of the whole country when the last reports were sent in. These maps are printed in quantities, and give all the signal stations. A dozen copies are laid on the table with sheets of carbon paper between them, and arrow stamps strike in them (by the manifold process) the direction of the wind at each station. The other observations as to temperature, barometric pressure, &c., &c., are also in the same way put on them.

These maps are displayed at various conspicuous points in Washington—*e.g.*, at the War Department, Capitol, Observatory, Smithsonian Institution, and office of the chief signal officer. They serve also as perfect records of the weather for the day and hour indicated on them, and are bound up in a book for future use.

Every report and paper that reaches the Signal Office is carefully preserved on file, so that at the end of each year the office possesses a complete history of the meteorology of every day in the year, or nearly 50,000 observations, besides the countless and continuous records from all of its self-registering instruments.

When important storms are moving, observers send extra telegrams, which are despatched, received, acted upon, filed, &c., precisely as are the tri-daily reports. One invaluable feature of the system as now organised by General Myer is that the phenomena of any particular storm are not studied some days or weeks after the occurrence, but while the subject is fresh in mind. To the study of every such storm, and of all the "probabilities" issued from the office, the chief signal officer gives his personal and unremitting attention. As the observations are made at so many stations, and forwarded every eight hours, or oftener, by special telegram from all quarters of the country, the movements and behaviour of every decided storm can be precisely noted; and the terrible meteor can be tracked and "raced down" in a very few hours or minutes. A beautiful instance of this occurred on the 22nd of February last, just after the great storm which had fallen upon San Francisco. While it was still revolving around that city, its probable arrival at Corinne, Utah, was telegraphed there, and also at Cheyenne. Thousands of miles from its roar, the officers at the Signal Office in Washington indicated its track, velocity, and force. In twenty-four hours, as they had forewarned Cheyenne and Omaha, it reached those cities. Chicago was warned twenty hours or more before it came. Its arrival there was with great violence, unroofing houses and causing much destruction. Its course was telegraphed to Cleveland and Buffalo, which, a day afterwards, it duly visited. The president of the Pacific Railroad has not more perfectly under his eye and control the train that left San Francisco to-day than General Myer had the storm just described.

While the observers now in the field are perfecting themselves in their work, the chief signal officer is training other sergeants at the camp of instruction (Fort Whipple, Virginia), who will go forth hereafter as valued auxiliaries. It has been fully demonstrated by the signal officer that the army of the United States is the best medium through which to conduct most efficiently and economically the operations of the Storm Signal Service. Through the army organisation the vast system of telegraphy for meteorological purposes can be, and is now being, most successfully handled. "Whatever else General Myer has not done," says the *New York World*, "he has demonstrated that there can be, and now is, a perfect network of telegraphic communication extending over the whole country, working in perfect order, by the signalmen, and capable of furnishing almost instantaneous messages from every point to the central office at Washington. Think of a single jump by wire from San Francisco 2,700 miles eastward three times a day! When General Myer undertook to put this system in working order, the telegraph companies said it was impossible—no such thing had ever been heard of in telegraphing. It is now a grand *fait accompli*, as much as the passing of the Suez Canal by ships or the escaping from Paris by balloons." *

At present the signal officer aims only to give a synopsis of each day's weather, and a statement of what weather may be expected or will probably occur. The "probabilities" so far have been most beautifully verified and confirmed.

It is not thought wise to undertake more than can be securely accomplished. The synopses and "probabilities" are all that intelligent shippers and careful seamen require. Shippers will not send their vessels to sea if the weather synopsis indicates threatening or alarming weather.

Travellers can consult the "probabilities" before leaving home; and any severe storm that menaces any city or port is now specially telegraphed thither, and the announcement is made by bulletins posted in the most public places.

By the modest estimate of the signal officers, the following is a table showing percentage of "probabilities" that have been verified:

| | |
|---|---|
| Fully verified . . . . | 50 per cent. |
| Verified in part . . . | 25 ,, |
| Failed . . . . . . | 25 ,, |

It must, however, be borne in mind that the failures have often been due to lack of information from points where as yet no observer-sergeant is stationed.

FUTURE AIMS

The Signal Service has, up to this time, acted upon the wise maxim of "making haste slowly," and undertaking to do nothing which was not in its power to do safely and securely without risk of failure. It has acted upon the confidence it has in the people that they will patiently await the development of solid science, meantime leaving no stone unturned to hasten forward the observations which may lead to a more exact acquaintance with the habits, movements, and tracks of our American storms. Great progress has in a very short time been made in this knowledge, and every day new light is dawning upon the science of storms.

The instruments of the service have been bought on trial. They are undergoing the most varied experiments. In a short time, it is hoped, they will be greatly improved and perfected, and then the chief signal officer's results will be more satisfactory to himself, and his labours will be greatly facilitated. The celerity with which important results have already been attained by this officer has surprised and startled both himself and the friends of the great movement.

As soon as possible, therefore, the Signal Office will have its signal posts along the lakes and on our Atlantic sea-board, where cautionary signals will be displayed, warning vessels of approaching gales and storms, and

* *New York World*, March 5, 1871.

also a signal for clear weather. These will be displayed by day and by night by a very simple and suitable contrivance now being perfected by General Myer. In New York already arrangements have been made for displaying the signals to shipping in the harbour from a lofty structure on the roof of the Equitable Life Insurance Company's office, the best station that could be chosen. The display of these storm signals proper will place the American Signal Bureau at once in a position to render inestimable service to shipping and all commercial interests.

These signals will at first be neglected by ruder and more unskilful seamen and shippers; but, as in the case of the famous Fitzroy signals on the English coast, every week will add new demonstrations of the value and utility of this system—one of the most splendid gifts bequeathed by modern science to the human race.

The signalling of storms and desolating cyclones to the unsuspecting seaman will, it is believed, mark a new era in our lake and coast navigation, and be the means of annually saving many lives and millions of dollars' worth of our floating property.

The comparison of these signals with the weather following the signals will be then a matter of special attention. Every discrepancy can then be carefully noted and probed, and every day the meteorologists in charge of the "probabilities" will find the means of rectifying any errors they may have fallen into, and daily increasing the accuracy and perfecting the plan of their forecasts.

The storm signals will be displayed at any hour of the day or night when the instrumental indications give notice of bad weather; and experience has already shown that generally at least twenty-four hours' forewarning can be given from the central office in Washington of all important weather phenomena. With the telegraph to premonish, forecasts for two or three days in advance are hazardous and unnecessary. For almost all practical purposes of life a day's notice of atmospheric disturbances is quite sufficient, and more reliable than longer premonitions. It will be a grand triumph for American science when the electric telegraph is so utilised that it will bring all citizens of the United States into electric communication with each other, and the most fearful storm, as well as the sunshine and shower, shall be every day a subject of forewarning or congratulation throughout the land, and even on the lakes and oceans that wash the American coasts.

## *OPENING OF THE MONT CENIS TUNNEL*

THE project of constructing a tunnel under the Alps —one of the favourite designs of that ardent patriot and eminent statesman, the late Count Cavour—has now been accomplished, thanks to the skill of the Italian engineers. The scientific requirements and methods adopted are well stated in a recent article in the *Daily News*, to which we are indebted for the following interesting particulars:—

The tunnel was commenced on the 15th of August, 1857. The two points at which it was determined to begin the boring were two wretched little Alpine villages, Bardonnecchia and Fourneaux, the former on the Italian, the latter on the French side of Mont Fréjus, the tunnel being nearly pierced under the above-named mountain, and not, as common report would have it, beneath Mont Cenis. These two villages were of the smallest size and most miserable character, and offered no accommodation whatever to the many hundred workmen employed on either side the mountain. Bardonnecchia, on the Piedmontese side, is a village which, in 1857, when the works commenced, contained about 1,000 souls. The houses in it were really little better than huts, being mostly occupied by shepherds, who were absent with their flocks on the mountains during the summer months. At Fourneaux things were even worse, there being an ordinary population of only 400 inhabitants.

The first problem to be solved, says Mr. Fras. Kossuth, one of the Royal Commissioners of Italian Railways, in his able report on the Mont Cenis Tunnel, was threefold. (1) To fix across the mountain several points which would all be contained in the vertical plane drawn through the axis of the tunnel. (2) To obtain the exact length between the openings. (3) To know the precise difference of level between the two extremities of the tunnel, so as to obtain the proper gradients. In order to execute this programme, a series of observations was established on all the favourable points, and an elaborate trigonometrical survey of the district was commenced. By the end of the season little could be done in the way of surveying; in the winter of the year 1858 all the surveys relating to the alignment and to the length of the tunnel were completed, and all was ready to compile the longitudinal section along the axis of the tunnel The whole system consisted of twenty-eight triangles, and eighty-six was the number of measured angles. All of these were repeated never less than ten times, the greater part twenty, and the most important as many as sixty times. To give the reader an idea of the extraordinary care and accuracy with which the surveying operations were carried out, it may be mentioned that Signor Mondino repeated his experiments for obtaining the level of the tunnel, or rather of the signals over the mountain in 1857 and 1858, and the difference in the two surveys (over more than 13,000 yards), was only 3·93 inches. Even this was reduced afterwards by Signor Termine to 1·57 inch. The preliminary measurements gave a distance of 13,861·5 yards between the two temporary openings We say temporary openings, because, although the tunnel is itself constructed in a perfectly straight line from Fourneaux to Bardonnecchia, passengers will not pass through the original straight tunnel, but will be conveyed through a branch one which joins the main line a short distance from Fourneaux. The nature of the ground was such as to necessitate the definite and permanent tunnel being taken through the mountain in a curve; but even the unprofessional reader will see that a straight line was indispensable, in order to secure not only accuracy of direction, but also a through draught of air through the whole length of the tunnel. A most important consideration this latter, as one of the main objections brought against the scheme was the supposed difficulty there would be in keeping the tunnel thoroughly well ventilated. It was also much easier to transmit the necessary motive power along a straight line than on a curve. The tunnel, although its axis was straight, was not constructed on a dead level. The gradients were: From the Bardonnecchia (Italian) end, 4,408·50 feet above the level of the sea, 1 in 2,000 (·0002 per metre) for a distance of 20,997·33 feet. From the Fourneaux entrance (French side), 3 945 feet above the sea, the rising gradient was 1 in 43 4782 (·023 per metre) for 20,587 feet.

The absolute figures are as follows:

Total length of the tunnel, 13,364·86 yards.

| | |
|---|---|
| Elevation above the sea-level of the Bardonnecchia entrance . . . . . . . . | 4,381·25 |
| Rise of gradient of 1 in 2,000 for 20,048 feet . . | 10·024 |
| Summit level from Bardonnecchia | 4,391·274 |
| Elevation above sea-level at Fourneaux entrance . | 3,946·50 |
| Rise of gradient of 1 in 45,045 for 200,045·10 feet. | 445·00 |
| Summit level from Fourneaux . | 4,391·50 |

This shows a very slight difference from the calculations of the summit level as reckoned at Bardonnecchia, and gives a mean level for the highest point of 4,391·386 feet. The greatest height of the mass of the Alps over the tunnel is 5,307 feet.

After giving these figures, it may be of interest to present the reader with the account given by an eye-witness, M. Génési, of the meeting of the workmen last winter in the depths of the earth, more than 5,000 feet beneath the summit of Mont Fréjus. "On the 9th of November, 1871,' says M. Génési, "I was on my regular round of inspection as usual, when I fancied I heard through the rocks the noise of the explosion of the mines on the Bardonnechia side. I sent a dispatch to discover if the hours agreed. They did, and then there could be no longer any doubt we were nearing the goal. Each following day the explosions were to be heard more and more distinctly. At the beginning of December we heard quite clearly the blows of the perforators against the rocks. Then we vaguely heard the sound of voices. But were we going to meet at the same level and in the same axis? For three days and three nights engineers, foremen, and heads of gangs never left the tunnel. The engineers Borelly and Boni directed the works on the Bardonnechia side, M. Copello on that of Fourneaux. We could not eat or sleep; every one was in a state of fever. At length, on the morning of the 26th December, the rock fell in near the roof. The breach was made, and we could see each other and shake hands. The same evening the hole was clear—the last obstacle—and the mountain was pierced, our work was done. What a rejoicing we had! In spite of the war, the cheers of all scientific Europe came to find us in the entrails of our mountain when the happy termination of our enterprise became known. The two axes met almost exactly; there was barely half a yard error. The level on our side was only 60 centimetres (less than three-quarters of a yard) too high. But after thirteen years of continual work, who could even hope for so perfect a result? We placed at the point of junction an inscription on a marble tablet, commemorative of the happy event."

How was the happy event brought about? For the variation of less than a yard in more than 13,000 is surely one of the triumphs of modern engineering skill. We cannot do better than borrow the description of the method pursued given by Mr. Kossuth:—"The observatories placed at the two entrances to the tunnel were used for the necessary observations, and each observatory contained an instrument constructed for the purpose. This instrument was placed on a pedestal of masonry, the top of which was covered with a horizontal slab of marble, having engraved upon its surface two intersecting lines marking a point, which was exactly in the vertical plane containing the axis of the tunnel. The instrument was formed of two supports fixed on a tripod, having a delicate screw adjustment. The telescope was similar to that of a theodolite, provided with cross webs and strongly illuminated by the light from a lantern, concentrated by a lens, and projected upon the cross webs. In using this instrument in checking the axis of the gallery at the northern entrance, for example, after having proved precisely that the vertical flame, corresponding with the point of intersection of the lines upon the slab, also passed through the centre of the instrument, a visual line was then conveyed to the station at Lachalle (on the mountain), and on the instrument being lowered the required number of points could be fixed in the axis of the tunnel. In executing such an operation it was necessary that the tunnel should be free from smoke or vapour. The point of collimation was a plummet suspended from the roof of the tunnel by means of an iron rectangular frame, in one side of which a number of notches were cut, and the plummet was shifted from notch to notch, in accordance with the signals of the operator at the observatory. These signals were given to the man whose business it was to adjust the plummet by means of a telegraph or a horn The former was found invaluable throughout all these operations. At the Bardonnecchia entrance the instrument employed in setting out the axis of the tunnel was similar to the one already described, with the exception that it was mounted on a little carriage, resting on vertical columns that were erected at distances 500 metres apart in the axis of the tunnel. By the help of the carriage the theodolite was first placed on the centre line approximately. It was then brought exactly into line by a fine adjustment screw, which moved the eye-piece without shifting the carriage. In order to understand more clearly the method of operating the instrument, the mode of proceeding may be described. In setting out a prolongation of the centre line of the tunnel, the instrument was placed upon the last column but one; a light was stationed upon the last column, and exactly in its centre, and 500 metres ahead a trestle frame was placed across the tunnel. Upon the horizontal bar of this trestle several notches were cut, against which a light was placed and fixed with proper adjusting screws. The observer standing at the instrument caused the light to move upon the trestle frame until it was brought into an exact line with the instrument and the first light, and then the centre of the light was projected with a plummet. In this way the exact centre was found. By a repetition of similar operations the vertical plane containing the axis of the tunnel was laid out by a series of plummet lines. During the intervals that elapsed between consecutive operations with the instruments, the plummets were found to be sufficient for maintaining the direction in making the excavation. To maintain the proper gradients in the tunnel it was necessary at intervals to establish fixed levels, deducing them by direct levelling from standard bench marks placed at short distances from the entrances. The fixed level marks in the inside of the tunnel are made upon stone pillars placed at intervals of 25 metres, and to these were referred the various points in setting out the gradients."

There will be two lines of rail in the tunnel. The vault itself will be six metres high and eight metres wide. The tunnel will be walled in along its whole length, and the lime rock will be nowhere exposed. The thickness of the internal masonry forming the tube is from half a yard to a yard and more, according to circumstances. On the French side the masonry cost on the average 1,300 francs the square metre. On the Italian side only 1,000 francs. The tunnel is wonderfully dry in comparison with many smaller works. There is only one subterranean spring of any importance in it. A water course, or rather aqueduct, has been constructed beneath the permanent way, in order to carry off any water which might drain into the tunnel.

Much has been said about the heat in the tunnel. All accounts agree that it is not excessive, and a recent French visitor to the tunnel gives the following figures:—At the entrance, 54° Fahrenheit; at the telegraph station inside, 76° Fahrenheit; the average temperature being about 65° Fahrenheit.

## *NOTES*

THE first session of the Newcastle-on-Tyne College of Physical Science will be opened by inaugural addresses from Professors Herschel, Aldis, Page, and Marreco, from the 9th to the 12th of October. The examination for the four exhibitions will be held on the 13th and 14th. On the 19th the Inaugural Ceremony will take place, when the Dean of Durham will deliver an address; after which the successful candidates for the exhibitions will be named. Further particulars are given in our advertising columns.

WE announced some time ago that the Council of the Working Men's College, in Great Ormond Street, was proposing a larger infusion of Science in the programme of the College course; and we are now very glad to be able to state that during the next term, which will commence on October 2, courses of lectures will be given on Geology, by Mr. J. Logan Lobley; on the Use of the Microscope, by Mr. J. Slade; and on Physiology, by Mr. J. Beswick Perrin. Students entering for the course on Geology will have the privilege of attending the ordinary and field meetings of the Geologists' Association. Among the Saturday General Lectures one will be delivered by Prof. W. H. Flower, of the Royal College of Surgeons. No more useful work could be performed than that so generously offered by these gentlemen, who give up their time to the scientific instruction of the working classes in London. We venture at least to predict that they will be rewarded by intelligent and appreciative audiences.

DR. ALLEYNE NICHOLSON, late Lecturer on Natural History in the Medical School of Edinburgh, has been appointed to the Chair of Natural History in the University of Toronto.

A SPECIAL prize was established a few years ago by the French Academy, for the best translation delivered to that body. This prize was awarded in the sitting of the 17th of August to the author of a translation of Mr. Grote's "History of Greece," published by Lacroix. Mr. Grote was an associate member of the Académie des Sciences Morales et Politiques.

LETTERS from Switzerland state that M. Gerlach, a distinguished Swiss engineer and geologist, was fatally injured on the 7th in a fall from the mountains of the Upper Valais, and died next day in the village of Oberwald The deceased gentleman was the author of several remarkable works relative to surveys and explorations in the Swiss Alps.

THE *Times of India*, of August 22, asserts that news has been received from Zanzibar that Dr. Livingstone had again been heard of to the west of Lake Tanganyika, whence he had sent to Ujiji, requesting his supplies to be forwarded. A young American was hurrying on by forced marches to Ujiji, in the hope of carrying relief to the traveller. The intelligence appears, however, to want confirmation.

THE Council of the Institution of Civil Engineers publish a list of forty-three special subjects, on which they invite communications for the approaching session, as well as upon others; such as: "*a* Authentic details of the progress of any work in Civil Engineering, as far as absolutely executed (Smeaton's Account of the Eddystone Lighthouse may be taken as an example). *b*. Descriptions of engines and machines of various kinds. *c*. Practical essays on subjects connected with Engineering, as, for instance, Metallurgy. *d*. Details and results of experiments or observations connected with Engineering Science and Practice. For approved original communications, the Council will be prepared to award the premiums arising out of special funds devoted for the purpose.

THE *Maidstone Journal* mentions that an educational effort of considerable promise is about to be made in that town. Several gentlemen have arranged to conduct junior classes during the evenings of the winter months, the subject being Physical Geography. Three hundred pupils from the senior classes of schools in Maidstone have already entered. The course will consist of thirty lectures, and the pupils will be educated up to the standard of the Educational Department at South Kensington. The lectures will be free.

PROF. HUXLEY has been lately engaged in inspecting and arranging the valuable reptilian and other remains from the Upper Elgin sandstones now placed in the Dundee Museum He has also been superintending some excavations at Lossiemouth, in order, if possible, to obtain materials for completing the structure of the huge Saurian, *Stagonolapis Robertsoni*, a full account of which is expected to appear before the Royal Society shortly.

A FEW years ago the existence of a new Tapir on the Isthmus of Panama was first made known by American naturalists. This animal departs so widely from the ordinary American Tapir in certain anatomical characters (particularly in the possession of a completely ossified septum between the nostrils, as in the Tichorhine Rhinoceros) that Prof. Gill (its describer) thought it necessary to make it the type of a new genus, calling it *Elasmognathus Bairdi*, after the distinguished assistant secretary of the Smithsonian Institution. The Zoological Society of London have just added to their living collection a fine young male specimen of that animal, which has been placed in the elephant house along with an example of the *Tapirus americanus*.

THE German surveying ship, the *Pomerania*, returned from her cruise in the Baltic on the 24th of August, after making some very interesting discoveries. She crossed the Baltic in different directions several times, and during these journeys soundings were carefully taken, the bottom was dredged, and the surface and deep-water currents observed, and the temperature of the water at the surface and at some depth was also carefully noted. These results will shortly be published in full, but a few details have already appeared. The greatest depth between Gothland and Windau was found to be 720 feet, and not 1,110 feet as formerly supposed. At the depth of from 600 to 720 feet, at the latter end of July, the temperature was only from 0·5° to 2° R. No marine plants were met with in this cold area, and only a few annelids were dredged up. Life was very abundant to a depth of about 300 feet, whilst plants were seldom found at a depth of more than 30 feet. Both animal and vegetable life were found to be most abundant on the coasts of Mecklenburg-Schleswig and Holstein and in the bay of Lübeck.

AS an addition to the list of exploring expeditions tending either directly or indirectly to develop a knowledge of the natural and physical features of the North American continent, *Harper's Weekly* states that a party of civil engineers has lately been organised at Victoria to survey a route for a proposed railroad through British Columbia and the Red River country to Canada. This is stated to be provided with ample means for the purpose of making a minute geographical reconnoissance of the country, and is expected to add much to our knowledge of the general geology of the continent.

GREAT geological changes are reported from the districts adjoining the Caspian Sea and the river Ural. During the last ten years the surface of the water in the river has sunk more than a foot, and many bogs on the North Eastern coast of the Caspian have entirely disappeared The delta of the Ural has diminished from nineteen to five branches, and whereas it formerly occupied one hundred versts, it now occupies only seven. Many islands have become joined to the mainland, and large sandbanks have been formed at the mouth of the river. The town of Guryer, formerly on the sea coast, is now six versts inland.

WE have now full details of the severe cyclone which visited Antigua, St. Kitts, St Bartholomew, St. Martins, Tortola, St. Thomas, and Porto Rico, on the 21st of August. The heaviest gusts of wind were felt at St Thomas between 4.30 and 5 P.M., and about 5 o'clock there was a sudden calm; the centre of the cyclone then passing over the island, and by 7 the violence of the wind had ceased. The damage done in all these islands is excessive; in St. Thomas the losses are returned at forty-two persons killed, seven-nine seriously injured, and 420 houses completely destroyed. At Antigua the cyclone was very severe, eighty persons are reported killed, and several hundred wounded. Scarcely a house or plantation in the island has escaped damage. Every place is "bleak, bare, and desolate." No confirmatory accounts are given of the earthquake shocks said in the first telegram to have accompanied the cyclone; they are probably due only to exaggeration.

Mr. Thomas Gray, the Marine Secretary of the Board of Trade, has collected a sum of 200*l.* as a prize for the most efficient and most simple green light for the starboard side of ships that shall fulfil the Board of Trade conditions, which require that it shall be of sufficient power to be seen on a dark night, with a clear atmosphere, for a distance of two miles uniformly over an arc of ten points of the compass, from right ahead to two points abaft the beam. Lamps intended to compete for the prize should be sent in by the 31st December next.

The preparations made by the Governments of the present age to have every phase of a total eclipse studied and recorded, contrast favourably with the superstition that prevailed a few centuries ago. For instance, the *Scientific American* quotes the following from a German paper:—"The Elector of Darmstadt was informed of the approach of a total eclipse in 1699, and published the following edict in consequence:—'His Highness, having been informed that on Wednesday morning next at ten o'clock a very dangerous eclipse will take place, orders that on the day previous, and a few days afterwards, all cattle be kept housed, and to this end ample fodder be provided; the doors and windows of the stalls to be carefully secured, the drinking wells to be covered up, the cellars and garrets guarded so that the bad atmosphere may not obtain lodgment, and thus produce infection, because such eclipses frequently occasion whooping cough, epilepsy, paralysis, fever, and other diseases, against which every precaution should be observed.'"

A Natal correspondent writes that the diamond fields on the Vaal River cover so large an extent of ground that to effect a thorough search would occupy 20,000 men 100 years. From this assertion it might be supposed that the diamonds lie very deep; but the contrary seems to be the case, for we are told that they all lie comparatively near the surface, the diggers seldom going down deeper than seven feet. The copper in Namaqualand is likewise found near the surface, and stone implements are also found in a similar position. This is accounted for by the fact that the country is fast wearing down. These implements and other indications of former habitations appear to be abundant in Basutoland. Upon digging several feet below the surface near any of the occupied villages of the Basuto people, stone implements are found, and at a less depth the remains of fire places, broken pots (clay), and ash and cinder heaps are discovered. These remains are very abundant throughout the whole of Basutoland.

American naturalists are anticipating with pleasure the promised visit from Mr. Gwyn Jeffreys. He is expected in the course of the summer; and though his stay will be a short one, it is hoped that he will be enabled to secure personal conference with the leading American naturalists, and to make such an examination of the sea-coast fauna as he desires. He will probably arrive in time to meet Prof. Agassiz before he starts on the expedition, which contemplates the expenditure of at least a year in an exploration of the physics and natural history of the deep seas of both the Atlantic and Pacific, under the auspices of the United States Coast Survey.

Science forms an important element in the educational course at the Friends' School, Sidcot. From the report and Transactions of the Boys' Literary Society for the past year, we find that sixty-six monthly reports and thirteen original papers on subjects connected with their several departments, have been read by the curators. Careful and systematic observations by a large section of the members have been made in ornithology, and several rare species have been observed. Considerable attention has also been paid to the collection of plants and insects.

The Ludlow Natural History Society has little to report in the way of active proceedings during the past year, owing to the illness and subsequent death of the secretary, Colonel Colvin. Many details of work, especially in the completion of arrangements, were however attended to. The balance sheet is satisfactory, and the museum attracts a certain number of visitors; but the donations acknowledged suggest the idea that a collection of curiosities rather than a Natural History Museum is the object of the society. Mr. Alfred Salwey has been elected secretary.

The Quekett Microscopical Club has just issued its sixth Annual Report, from which it appears that the club continues to maintain its usefulness; not only has the number of members considerably increased during the year, and the selection of microscopical slides kept for the use of members and the number of volumes in the library been augmented, but the papers read at the fortnightly meetings show that important additions to microscopical knowledge have been made by members of the club. The fortnightly field excursions during the summer months have been well attended. The number of members now amounts to 550.

We have received an abstract of the reports of the surveys and of other geographical operations in India for 1869-70. It includes notices of Indian marine surveys, the great trigonometrical survey, and the topographical, geological, and archæological surveys during these years, with a chapter on geographical exploration.

The Royal Society of Victoria is just recommencing the publication of its Transactions, discontinued since 1868 in consequence of the withdrawal since that year of the customary annual grant of 100*l.* from the Colonial Government. Notwithstanding this official discouragement, the society was never in a more prosperous and active condition; the premises have been rebuilt, and considerable additions have been made to the library.

An event of rare occurrence has happened in the southern part of the great rainless desert of Atacama, a heavy fall of rain having taken place in Northern Chile on the 31st May from the coast to the Cordillera, and from Tres Puntas to Chonarcillo, including Copiapo. This was, perhaps, an extension of the rains in Southern Chile.

There were several earthquakes in Chili and Peru in June. On the 20th there was a strong shock at Tacua about 7 p.m., but no damage was done.

Dr. Henry Cassere, a German, has been sent by the Peruvian Government to make a collection of plants and animals in its Amazon territory, which are to form part of the Great International Exhibition at Lima.

The great subject of excitement in the South Pacific is the continued discoveries in the new Caracoles district of Bolivia. Silver is now being produced at the rate of 4,000 lbs. per day, or 400,000*l.* a year. Coal has been discovered, and new gems are found. The amethyst is the most abundant, and the opal of the finest quality. Marine fossils have been recognised in the formations.

The artesian well at Umballa had in July reached a depth of 527 feet.

A mine of silver lead of good quality has been found in the Marwar State in India.

The sea has made considerable encroachments at Aleppey in India. We lately recorded the high tide which swept over the Laccadive islands.

The Agri-Horticultural Society of India have reported that the nettle of the Neilgherries furnishes a valuable fibre, at least equal to Rhea grass, but attended with the same difficulties in working.

It may be of interest to collectors to known that there is now an ornithologist or bird stuffer at Constantinople, Mr. William Pearse, and a dealer at Smyrne, Mr. A. Lawson.

We must add to our maps the ports of the growing region of Bolivia on its narrow strip of coast. Besides Cobija there are now as trading ports Mejillones, Tocopilla, and Caleta de la Chimba.

Gold operations are being undertaken at Penang by English enterprise, with great hopes of success. The object is to work the quartz reefs.

Gold mining is reviving in Colombia or New Granada, a country once famous for its riches.

## *THE LATE CAPTAIN BASEVI, R.E.*

A LETTER in the *Times* of the 19th inst., from Col. J. D. Walker, R.E., announces the death of Captain James Palladio Basevi, of the Royal (late Bengal) Engineers, Deputy-Superintendent of the Great Trigonometrical Survey of India, an officer of great worth and ability, whose loss will be long felt in the department of the public service to which he belonged. He was the son of the celebrated architect, George Basevi, and was distinguished as a lad for more than ordinary talent, and particularly for his mathematical abilities. First at Rugby, then at Cheltenham College, and afterwards at Addiscombe, he won for himself a high position among his fellow students, and in December, 1851, he left Addiscombe as the first cadet of his term, obtaining the first prize in mathematics, the sword for good conduct, the Pollock medal, and a commission in the Honourable East India Company's Corps of Engineers.

The first few years of his services in India were spent in the Department of Public Works in the Bengal Presidency; but in 1856 he was appointed to the Great Trigonometrical Survey of India, in which he continued to serve up to the time of his death, performing many services of great value.

His bent of mind and habits of study led him, however, to feel a preference for the more purely scientific branches of the operations of the Trigonometrical Survey. Thus, in 1864, he was selected to undertake certain operations which had been proposed by the President and Council of the Royal Society for the determination of the force of gravity at the stations of the great meridional arc of triangles measured by Lambton and Everest, which extends from Cape Comorin to the Himalayan Mountains. The investigations were to be effected by measuring the number of vibrations which would be made in a given time by certain invariable pendulums when swung at the several stations.

Captain Basevi entered on the pendulum observations with his characteristic ardour and devotion. He carried his observations of pendulum and clock coincidences over at least twelve days at each station; for ten hours daily—from 6 A.M. to 4 P.M.—he never left his pendulums for more than a few minutes at a time, taking rounds of observations at intervals of an hour and a half apart; then at night he would devote a couple of hours to star observations for determining time.

His observations of the pendulums on the Indian arc showed that the local variations of gravity which are superposed on the great law of increase from the equator to the poles, though apparently irregular when examined singly, are subject to laws which are highly interesting and curious, and are well worthy of investigation. At the northern extremity of the arc the results indicate a deficiency of density as the stations approach the Himalayan Mountains, while at the southern extremity they indicate an increase of density as the stations approach the ocean; thus both groups of results point to a law of diminution of density under mountains and continents, and an increase under the bed of the ocean.

Thus far, however, observations had not been taken at any very great altitudes, the highest station in the Himalayas being under 7,000 feet; arrangements were therefore made to swing the pendulums on some of the elevated table lands in the interior of the Himalayas, which rise to altitudes of 14,000 feet to 17,000 feet. It was expected that this would be sufficient to complete the work in India, and then the pendulums would be taken back to England to be swung at the base stations of Greenwich and Kew, and *en route* at Aden and at Ismailha on the Suez Canal, places which are in the same latitudes as some of Captain Basevi's stations. Thus gravity at Aden would be directly compared with gravity at certain points of the coast and continental stations of the Indian Peninsula, and similarly the plains of Egypt would be compared with the Himalayan Mountains.

In the spring of the present year Captain Basevi proceeded to Kashmir on his way to the high table lands in the interior.

Early in June he reached Leh, the capital of Ladak. He then proceeded to the Khiangchu table land in Rukshu, about eighty miles to the south of Leh. There, at a spot called Moré, in lat. 33° 16′ and long. 77° 54′, and at an altitude of 15,500ft., he completed a satisfactory series of observations, which show a very gross deficiency of density. After applying the usual reductions to sea level, &c., it was found that the force of gravity at Moré did not exceed the normal amount for the parallel of latitude 6° to the south, as determined by the previous observations with the same pendulums.

Wishing to have one more independent determination at a high altitude, Captain Basevi proceeded to the Changchenmo Valley, which lies due east of Leh, across the newly-proposed trade route between the British province of Lahoul and the States of Eastern Turkestan. Near the eastern extremity of that valley, on the confines of the Chinese territories, he found a suitable position in lat. 34·10 by long. 79·25, at an altitude which is not exactly known, but must probably have exceeded 16,000ft. He hoped to complete his observations in ten days, and then commence the journey back to India. But he did not live to carry out his intentions; already the hand of death was upon him, and, all unconsciously to himself, the over-exertion to which he was subjected in a highly rarefied atmosphere and under great vicissitudes of climate was rapidly undermining a constitution which, though vigorous, had already been sorely tried.

With the devotion of a soldier on the battle-field, he has fallen a martyr to his love of science and his earnest efforts to complete the work he had to do, and in him we have lost a public servant of whom it may be truly said that it would not be easy to find his equal in habitual forgetfulness of self and devotion to duty.

## *SOCIETIES AND ACADEMIES*

### Paris

Academie des Sciences, Sept. 11.—M. Faye in the chair.—M. Dumas read an abstract of a pamphlet published by MM. Lomer and Ellershausen, advocating the establishment at Bellegarde, in the department of Ain, of hydraulic machines worked by the Rhone, and giving a force of 10,000 horse-power. The site is called "Le perte du Rhone" at Bellegarde, and this immense hydraulic pressure is to be obtained by boring a tunnel, through which only one-third of the water of the Rhone will go. The height of the fall will be sixty feet, and the result is to be obtained very easily, as the tunnel is only to have a length of 550 yards. The engineers hope to create at Bellegarde a city as important as Lowell in the United States. It is intended to induce Alsatian manufacturers to move from Mulhaus, and to settle in that locality.—M. Decaisne sent some observations relating to animals fed with bread infested with the *oidium aurantiacum*, and it is considered as demonstrated that, at least under special circumstances, such food must be considered as being really poisonous.—M. Berthelot sent a very long paper on the union of alcohol with bases, which was inserted *in extenso* in the *Comptes Rendus*.—M. Lecoq de Boisbaudron sent also a paper which was published by him some time ago, on the constitution of luminous spectra.—M. Favre sent a paper to elucidate certain points of a special theory worked out to explain how a certain weight of copper rotating between the poles of an electro-magnet is heated by the influence at a distance. The fact was discovered by Foucault.

### San Francisco

California Academy of Sciences, August 22.—Mr. Dall called the attention of the members to some shells of oysters that had been transplanted from the Eastern States, and which during the last twelve months had been growing in the waters of the bay. The recent growth of these oysters had been modified in a manner so that they corresponded very closely to that of our native oyster. In the eastern oyster the shell is white and smooth, whilst our bay oyster has the shell much corrugated, of a brown colour, and frequently with purple stripes between the ridges. Now the recent growths of the shell of these transplanted eastern oysters exhibit the same corrugations as our native, the colour is decidedly more brown than in the east, and purplish stripes are frequently found between the corrugations.—Dr. Blake gave a description of some prismatic dolerite found in the neighbourhood of Black Rock, Nevada. The prisms were six-sided, measuring from 0·1 in. to 0·3 in. across, and some were from 3 in. to 4 in. long, but they all had evidently been broken. The separation of the crystals was caused by weathering, as in some specimens they were still aggregated. A thin section under the microscope showed that the rock was composed of augite, nephaline, and titanite, imbedded in a green vitreous matrix. Dr. Blake also read a paper on the diatoms found in the Puebla hot spring, Humboldt county, Nevada. The temperature of the water where they were collected was 163° F. They were contained in the decomposing layers of an abundant growth of red algæ, which formed a membranous covering at the bottom of the channel, through which the waters of the spring were discharged. This growth consisted of oscillariæ and a minute hair-like alga, which presented nothing but a mere outline even when magnified 700 diameters. This alga seems identical with the *Hygrocrosis Bischofi* found by Cohn in carnallite. By the interlacement of its fibres it formed a tough membranous layer covering the bottom of the channel, but this layer was coloured red, apparently by the oscillariæ. In the upper layer of these algæ but few diatoms were found, but those layers which had been covered in by new growths, and which were in a semi-gelatinous state, afforded a nidus in which the diatoms seemed to flourish with the greatest luxuriance both as regards species and individuals. In one slide, without any previous preparation of the deposit, as many as forty-six species were observed. But the most interesting point in connection with them is their almost perfect identity with the diatoms found in the infusorial strata in Utah, and which have been so fully described by Ehrenberg in his recent memoir on the Bacillariæ of California. Amongst the more marked species which were peculiar to the Utah strata, Cocconema unciale. Hyalodiscus Whitneyi, Stephanolithis hispida, and Cosmiolithis Henryi were readily recognised; in fact, had it not been for the presence of a small quantity of these hair-like algæ in the recent specimen, it might have been regarded as having been taken from the Utah beds. The resemblance of form between these hot spring and Utah diatoms, and the fact of their growing so luxuriantly in water so hot as to render it unfit to support any other form of living being, makes it more than probable that the Utah infusorial layers were formed in an inland fresh-water sea, the temperature of which was probably about the same as that of the Puebla hot spring. The great difficulties in explaining the formation of these extensive infusorial deposits have been the time required for their formation, and also the entire absence of all other fossil remains in strata that were evidently quietly deposited in fresh water. Both these difficulties are removed by admitting that the inland sea in which they were formed was of a temperature which is seen to be most conducive to their rapid growth, and which, at the same time, was incompatible with the existence of other forms of living beings. It is probable that the temperature of the air was not much below that of the inland sea, so that no land plants or animals could exist at the time when the Utah beds were being deposited. The admission of the existence of such an extreme climate even in the temperate zone at so recent a period as the post-pliocene (the position these beds are supposed to occupy) would certainly lead to important modifications in our views as regards the condition of the surface of the earth at that period. The author thinks it probable that these Utah infusorial beds are miocene, as at the close of that period we know that the temperature of the Arctic region was some fifty or sixty degrees warmer than at present. He proposes in a future communication to enter more fully into this question, and also to consider the bearing of the discovery of the production of these low forms of living beings in such apparently abnormal conditions on the origin of living matter. — Prof. Whitney gave an account of the investigations carried on during the progress of the Geological Survey of California, having for their object the determination of the value of the barometer as a hypsometrical instrument, the expectation being, that after a sufficient stock of observations shall have been accumulated and reduced, it will be possible to designate the hours of the day for each month when the result will approach nearest to the truth; and in general to give practical rules in regard to the times of observing and the methods of reduction, the following of which will secure a close approximation to accuracy than can now be attained. An elaborate series of observations with this end in view was begun on this coast some ten years after by Colonel R. S. Williamson, of the U. S. Engineers; but the work was suspended by the Engineer Bureau just before being completed. Colonel Williamson's results, however, were published in the form of a superb quarto volume, as an "Engineer Paper," and this contains a large amount of valuable material, so that the work of the Geological Survey is only to be looked upon as supplementary to that so ably commenced by him. The stations at which observations are being carried on at present, under the direction of the Geological Survey, are along the line of the Central Pacific Railroad, and their elevations are presumed to be accurately known from the levellings of the railway surveyors. The points selected are San Francisco, Sacramento, Colfax, and Summit, approximately 0, 30, 2,400, and 7,000 feet above the sea-level. The observations have already been continued at these points nearly a year, and are made at the Smithsonian hours (7 A.M., 2 P.M., and 9 P.M.). The greatest care has been taken that the instruments should be kept in perfect order, well placed for accurate results, and carefully and punctually observed. The observations of the first ten months have already been partially worked over by Prof. Pittee, of the Geological Survey, and the results attained indicate very clearly that valuable assistance will be derived from the completed series in the reduction of the copious barometric determinations of altitude made during the progress of the survey.

## BOOKS RECEIVED

ENGLISH.—Hardy Flowers: W. Robinson (Warne and Co.).

AMERICAN.—Mammals and Winter Birds of East Florida: J. A. Allen.

FOREIGN.—Verhandlungen des naturhistorischen Vereines der preussischen Rheinlande; Parts 1 and 2.—Sitzungsberichte der Niederrheinischen Gesellschaft zu Bonn, 1871.—Schriften der Naturforschenden Gesellschaft in Danzig.

## PAMPHLETS RECEIVED

ENGLISH.—On the Spirit Circle: Emma Hardinge.—Transactions of the Literary Society of Sidcot School for 1870-71.—The Climate of Brighton: S. Barker.—The Dependence of Life on Decomposition: H. Freke.—A Complete Course of Problems in Plane Geometry: J. W. Pallisser.—Sixth Report of the Quekett Microscopical Club.—On the Relative Powers of Various Substances in Preventing the Generation of Animalculæ: J. Dougall, M.D.—Testimonials in favour of J. W. Davidson, candidate for the Chair of Anatomy in the Edinburgh Veterinary College.—The Traveller; Vol. I., No. 5.—Water not Convex, the Earth not a Globe: W. Carpenter.—On the Economical Production of Peat and Charcoal.—The Contagious Diseases Act and the Royal Commission.—Some Simple Sanitary Precautions against Cholera and Diarrhœa: M. A. B.—The proposed India and England Railway: W. Low and G. Thomas.—Contributions to the Knowledge of the Meteorology of Cape Horn and the West Coast of South America.—Transactions of the Geological Society of Glasgow; No. 3, Supplement.

AMERICAN AND COLONIAL.—Fourth Annual Report of the Trustees of the Peabody Museum.—Transactions of the Entomological Society of New South Wales; Vol. II., Part 2.—Notes on the Birds of New Zealand: T. H. Potts.—Arrangement of the Families of Molluscs: T. Gill, M.D.—On the Early Stages of *Terebratulina septentrionalis*: E. S. Morse.—What are they doing at Vassar? Rev. H. H. Macfarland.

FOREIGN.—Le Chiffre Unique des Nombres.—Sulle Distribuzione delle protuberanze intorno al disco solaro: P. A. Secchi.

## CONTENTS



ERRATUM.—Page 383, second column, lines 4, 11, for "Geneva" read "Genoa."

### *NOTICE*

*We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception.*

THURSDAY, SEPTEMBER 28, 1871

## EXPERIMENTAL SCIENCE IN SCHOOLS

*The Elements of Physical Science.* By Gustavus Hinrichs, A.M., Professor of Physical Science in the State University of Iowa, &c. In 3 vols. Vol. 1. Physics. (Griggs, Watson, and Day, Davenport, Iowa, U.S.)

*The School Laboratory of Physical Science.* Edited by Gustavus Hinrichs. Nos. 1 and 2.

"BY resolution of the Board of Regents in 1870, the Iowa State University has finally cut loose from the old college course. Only by this resolution, *placing the elements of Physical Science at the very beginning of the course*, can instruction in science become thorough. For the first time the students in physical science have been offered facilities not too inferior to those they have for ten years enjoyed in other branches of learning." And with what result? "A marvel of studious industry there" (in the laboratory). "Young men and young women, boys and girls, measuring, weighing, testing, demonstrating, and recording fact upon fact in physics, that, at least in our school days, were pored over in a maze of bewilderment, in dryest of text-books, to be bolted in sections without question." We trust that these important reforms in science teaching will prove contagious, and spread rapidly from the plateau of Iowa City to a region of even greater extent than the American continent. Let us examine how Prof. Hinrichs is doing his part to attain this desirable result.

Bearing in mind the important fact that science teaching in schools must be of a practical nature from beginning to end, the American Professor has sketched out in his "School Laboratory" a plan which in the main will recommend itself to every competent teacher both in his own country and in ours. He proposes that the course shall be divided into three parts :—Rudiments, Elements, and General Principles. The Rudiments, which ought to be studied in the first year or so of a boy's school life, embrace only prominent general facts and determinations, easily observed and measured with a sufficient (but limited) degree of accuracy ; together with the collective study of these facts, so as to bring to light several of the so-called laws. The Elements comprise the same subjects, treated however, more fully, and they should be completed "in the first year of the high school course." The General Principles embrace mathematical deductions of a concise and simple nature, together with some of the most important hypotheses of Physical Science ; this portion should be completed in the last year of the high school course. Prof. Hinrichs is careful to point out that technical instruction in schools will not result in the advancement of science ; but that a thorough general training in the phenomena of Nature, together with that already given in languages and mathematics, will lead to hitherto unimagined progress.

Such is Prof. Hinrichs' idea of a sound scientific training, and a very admirable one it is. To carry it out we must strive after good teachers, capacious laboratories, and trustworthy text-books. For our own part, we think that good teachers would not be found so scarce as is imagined, if there were only a genuine demand for them ; from a variety of causes, however, such as parental ignorance, false economy on the part of schools, and the ridiculous demands of public examiners, science has been kept, up to the present time, at the lowest possible ebb, except in the wealthiest of our public schools. It is deplorable to think how few school laboratories there are in England which could in any way vie with that in the Iowa State University, where "more than two hundred students have experimented within six months ;" and we fear that this state of things will continue for the most part unaltered until the public examiners require a *practical* knowledge of the sciences taught in schools.

We are perhaps as deficient in good text-books as we are in laboratories ; and the reason for this is not far to seek. If a candidate is asked to explain a phenomenon or a class of phenomena, but is never required to exhibit it to the examiner, it is natural that he should content himself with learning the explanation without performing the experiment. Hence we find that the great majority of our text-books are merely explanatory, and not at all experimental ; the phenomena are fully described and most ably explained, but the work which should be done in the laboratory to bring about these phenomena is forgotten by the teacher and the taught, because—*it is not required at public examinations.* It was therefore a bold undertaking for Prof. Hinrichs to bring out his "Elements of Physics," which is an excellent and almost unique specimen of a practical treatise ; and we trust that it will meet with a reception worthy of it.

In the first volume of this work, the student is taken, in about 150 pages, through a course of simple and easy experiments relating to Magnitude, Weight, Machines, Properties of Matter, Light, Electricity, and Magnetism. Each operation is so clearly described that the book might almost be employed by a solitary student, and many of the experiments, we are convinced, not only could but ought to be performed by children at the very commencement of their school career.

There is great difference of opinion as to whether qualitative and quantitative observations of natural phenomena should be performed simultaneously or consecutively—we are disposed to hold the latter view rather tenaciously, believing that science should be one of the first subjects taught in all schools. However, no one need be dismayed by the simple measurements of length, area, weight, and so on, which form the main portion of Professor Hinrichs' first chapter. The metrical system is taught by him in the only practicable manner, by means of actual measurements performed by the pupils themselves, without any reference, beyond a passing contemptuous notice, to the English system. The student is also familiarised with various forms of surfaces and solids, learns the management and the use of very simple apparatus, such as could well be provided in any village school, constructs his own measures of weight and length, makes numerous determinations, and enters his results in a journal. The exercises in mensuration and co-ordinates are especially useful, both from a scientific and a mathematical point of view ; and the Journal of Experiments—blank pages at the end of the volume to be filled up by the pupil—is perhaps the most suggestive portion of this original work. In short, the experimental method is adopted in every chapter; and it is thus that the inquirer after truth is taught, step by step, to appeal to the fountain source for most, if not all, information concerning "the wonder and mystery of Nature."

There is, however, a very marked disproportion in the amount of space allotted to each subject. Machines occupy only sixteen pages—probably the feeblest chapter in the book; while Crystallography extends over as many as thirteen pages. We think also that too much attention (relatively, at least) has been paid to Electricity and Magnetism. Pure and simple observation, even of natural phenomena, cannot properly be said to educate the mind, unless the reasoning faculties are called into play; and such subjects as Electricity, Botany, and Crystallography, if made an essential portion of school training, would doubtless tend to bring the whole question of science-teaching into disrepute. The only experiments that should be performed in the laboratory are such as will bring to light a scientific fact; and it should be remembered that a fact is scientific only in so far as it is interconnected with other facts. The more intimate this interconnection is, the better suited is the fact for elementary education; because it gives rise to a greater amount of rational explanation, and tends, by reaction, to imprint upon the mind knowledge already acquired. Professor Hinrichs does not appear to us to attach sufficient importance to these views; his work has therefore a disjointed aspect, and is wanting in large general ideas which should be cautiously introduced at proper intervals for the purpose of increasing the scope of the pupil's understanding. We agree with him that the quantitative study of such subjects as the Law of Gravitation should be postponed to the last year of the school course; but its qualitative study might be carried on with great advantage at a much earlier period; for previous familiarity with such theoretical views as are capable of some sort of experimental proof will make a student anxious to examine the subject quantitatively at the earliest opportunity. For these reasons we regret to find certain points omitted in the present volume, such as the Laws of Motion, which are so admirably adapted, not only for experimental verification, but as a means of explaining the principles of scientific induction. Still, if Prof. Hinrichs has not discovered every gem, he has nevertheless succeeded in pointing out the right path of discovery, along which he has acted on the whole as a faithful and thoroughly painstaking guide.

The idea of the "School Laboratory" is also a very admirable one. It is, in fact, a monthly magazine, the aim of which is to inculcate the system of experimental work upon which Prof. Hinrichs so strongly insists; to give examples of methods and results; and to aid both teacher and pupil.

We trust that the efforts of this able reformer of science-teaching will be amply seconded; and we believe that these Elements will be found of great service to every conscientious teacher, who will be able to glean from them many valuable suggestions both as to method and treatment; and we recommend them especially, because a widely-spread knowledge of a work of this kind will tend very much towards the introduction of experimental science into the curriculum of our schools.

## OUR BOOK SHELF

*Phrenology, and how to use it in Analysing Character.* By Nicholas Morgan. (London: Longmans, Green, and Co. 1871.)

THE appearance of a book of this kind from time to time shows what a deep hold phrenology took upon the popular mind. Had it not been so, we should have neither writers nor readers of works upon "The Science of Phrenology," now that almost the whole foundations of the system have been shown to be either untrue or based upon misconceptions. The present work is illustrated by numerous portraits and other engravings, and several of the former are remarkably truthful representations of living or recently-living celebrities; though we doubt whether the accompanying analyses of character will prove as agreeable to the originals as they are destined to be edifying to the public.

*The Dependence of Life on Decomposition.* By Henry Freke, M.D., T.C.D., &c., Professor of the Practice of Physic and Lecturer on Chemical Medicine in Steven's Hospital Medical College. (London: Trübner and Co.)

THIS is a pamphlet of a controversial character, which would not prove interesting to the general reader. Dr. Freke's views were originally published in 1848 in a work "On Organisation." They are peculiar in many respects, but contain the germs of some important biological truths. The following passage (p. 28) may serve as an example:—"Why, with an adequate supply of food, are we not able to work our brains, muscles, &c., for an *indefinite* period, like a steam-engine with an adequate supply of steam? Because the tissues are disintegrated, and require nutritive repair. If the animal tissues did *not* undergo disintegration during the active discharge of their functions, why should not the animal, like the vegetable, continue to increase in dimensions during the entire period of its organic existence? It is because the organic tissues developed by the vegetable do *not* undergo disintegration when their construction has been completed, that the vegetable continues to grow and increase in dimensions during its entire life. Such is not the case with the animal, and that for this reason, namely, when the construction of the animal tissues, brain, muscle, &c., is completed, those tissues undergo disorganisation while discharging their functions."

*The Estuary of the Forth and adjoining Districts viewed Geologically.* By David Milne Home, of Wedderburn. (Edinburgh: Edmonston and Douglas.)

MR. MILNE HOME'S name has long been known in connection with Scottish geology. His memoir on the Coalfields of the Lothians was for many years the only trustworthy geological account of those areas. In addition to this he has from time to time communicated to various scientific journals a number of papers chiefly on subjects relating to glacial geology. In this present volume he returns to these subjects, and gives us a description of the superficial formations of the basin of the Forth, together with what he considers to be the most feasible explanation of the somewhat intricate details he brings before his readers. He treats first of the form and physical features of the Estuary and the districts adjoining; secondly, of the formation or origin of the Estuary; and, thirdly, of the superficial deposits met with in the area described. He conceives that the faults which intersect the strata along both sides of the Firth, and which not only have the same general bearing as the Estuary, but are also for the most part downthrows to south, in Fifeshire, Clackmannan, &c., and, in the Lothians, downthrows to north, have formed the deep trough or valley of the Forth—the depression caused by this series of step-faults having reached at least 2,000 feet. "Along the lines of these slips great precipices, or cliffs, were formed, several hundred feet in height, which, under the action of the sea or the atmosphere, crumbled down." The materials thus supplied went to form the superficial deposits, it being supposed that almost the whole of Scotland was under the sea at the time these changes took place. We feel sure that Mr. Milne Home will get few geologists to agree with him in these conclusions. In the first place, it may very well be doubted whether the faults which cut the strata ever actually showed at the surface in the manner supposed. It is much more probable that the dislocations took place so gradually that any inequalities arising therefrom were planed away by denudation as fast as they appeared. But even were this not the case, it is quite certain that the faults referred to by Mr. Milne Home must date back to a vastly more remote antiquity than the later Tertiary periods. The Scottish Coal-fields, indeed, would appear to be traversed by some faults which, according to the Geological Survey's map and description of the South Ayrshire Coal-fields, do not influence the overlying Permian. It is also indisputable that the igneous dykes, which Professor Geikie has shown to be of Miocene age, are all posterior in date to the faults which shift the Coal-measures. Mr. Milne Home does not take into consideration the prodigious amount of denudation that the palæozoic strata of the valley of the Forth must have undergone in the long ages that intervened between the close of the Carboniferous period and the advent of the glacial epoch. There cannot be any reasonable doubt that the valley of the Estuary of the Forth existed as a valley long before the dawn of the age of ice. But Mr. Milne Home's memoir is taken up chiefly with the history of the drift deposits, which he describes in considerable detail. Especially valuable are the numerous sections given, and the long lists of localities where glacial-striæ, erratic blocks, kaims, and the other phenomena of the drift, may be studied. The author inclines to the iceberg theory of the formation of the boulder-clay, and thinks it may have originated at a time when "the ocean over and around Scotland was full of icebergs and shore-ice, which spread fragments of rocks over the sea-bottom, and often stranded on the sea-bottom, ploughing through beds of mud, sand, and gravel, and blocks of stone, and mixing them together in such a way as to form the boulder-clay." Mr. Milne Home points to the presence of beds of sand included in the boulder-clay as one of several objections to the land-ice origin of that peculiar deposit. He thinks that if the iceberg theory be adopted, the explanation would be simply this, "that icebergs came at different periods, new sea-bottoms being formed in the intervals." But, on the other hand, if the glacier theory be accepted, then it would have to be admitted that the land must have sunk under the sea for every bed of sand we find in the boulder-clay. The author, however, does not seem to be aware that fresh-water beds are found interstratified with the boulder-clay, so that the difficulty in either case is equal. We have not space to notice several other interesting points treated of in this memoir, which contains so many important data, that we can recommend it confidently to our geological readers. We may dispute some of the author's conclusions, but it matters not what interpretation may eventually be put upon the facts, many of the facts are here, and Mr. Milne Home has done good service in bringing them together.

J. G.

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Phenomena of Contact

IN NATURE for August 24, Mr. Stone controverts two propositions incidentally put forward in a review of Mr. Proctor's book, "The Sun." They are :—

"1. That the irregular phenomena of internal contact of a planet with the sun, variously described as 'distortions,' 'black drops,' 'ligaments,' &c., are not always present, but are only seen 'sometimes.'

"2. That when seen they are due to insufficient optical power or bad definition."

In writing that review, I tried to avoid the assertion of any proposition I could not fully sustain, and therefore very willingly give the evidence on which these propositions rest. At the outset, however, I beg leave to call especial attention to the fact that I did not assert the second in an absolute manner, but only said that it was "indicated" by observations and experiments.

The first proposition is sustained by the fact that at the last transit of Mercury, the majority of those observers who have described the phenomena saw neither ligament nor distortion, but only the geometrical phenomena of contact, the planet preserving its rotundity to the last.

The following is a statistical summary of the evidence on both sides :—Among the numerous English observations published in the monthly notices, fourteen describe the phenomena. Of these three saw the phenomena go on regularly, while eleven saw ligament, black drop, or distortion either before or after the contact. Among these eleven there is little agreement as to the exact nature of the distortion. Owing to the low altitude of the sun in England, I take it that the atmosphere was much less favourable than on the Continent.

At Marseilles Le Verrier saw the black drop. He used a seven-inch glass, of which both the centre and circumference were covered by a screen, which is sufficient to account for the phenomenon by the diffraction thus produced. Mr. Stephen, who observed at the same place with a very large reflector, "déclare n'avoir rien vu de pareil." *

Of the five observers at the Paris Observatory, Le Verrier says† :—"Les observateurs ont remarqué qu'il ne s'est rien présenté de particulier, ni au moment du contact intérieur, ni après ce contact. Mercure a touché le bord du Soleil en amincessant progressivement le filet de lumière, mais sans produire le phénomène de la goutte." Le Verrier was, therefore, so far as we know, the only observer in France who saw the black drop.

At Madrid Ventosa may have seen several black drops "tout-à-coup." His description, however, is rather obscure.‡

At Lund the egress was observed by Duner under very favourable circumstances with a nine-inch glass. He says § :—"Die Bilder waren sehr ruhig, und die innere Berührung geschah in der Weise, dass der Lichtfaden Zwischen den Rändern des Mercurs und der Sonne erst dann brach als seine Breite verschwindend klein geworden war. Es zeigte keine Spur einer Verdrehung der Bilder oder des von anderen Beobachtern erwähnten schwarzen Tropfens."

At Pulkowa fourteen observers observed the egress. I learn that not one saw anything but the geometrical phenomena of contact.

To avoid a tedious collation of accounts which nearly all say the same thing, I remark that only two observers on the Continent saw any abnormal phenomena, namely, Kaiser at Leiden, and Oppolzer at Vienna. The first saw an elongation of the planet, which he thought might be due to maladjustment of his instrument.|| The second saw the sun's limb pushed out by that of Mercury, so that apparent contact took place before the breaking of the thread of light.**

Summing up all the accounts, I find the result to be :—

| | |
|---|---|
| Total number of observers who describe phenomena . | 39 |
| Number who saw the planet remain perfectly round, and the phenomena of contact occur with entire regularity, and without distortion, ligament, or drop | 24 |
| Number who saw ligament, distortion, one or more drops, or other abnormal phenomena . . . . . | 15 |

The twenty or thirty observers who do not describe the phenomena probably saw nothing abnormal, but they are not counted in the above list.

The first proposition is, I conceive, fully established by the statistical facts cited.

Passing now to the second, it may be remarked that when different observers give different descriptions of the same

* Comptes Rendus, 1868, ii., p. 921-924.
† Ibid. p. 948.
‡ Astronomische Nachrichten, vol. lxxii., p. 356.
§ Ibid, p. 378.
|| Ibid, vol. lxxiii., p. 214.
** Ibid, vol. lxxii., p. 347.

phenomena, there must be some corresponding difference in the circumstances of observation, and that when among five observers three see the phenomena exactly as we know they are, while two see them as we know they are not, and even then do not agree between themselves, there is a strong presumption that the latter do not see them rightly. I am aware of but a single attempt to determine experimentally the causes why one-third of the observers of the late transit, and many observers of former transits, saw the planet distorted, namely, that of Wolf and André, to which Mr. Stone alludes. They found, in observations of artificial transits under various circumstances, that when they used a telescope of at least twenty centimetres aperture, with a good object glass, well adjusted to focus, they saw only the geometrical phenomena of contact, while, if the object glass was small, or not well corrected for aberration, or not well adjusted to focus, they saw the phenomena of distortion.*

In the absence of farther investigation, which is much to be desired, these results seem to me, at least, to "indicate" that the phenomena in question are due to insufficient optical power, or bad definition either in the object glass or the atmosphere. At the same time I by no means insist on this proposition as established, and it is a great defect in the experiments in question that they do not extend to the effects of using shades of different degrees of darkness in observing the sun; but of this anon.

In his letter Mr. Stone quotes the observations of Chappe, Wales, Dymond, &c., in 1769; but I cannot admit that they bear strongly on either of the points in question, till we have some better evidence than now exists that their object glasses were such as Clarke or Foucault would call good.

Again, the argument from irradiation, if it proves anything, proves too much. I do not see why, upon the theory of Mr. Stone, the distortion should not always be seen. To be satisfactory, any theory of the matter must explain why it is that A, B, C, &c., see the phenomena, while X, Y, Z, &c., do not, and that of Wolf and André is the only one which does this.

Mr. Stone objects to the experiments of the Paris astronomers, that their disc was not sufficiently illuminated to exhibit any optical enlargement. I do not know his authority for this assumption, but, whether well founded or not, it seems to me that if the sun were viewed through a dark glass, it would present the same optical phenomena of irradiation with a disc so illuminated as to appear of the same brilliancy with the darkened sun. Thus, in the absence of evidence to the contrary, the Paris experiments may be taken as showing how the phenomena would present themselves in the case of the sun viewed through a shade of a certain (unknown) degree of darkness.

Before we can make any application of the theory of irradiation to the phenomena of contact, we require to know whether the irradiation of an extremely minute thread of light, darkened so as to be barely visible, is the same with that of a large disc. I am decidedly of opinion that it is not, and, if not, the fact that the sun's disc is optically enlarged by the telescope or the eye of the observer, cannot be directly applied to the phenomena of a transit.

To sum up my views;—neither Mr. Stowe nor any one else will claim that the ligament he saw before the time of internal contact was a celestial reality—he considers it a result of irradiation, but whether of telescopic or purely ocular irradiation I do not understand. If the former, this is simply a species of bad definition, and there is little difference between us. I also admit, on my part, that if the telescope and the eye are such that from any cause whatever an exceedingly thin thread of light presents itself to the sense as a band several times thicker than it really is, then, as the real thread becomes invisible, the seeming band will appear to be broken through by what some may consider a ligament and others a black drop, and the really sharp cusps will seem to be rounded off at their points. If I rightly understand Mr. Stone, he holds that these results of the thickening of the thread of light by what he considers irradiation are unavoidable. But this view is conclusively negatived by the fact that they actually were avoided by a large majority of the observers of the late transit. Admitting, then, that these spurious phenomena are not unavoidable, it matters little whether we call their cause irradiation or bad definition, though it is important that we should know its exact nature. The only attempt I know of to determine this is that of Wolf and André, and their results seem to me so nearly in accordance with what we should expect, as to be quite worthy of acceptance, at least in the total absence of rebutting evidence.

SIMON NEWCOMB

* Comptes Rendus, 1868, i. p. 921.

## Solar Parallax

I HAVE waited somewhat anxiously for Prof. Newcomb's statement of the errors in a chapter on the Sun's distance ("The Sun," Chapter I.). His review was certainly so worded as to imply very gross inaccuracy, and his explanatory letter, in which he remarked that more than a column of NATURE would be needed for the mere record of my errors, did not improve matters. This morning I have received his notes. The errors enumerated amount but to seven in all; I will leave your readers to judge of their importance.

1. At p. 50, I assign to Hansen's letter of 1854 the announcement of the value 8″·9159 for the solar parallax; whereas this value was not announced by Hansen until 1863. *Tanquam referat.* Hansen's priority remains unaffected by the change.

2. At p. 53, I mention that Prof. Newcomb deduced a value of 8″·84 (probably a misprint for 8″·81) for the solar parallax by a certain method. His real result was 8″·809. Again my comment is *tanquam referat.*

3. At p. 53, Foucault's "parallax is given as 8″·942, whereas the result actually deduced was 8″·86." The matter again is utterly insignificant; but it chances that I have not given Foucault's estimate of the parallax as 8″·942. I remark only that if Foucault's estimate of the velocity of light is correct, the parallax would be 8″·942. I deduced this result by a calculation made on my thumb-nail as I wrote. It is correct, however, and Foucault's was not.

4. At p. 59, I say that Mr. Stone deduced the solar parallax from observations of Mars made at Greenwich alone, and then by combining these observations with others deduced the solar parallax at 8″·943. Now, Prof. Newcomb says that he "finds no discussion of the observations at Greenwich alone, *in the paper here referred to.*" But I refer to no paper whatever. A rough calculation of the parallax was certainly made from the Greenwich observations alone, though, as Mr. Dunkin remarks at p. 507 of his edition of "Lardner's Astronomy," "the observations by this method (single-station observations) were comparatively unsuccessful," "owing to unfavourable weather at Greenwich." Apart from the facts, which fully justify my statement—what could the correction be worth in any case? Only the final result was insisted upon.

In a note on this matter, Prof. Newcomb makes "in passing" the really important observation that the method of determining the sun's distance by observations on Mars from a single station was applied by the Bonds as far back as 1849. Mr. Carrington had already told me that he believed the Bonds had anticipated the Astronomer Royal. I wrote to Prof. Young asking for further information, and was waiting for his reply. I am obliged to Prof. Newcomb for aiding me in this matter. The priority of the Bonds in this matter should certainly be more widely known than it is.

5. At p. 61. This is a very curious correction. I speak of Prof. Newcomb as having successfully treated the problem which was *afterwards* discussed by Mr. Stone; and he remarks that he knows nothing of the matter, and has read my statement with great bewilderment. I am not responsible for it. There is a letter in the *Astronomical Register* for December 1868, signed only "P. S.," but with unmistakable internal evidence of coming from the Astronomer Royal for Scotland, in which the following passage occurs:—"I must not say a word about the pyramid sun-distance here, or my letter will never be allowed to see the light; something, however, on the score of modern justice to our contemporaries I must beg leave to put in. Admirable is the praise given to Mr. Stone, and worthy, in so far, the credit abundantly bestowed on him at every step of the undertaking; but why is there not one word about Prof. Simon Newcomb, of America, having already gone over that same problem similarly, and published the results a year sooner?" Of course, as Prof. Newcomb now writes that "he has no recollection of ever having made any independent investigation of the observations of the transit of Venus," Prof. Piazzi Smyth was mistaken, and "the abundant discussions of Prof. Newcomb's paper in various northern scientific societies last winter" (so speaks Smyth) were founded on some misconception of its purport. But Prof. Smyth's statements were permitted to remain uncorrected;—*hinc illæ lacrymæ.*

6. At pp. 61, 62. "The distortion of Venus at the time of internal contact is described as an ever-present phenomenon, and the apparent formation of the ligament as contemporaneous with true internal contact." If what I say in pp. 61, 62 admits of being so misinterpreted (which I question), the same cannot be said of

my remarks in p. 63. My belief is now, as it has been for years (long before Mr. Stone's paper was published), that under favourable conditions an exceedingly fine ligament must be visible at the moment of real internal contact, the planet's outline being otherwise undistorted. But in most instances a coarser ligament is formed *not* contemporaneously with the moment of real contact. I have shown that the true moment of contact can be *inferred* from the formation of a coarser ligament as exactly as when a fine ligament is observed. This I still maintain, and I further believe that Mr Stone's opinion as to the cause of the phenomenon, an opinion independently enunciated by myself in November 1868 (see *Scientific Opinion*) is correct, and that the experimen al tests which have been supposed to disprove it, have in reality no sufficient bearing on the question at issue.

7 At p 63. It is unfortunate if my account of Stone's proceedings suggests that I maintain he was the first to consider whether real or apparent contacts had been observed; for I have but lately been maintaining the contrary view in a correspondence with an ex-president of the Royal Astronomical Society. I have invariably opposed the opinion here ascribed to me. Mr. Stone himself has never claimed what I am said to have claimed for him, He has made a definite claim, and that claim I have repeated and still hold to be just.

Prof. Newcomb concludes with some general statements. He considers I am mistaken in supposing that astronomers generally regard observations on Venus in transit as the most trustworthy method of obtaining the solar parallax; mistaken again in supposing that Mr. Stone has removed any "difficulties that had perplexed astronomers;" and so on. Such statements are so vague that I shall scarcely be expected to discuss them. Until proof, or at least some evidence to the contrary, is supplied, I can only say that now, as when I wrote "The Sun," my opinions on these points seem to me to be just. I am certainly not alone in holding them. RICHARD A. PROCTOR

Brighton, September 23

### Elementary Geometry

THE question raised on this subject naturally consists of two parts. The first relates to the unsuitableness of Euclid as a text-book, and the need of a work which shall so commend itself to examiners and teachers, so to supplant it. The second question is—given the authoritative text-book, how is the geometry of which it treats to be taught to young students? The arguments on the first of these questions have been so ably and conclusively stated lately by several mathematicians, especially by Mr. Wilson, Dr. Joshua Jones, and Dr. Hirst, that there is no need to revive the discussion; but I entirely agree with your correspondent in his conclusion that the book which is to supplant Euclid is at present a desideratum, and that it will probably be the work of more heads than one. Several books have been written during the last four years, and have formed the basis of the discussions which have since taken place on the requirements of the new programme. By their means, the questions at issue between the opponents and supporters of Euclid have become more clearly defined, and a greater unanimity of action has resulted amongst those who are labouring to supply this desideratum of modern education. But I am sure that most of these authors will admit that the issue of works intended for permanent text-books was premature.

When the first question is settled, the second remains. Geometry is not essentially difficult, nor is it generally distasteful to young students. It becomes so, however, when they are required to commit the propositions to memory before they understand them. The educational purpose which geometry serves is not the discipline and exercise of the memory. A choice and pregnant passage from a good author may be learnt and retained in the memory without much difficulty, although its meaning may be very imperfectly understood, and it will richly repay the labour of its acquisition. It will be recalled again and again, and receive new light, and afford new pleasure with every fresh association. Not so with geometry; it is useless if not understood. A child should be made to comprehend even the definitions before he commits them to memory Let us suppose, for instance, that the definition of a circle is to be learnt, the preliminary explanation should take some such form as the following.

The teacher at the black-board, with chalk and compasses, and the pupils at their desks, with paper and compasses—the teacher draws a circle and names the figure—he tells each boy also to make a circle, and then proceeds to question. What name is given to such a surface as that on your drawing paper? What kind of a figure shall we call one which can be drawn on a plane surface? Compare a triangle and a circle, and say how many lines form the boundary of the triangle? How many lines contain the circle? Explain exactly what you do with the points of the compasses when you use them to make a circle? Why must the joint of the compasses be tight? Fix a drawing-pin in your drawing-board, and with a piece of thread construct a circle. What purpose does the thread serve in the construction? The defining properties of a circle are, therefore, these—(1) it is a plane figure; (2) it is bounded by one line, termed the circumference (3); every point of the circumference is at the same distance from a fixed point, termed the centre.

After the definition is worded in its permanent form, and repeated, and written several times on paper, it will be remembered.

Again, let us suppose the propositions on the equality of triangles to be the subject; the following introductory questions and exercises suggest themselves. Draw two straight lines, one 5 in. long, and the other 8 in.; then make with your protractor an angle of 43°. Construct a triangle having one of its angles equal to the angle drawn, and the sides of this angle equal to the given straight lines. Take the figures drawn by different boys, and compare them as regards size. Now consider the parts of each; how many sides has each? How many angles? How many sides are drawn from given dimensions? Letter them and then name them. How many angles? Name it. How many angles were not originally given? Name them. How many sides? Name it. Compare this third side, B C, in two of the figures. If the figures are all accurately drawn throughout the class, what must necessarily follow with regard to this third side B C in all the figures, &c.?

Finally, the proposition should be enunciated, and the proof learnt in the form in which it is to be remembered.

Then the teacher may give three angles which may form the angles of a triangle, and when the constructions are made compare two figures from distant parts of the class. Similarly he may treat all the allied propositions. When taught in this way, the subject becomes so easy and attractive that it may be commenced at an early age.

If, as some teachers maintain, Spartan severity be necessary to secure mental discipline, then this plan of teaching elementary geometry will not be an improvement on that of forcing into the memory Euclid, pure and simple, without note or comment; but when the test of success is applied, I am sure the plan of making the early school work as easy and as pleasant as possible will require no other argument to support it. R. WORMELL

### Deschanel's "Heat"

IT is remarkable that Prof. Everett asserts $h$ to represent the reduced height of the mercurial column, when the *unreduced* height is carefully indicated in Fig. 264 by the same symbol $h$. Moreover it is distinctly stated on page 362 that "the tension of the vapour is evidently equal to the external pressure minus *the height of the mercury in the tube*."

Prof. Everett writes, "In some instances I have endeavoured to simplify the reasonings by which propositions are established or formulæ deduced" (Preface, part I). This would lead most people to expect simplicity, which includes accuracy; and they may well be astounded when they find not only unexplained but inaccurate formulæ. Prof. Everett's promises, and not his complaint, were the grounds of expectations which have not been realised.

THE REVIEWER OF DESCHANEL'S "HEAT"

Sept. 22

### Newspaper Science

MR. FORBES does not stand alone in his experience of newspaper science. The *Globe*, however, is not generally looked on as a scientific paper, and no one would be likely to go to it for information on matters other than political. What shall we say, however, to the following paragraph, copied *verbatim et literatim* from the columns of the *Mark Lane Express* for September 4?—

"CHARLOCK.—A correspondent inquires what he must do to abate the annoyance of it in his crops. We do not believe there is any mode of preventing its presence. Some seasons are distinguished by its appearance. We do not think they come from seed, but is the result of some electrical action producing them spontaneously. The late Duke of Portland used to say they need not sow white clover where bones were used freely; and where the pure white lime is used, clovers are seen without sowing seed. Also, if, as may be seen any season on the roads of Derbyshire, where the roads are repaired with white limestone, the clovers are present by the side of the wheel-tracks. The same may be seen on laying land down to permanent grass. Use farmyard manures, and the coarser grasses are seen; use road-scrapings and compost, and the finer grasses are sure to come. The charlock is an unwelcome visitor; but its removal in corn crops is often worse than the evil itself. 'Let both grow together till the time of harvest.' The seed has more value than some suppose, and when crushed will be found a good tonic. Nothing is given to us in vain."

Comment in this case also is needless. One hardly knows which most to admire in this rich paragraph; the independence of the trammels of the ordinary rules of syntax displayed by the writer; the teleological moral drawn at the end; or the contempt for science manifested in the assertion of the possibility of so highly-organised a plant as the charlock arising "spontaneously" in the ground. When such lamentable ignorance of the very elements of science is displayed by those who should be the leaders, what can we expect from the farmers themselves? Well may we exclaim, *Quis docebit ipsos doctores!*

ALFRED W. BENNETT

London, Sep. 23

## ICE FLEAS

THE water flea, *Daphne pulex*, is a well-known inhabitant of rivers and fresh-water lakes, and, being distinctly visible to the naked eye, often attracts the attention of water drinkers. Though a harmless crustacean, this little creature not only excited great interest in parliamentary committees during the last session, but exercised a very powerful influence over the choice of a water supply for the northern capital of Great Britain. The ice flea, if known at all, is certainly less celebrated, and probably by no means likely to be so potent in its parliamentary influence; nevertheless a short account of it may not be wholly uninteresting to the readers of NATURE.

During a recent ramble upon the Morteratsch Glacier, I turned over some of the isolated stones which lie upon its surface partially imbedded in the ice; under many of them I found hundreds of a minute jet black insect, which jumped many times its own length at a single spring, in a manner somewhat resembling the performance of a common flea.* The ice flea is about one-twelfth of an inch long. Viewed through a pocket lens, it was seen to have six legs, supporting a body obscurely jointed like that of a bee, and furnished with two jointed antennæ. The total length of the insect appeared to be about six times its thickness, the antennæ being about one-fourth as long as the body. The insects were not found under every stone, they generally occurred under flattish fragments of rock, presenting a surface of about a square foot, and having a thickness of from 2 to 4 inches. Stones of this size are sufficiently warmed by the sun's rays to melt the ice beneath them more rapidly than it is liquefied by the direct solar beams. A surface of rock absorbs luminous thermal rays better than does a surface of comparatively white ice, and it transmits these rays to the ice beneath it, partly by conduction and partly by radiation from its under surface. The stone thus melts its way an inch or two deep into the ice, forming for itself a kind of basin. Sometimes these cavities are watertight, and then any space between the stone and the walls of its basin are filled with water derived from the melting ice. Under such conditions I have never found any fleas beneath the stone. But occasionally the ice basin is drained, and it was under stones resting in such comparatively dry basins that the insects were found. In all cases nearly the whole of the fleas were found upon the ice, very few being attached to the stones. They were grouped together in shoals, so that probably forty or fifty of them frequently rested upon a single square inch of ice. On removing the stones, the insects were very lively, but this might be owing to their sudden transition from comparative darkness to direct sunlight

I saw no indications of food of any kind beneath the stones, indeed these insects must have a struggle for existence of a most severe character. Living in an atmosphere the temperature of which never rises above the freezing point, they must be continually exposed to inundations during the day by the stoppage of the drainage of the ice basin, whilst on clear nights severe frosts frequently threaten them with an icy grave. Again, during the day the roof of their habitation is, as it were, continually falling in upon them, and thus constantly exposing them to the risk of being crushed to death; for, as the ice melts beneath the stone, the latter is continually changing its points of support. It may be, however, that the crystalline structure of the ice causes it to melt with a corrugated surface, which provides everywhere valleys of sufficient depth to protect the fleas from destruction by the fall of the superincumbent mass of rock. We have also not to search far for a possible source of food. The cold of the glacier benumbs and kills thousands of insects which alight upon its surface, and bees, wasps, flies, and moths are frequently seen dead upon the ice. Then there is the so-called "red snow," and other allied organisms of similar habits, which may perhaps minister to the wants of this singular insect. Is the ice flea, like its irritating cousin, a nocturnal predatory insect, and does it issue from its dangerous abode at nightfall in search of frozen bees and butterflies? Perhaps some of the entomological readers of NATURE may be already acquainted with this animal, and be able to supply further information respecting it.

E. FRANKLAND

* My friend Prof. Eschenburg, of Zürich, had previously observed these insects on the Morteratsch Glacier, and it was his verbal account of them that led me to search for them.

## REMARKS ON PROF. WILLIAMSON'S NEW CLASSIFICATION OF THE VASCULAR CRYPTOGAMS

IN discussing the points at issue between Prof. Williamson and myself, it will be necessary for me to say a few words on stems in general, because we evidently have very different views of the construction of stems; and until we thoroughly understand each other, it is impossible for us to come to any definite conclusions. In a young dicotyledonous stem (see Oliver's "Lessons," p. 112, fig. 67) we find three things: a quantity of cellular tissue surrounded by an epidermis, and near the centre a series of young fibro-vascular bundles. As growth goes on, these separate bundles coalesce and form a central cylinder of united fibro-vascular bundles. These bundles leave a portion of the cellular tissue in the middle of the stem, which becomes the pith. Outside the fibro-vascular bundles we have also a small quantity of the cellular tissue, but it soon becomes to a great extent inseparable from the sub-epidermal cells. Other portions of the cellular tissue remain between the united fibro-vascular bundles, and form the medullary rays. In many stems and in most roots these rays are wanting, and the cellular tissue would therefore be divided into two portions by the united bundles. Each fibro-vascular bundle consists of two portions, which are separated by a layer of cells capable of division, the cambium. On the inner side of the cambium cells we have in general spiral vessels, porous vessels, and wood cells, while on the outer side we have the soft bast and bast fibres. The epidermis is soon thrown off in many cases, and is replaced by layers of cork-cells or peculiar thickened bast-fibre-like cells from underneath. The stem thus consists of three sets of tissues: (1) the limitary tissues, including epidermis, periderm, &c.; (2) the fibro-vascular bundles; and (3) the primitive tissue or *Grundgewebe* of Sachs (see "Mo. Mic. Journal," vol. iii. p. 160). In an older dicotyledonous stem we find the limitary tissues becoming largely developed, cork-cambium and layers of cork being formed. The fibro-vascular bundles have also largely developed, the cambium cells by division, and the conversion of these new cells into permanent tissue has formed a number of annual rings of wood-cells and vessels as well as layers of bast, while the primitive tissue only increases very slowly in the medullary rays, the pith not increasing, and the primitive tissue under the epidermis becoming lost in the rapidly-developing bark. Such is the structure of a dicotyledonous stem.

In a monocotyledon we have the same tissues, limitary, fibro-vascular, and primitive. The primitive tissue is largely developed, forming the cellular tissue by which the fibro-vascular bundles are surrounded (Oliver, "Lessons," p. 113, fig. 68). These fibro-vascular bundles differ quite as much in the nature of their cells and vessels as those of the dicotyledon, often one form being developed in excess of the other. The limitary tissues also develop cork and other cells. There is thus very little difficulty in comparing a very young dicotyledonous stem with that of a monocotyledon. In the monocotyledons the fibro-vascular bundles are closed, and therefore no annual layers are found; but in such stems as Dracæna, Aloe, Yucca, &c., we have the stem increasing in diameter. The outer cells of the primitive tissue divide and form not only new primitive tissue but new fibro-vascular bundles (Sachs, "Lehrbuch der Botanik," ed. 2, p. 103, fig. 90). Prof. Williamson would probably call these Exogenous Endogens.

When we come to the Lycopod and Fern stem, we find the same parts—limitary tissues, fibro-vascular bundles, and primitive tissue. In ferns the bundles are more or less scattered, like those of the monocotyledon, while in the Lycopods we either have them separate or else all joined together to form a central axis (see Sachs, *op. cit.*, figs. 66 and 89). Round this central axis in Lycopods we have the primitive tissue, while outside we have the epidermis often with peculiar thickened cells underneath, forming part of the limitary tissues. In Mosses, Charas, and Thallophytes we have only the primitive and limitary tissues, the fibro-vascular bundles being entirely absent. In some of the Thallophytes, however, as in Lessonia, we may have the primitive tissue increasing just as in Dracæna.

In Lepidodendron, as in some of our modern Lycopods, we have a central axis of combined fibro-vascular bundles, and a large quantity of primitive tissue, no longer all parenchymatous, as in many of our recent Lycopods, but mostly prosenchymatous, as in *L. chamæcyparissus*. This primitive tissue went on increasing year after year, new cells forming by division, these being soon changed into hard prosenchymatous cells. Outside we have the limitary tissue strengthened, as in some of our recent species, by remarkable prosenchymatous cells. In Lepidodendron the primitive tissue was capable of dividing in the same way as that of Dracæna. The stem increased year after year, not by growth of the wood-cells, &c., of the fibro-vascular bundles, as in a dicotyledonous stem, but by additions to the primitive tissue. I never denied that the Lepidodendron stem increased in diameter, but pointed out that the increase takes place by multiplication of the cells near the periphery of the primitive tissue, the portion not likely to be often preserved in Lepidodrendon stems. This mode of growth is quite compatible with the statement that the fibro-vascular bundles are closed as they are both in Ferns and Lycopods. As Prof. Williamson admits that "the large vascular cylinder of the fossil forms is a development of what is seen not only in *Lycopodium chamæcyparissus*, but in every one of the numerous Lycopods of which I have examined sections," there is no difficulty in settling the matter. The cylinder in *L. chamæcyparissus* is part of the primitive tissue, not of the fibro-vascular bundles. Such being the case, the central axis of Lepidodendron is not a "vascular medulla," but a series of closed fibro-vascular bundles. In Lepidodendron we have merely a pseudo-exogenous growth taking place in the primitive tissue, while in Gymnosperms and Dicotyledons we have true exogenous growth in the fibro-vascular bundles. In Ferns this pseudo-exogenous growth is not likely to take place, as a fern produces only a few large leaves, while in a Lycopod or Lepidodrendron, which produces numerous small leaves, water for purposes of transpiration would have to be rapidly supplied in yearly increasing quantities. This is provided for by the increase which takes place in the wood-cells of the primitive tissue, not as in Dicotyledons, by additions to the wood-cells of the fibro-vascular bundles. Prof. Williamson has been led away by the mere superficial resemblance of the parts, and has never tried to understand the homologies of these stems. He has mistaken the united closed fibro-vascular bundles in the centre of the stem for a vascular medulla, *i.e.*, for a portion of the primitive tissue; and he has mistaken the woody cylinder surrounding this—which is a modified portion of the primitive tissue—for the united fibro-vascular bundles of a dicotyledon. After making two such fatal errors, can his proposed new classification be considered of any value?

W. R. M'Nab

## *A NEW DYNAMETER*

It need not be said that in astronomical observation it is always desirable, to say the least of it, to have a tolerably correct estimate of the magnifying power actually in use. This has hitherto been only attainable either by means of the maker's valuation, or through the employment of the apparatus unfortunately termed a "dynameter," a word which every classical scholar would wish to see as soon as possible dismissed from circulation. The former alternative is, I am sorry to say, often far from reliable; the latter involves an outlay not within the reach of every astronomical student. The Rev. E. L. Berthon, Vicar of Romsey, Hants, well known already for many ingenious and valuable inventions, has recently devised a little apparatus for attaining the same object, which deserves high commendation. Its very moderate price places it within the reach of all; and its accuracy appears equal to that of instruments of more complicated construction and higher pretension. I have heard on excellent authority that very little dependence can be placed on the estimates of magnifying powers too frequently furnished to purchasers. Eyepieces are both constructed and rated too frequently by "rule of thumb," and their real, if measured, will be found widely different from their nominal power. Some opticians, as, for instance, the celebrated reflector-maker Short, have had an unfortunate reputation for exaggerating the power of their instruments, and without any suspicion of misrepresentation: such has been the case even at the celebrated Optical Institute of Munich, as appears by the corrections made by W. Struve in the numerical values of the Dorpat oculars, 94, 140, 214, 320, 480, 600, 800, 1,000, 1,500, 2,000, being respectively lowered by him on trial to 86, 133, 198, 254, 420, 532, 682, 848, 1,150, 1,500. In this instance, it is possible that some different mode of measurement may have led to the discrepancy. Uncertainty, it may be suspected, occasionally arises from this cause. I once undertook, at the special request of a friend, to verify with a double-image dynameter the power of some oculars constructed by a very eminent optician, whose name was an abundant guarantee for his good faith; but the results, on which I bestowed a great deal of care and trouble, trusting only to averages of many repeated measures, did not agree satisfactorily with the maker's statement. I do not know whether it may have been generally noticed, but the remark is a very obvious one, that the limit of numerical error increases with the power, so that in the case at any rate of ordinary dynameters, minute accuracy in the estimates of very deep oculars bears evidence of its own futility. If it represents anything of value, it can only be the care and attention with which a mean was deduced from repeated trials; but even this would be better expressed in round numbers, as less likely to convey an erroneous impression to the uninitiated.

Probably some form of apparatus may yet be devised which may secure greater minuteness in the measurement of very high powers, without entailing a disproportionate outlay. In the mean time Mr. Berthon's invention may be safely recommended as likely to prove of especial advantage to observers in general.

T. W. WEBB

## THE NEW GANOID FISH (CERATODUS) RECENTLY DISCOVERED IN QUEENSLAND

### II.

It appears to me that there is not the least justification for separating the living fish *generically* from that extinct form, the teeth of which are known by the name of *Ceratodus*. Immediately after its discovery became known, and before we knew more than the outlines of its external characters, views to the contrary were expressed, evidently based on the assumption that a genus was not likely to have survived from the Triassic epoch. This is certainly a remarkable fact, but it is not more surprising than the other, viz., that fishes from one of the oldest epochs from which fish remains are known, are most closely allied to *Ceratodus*. We know that the same *species* occur on both sides of the Central-American isthmus—that is, that they have existed at, and remained unchanged from, the time when the Pacific and Atlantic Oceans were connected by one or several channels. Therefore, it would appear that there is a greater persistence in the ichthyic type than we have hitherto been willing to admit.

Whoever has compared the teeth of *Ceratodus runcinatus* from the German Muschel-Kalk, and those of the Indian species described by Prof. Oldham, with the teeth of the living species, must admit their generic identity; and if the Australian form really grows to the enormous size stated by some colonists, I have no doubt that the teeth of such large examples cannot be distinguished from the fossils mentioned. So close a resemblance in highly specialised teeth like those of *Ceratodus* is generally admitted to be of generic significance. Unfortunately no other part of *Ceratodus* is known to have been preserved in the fossil state to serve as a further guide in answering this question The strata in which the teeth are found must have been much disturbed, as no two teeth have ever been met with *in situ* together; but I cannot help thinking that sooner or later the vomerine teeth will be recognised. From their smaller size they would easily have escaped observation; and their shape (which differs so much from that of the molars) would scarcely have allowed an observer to connect them with the genus to which they in reality belong.

The next most nearly allied forms are the American and African *Lepidosirens*, a genus at present unknown in a fossil state. The skeleton (in some respects even to its minute details), structure of the fins, dentition, internal nostrils, three-chambered heart, co-existence of a lung with gills, intestinal tract, small size of ova: all present the strongest possible evidence of the close relationship between these fishes. The points in which they differ are of such a nature that characters indicative of an amphibian affinity in *Lepidosiren* are modified in *Ceratodus* according to a distinctly ichthyic type, thus tying, as it were, *Lepidosiren* firmly to the class of fishes. The longitudinal valves in the bulbus arteriosus of *Lepidosiren*, reminding us of a similar structure in the heart of Batrachians, are replaced by truly Ganoid valves in *Ceratodus*; the imperfect gills of the former genus are as perfect in the latter as in any other fish; the lungs of *Lepidosiren*, paired as in a frog, are confluent into a single air-bladder-like sac in the Australian form; instead of the closed ovaries with a developed oviduct and fallopian tubes of *Lepidosiren*, we find the ovaries of the Barramunda open, discharging the ova into the abdominal cavity, as in the Salmon family and other fishes. These differential characters may be considered by some of sufficient importance to refer the *Lepidosirens* and the Barramunda to two distinct groups.

Some of the oldest fishes, known from the Devonian epoch, are designated by the names *Ctenodus* and *Dipterus*. Whether they should be referred to one genus or two is a question about which opinions are divided, and into which I need not enter here. They are evidently representatives of the same ichthyic type as the Dipnoi of the present epoch. The similarity of the large molars to those of *Ceratodus* has been recognised for a long time; but it is only recently that I have been able to ascertain, in an example in the Jermyn Street Museum, the presence of a pair of small vomerine teeth. Moreover, the same example presents as good evidence as we can expect in a fossil, that the nostrils are placed within the mouth. These characters are combined with the presence of acutely lobed paddles, and of a notochordal skeleton; but there is the great difference that the end of the vertebral column is heterocercal, instead of being diphyocercal, as in *Lepidosiren* and *Ceratodus*. Therefore *Ctenodus* will form the type of a distinct dipnöous family.

Thus, then, we arrive at the conclusion that *Lepidosiren*, far from being an isolated representative of a distinct sub-class of fishes, is only one of the representatives of a sub-order of Ganoid fishes, characterised by the position of the nostrils within the mouth, by paddles supported by a jointed axis, by lungs co-existent with gills, by a notochordal skeleton, and by the absence of branchiostegals. The term "Dipnoi" may be retained for this sub-order, which was developed in the earliest epochs from which fish-remains are known, while we have scanty evidence of its presence in Liassic and Triassic strata, and, in the present state of our knowledge, it appears to be lost, until we find it again represented by three living forms in the present period. Probably some other extinct genera belonged also to this sub-order, but their remains are in too fragmentary a condition to admit of an exact definition of their affinities.

During the examination of *Ceratodus*, I had so frequently occasion to refer to structural peculiarities of the *Plagiostomata* (Sharks and Rays), that I was induced to reconsider the relations existing between this sub-class and the Ganoid and Teleosteous fishes; and I came to the conclusion that the two former are much more nearly allied to each other than the Ganoids are to the Teleostei. The Plagiostomes were considered to be a distinct sub-class of fishes on account of the highly-developed state of the organs of reproduction in the female, besides the presence of copulatory organs in the male. Their ova are different from those of other fishes, having a very peculiar shape, and shell with adhesive appendages, and being of an unusually large size, and few in number. They are impregnated internally; some of the species are viviparous. They have from five to seven external gill-openings. Although in external appearance a Ray and a Shark are apparently very different, yet these extremes are connected by a number of intermediate forms, and they form altogether one of the most homogeneous groups in the zoological system.

On the other hand they agree with the Ganoids in having, in addition to the ordinary two divisions of the fish-heart, a third contractile chamber. This bulbus arteriosus is very different from the *Bulbus aortæ* of the Teleosteous fishes, where it is simply a swelling of the walls of the aorta, not contractile, without valves in the interior, and separated from the heart by two valves opposite to each other. If this remarkable arrangement is deemed (and, I think, very justly) to be sufficient to separate the living Ganoids as a sub-class from the *Teleostei*, it is certainly significant enough to suggest the union of the Ganoids with the Plagiostomes. Moreover, this character is supported by two others of great importance, viz., the presence of a spiral valve in the intestine, which is found in a more or less developed state in all Ganoids, Sharks, and Rays, but is entirely absent, even in a rudimentary condition, in the Teleostei, and by the optic nerves being placed side by side, and not decussating, as is the case in all our ordinary fishes. Of the characters connecting these fishes I will refer to one other, as it has been described above, namely, that the fore and hind limbs of the Plagiostomes are also paddles supported by a cartilaginous structure, as in the *Dipnoi*.

The evidence in favour of a union of Ganoids with Plagiostomes is rendered complete by the Chimæras, which hold a surprisingly intermediate position. They are Sharks in external appearance and with regard to the structure of their organs of propagation; they are provided with the same copulatory organs, and their ova are large, enclosed in a horny case, and provided with adhesive appendages. Many species of Sharks, when in a very young state, are provided with a double dorsal series of spines (permanent in certain Rays), which are lost with age; and this most remarkable developmental character occurs likewise in young Chimæras. On the other hand, there is only one external gill-opening on each side, as, for instance, in *Ceratodus*, which, on the other hand, shows the first step towards a coalescence of the gills with the walls of the gill-cavity. The skeleton is notochordal, and the palatal and maxillary apparatus coalesce with the skull, as in *Dipnoi*, which is not the case in any Plagiostome; likewise the dentition approaches that of *Ceratodus*. Finally, Sir P. Egerton has drawn attention to the most important fact that the dorsal spine is articulated to a neural apophysis, and not merely implanted in the soft parts and immovable, as in Sharks.

Thus, then, the union of these fishes in one sub-class appears to me fully justified, as far as the living forms are concerned; but, as is implied by the name *Palæichthyes*, which I have proposed for this sub-class, it is intended to comprise also a great variety of forms from the Palæozoic Era, in fact, the predecessors of the Teleostean fish-fauna of the present period. I am aware of the objections that may be urged. First, it may appear to some to be an improper proceeding to unite in the same sub-class fishes of so different an appearance as a Shark and Lepidosiren, or as an Amia and a Pteraspis; but let them consider what a comprehensive category a sub-class necessarily is—that the diversity between the fishes just named is not greater than that existing between a Sun-fish (*Orthagoriscus*) and an eel, or between a viviparous Embiotoca and a Loricaria, forms admitted by every ichthyologist of the present day as members of the same sub-class, that of Teleosteans. In fact the Palæichthyes are composed of a similar series of modifications as the Teleosteans, some of the members of one sub-class exhibiting marked analogies with those of the other, in the same manner as is the case with Placentalia and Implacentalia among mammals. To mix up ganoid-looking Teleosteans, like the Siluroids, with Ganoids, is as little in accordance with the advanced state of our ichthyological knowledge, as the union of Salamandra with Lacerta would be. Secondly, other naturalists may consider it very hazardous to establish a division, of which the majority of members are extinct and known from remains of the hard parts only, and to characterise it by peculiarities of the soft parts. But why should we not make use of zoological evidence for the completion of the imperfect palæontological record, with the same benefit to science as in other cases, since not a few zoological problems have been, or can only be, solved by reasoning founded on palæontological facts? If, in the determination of affinities, we were to limit ourselves only to the consideration of those parts which have been preserved in the process of fossilisation, we could never expect any other result but the creation of most artificial assemblages of forms, although the characters of some natural families, or even orders, might be partly recognised.

On the one hand, we know that all the Teleosteous fishes, that is, the types which are predominant in the present and next preceding epochs, and which were but sparingly (*Coccosteus?*) represented in the Palæozoic, if they existed at all, agree, in spite of all other differences, with one another in possessing a two-chambered heart, with a rigid bulbus aortæ and decussating optic nerves, and in never exhibiting a trace of a spiral valve in the intestine.* On the other hand, we find that the few ichthyic types which have survived from the Palæozoic Era into our period, and those of which no immediate representative is known in that Era, but which approach that Amphibian fish-type by unmistakeable characters, agree, in spite of all other differences, in having a three-chambered heart, non-decussating optic nerves, and a spiral valve in the intestine. These are facts; and it seems to be a fair conclusion that the members of the Palæozoic fish-fauna had essentially the same organisation of those soft parts as their surviving representatives.

In conclusion I may shortly pass in review the living *Palæichthyes*, especially in regard to their distribution over the globe.

1. Of the order *Plagiostomata* or *Marine Palæichthyes*, 140 species of Sharks, distributed among 39 genera, are known, and 150 species of Rays, belonging to 25 genera. They inhabit nearly all the seas of the globe, decreasing in number from the tropics towards the poles. Only very few enter or live in freshwater.

2. The order *Holocephala* contains only four species, viz., three Chimæras and one Callocephalus; they are restricted to the seas of the temperate zones of both hemispheres, and are absent between the tropics.

3. The order *Ganoidei* or *Freshwater Palæichthyes* is represented by one species of *Amia*, from North America; three species of *Lepidosteus*, from the same region, but extending southwards into Central America and Cuba; two species of *Polypterus* (*Calamoichthys*) from the tropical parts of Africa; two species of *Polyodon*, from the Mississippi and the Yantsekiang; about twenty-five Sturgeons, from the temperate and sub-arctic regions of the Northern Hemisphere; two species of *Ceratodus* from tropical Australia; one species of *Lepidosiren* from the Amazon river, and one of *Protopterus* from tropical Africa. Although the majority of the Sturgeons pass a part of the year in the sea, they must be regarded as freshwater fishes like the migratory Salmones, because they deposit their spawn in the rivers, where they also pass the first period of their growth; some species never enter the sea at any period of their life, and none are known to propagate in the sea.

The total number of fishes known at present being about 9,000, the *Palæichthyes* form only 3·6 per cent. of that number. But from the extent of the regions hitherto ichthyologically unexplored, and from the numerous additions annually made to the list of known forms, I do not believe that we are acquainted with much more than one-tenth of the species of fishes actually existing.

ALBERT GÜNTHER

* From these considerations *Amphioxus* and the *Marsipobranchii* are excluded, the former being evidently the type of a distinct sub-class.

## *METEOROLOGY IN AMERICA* *

### III.—SELF-REGISTERING INSTRUMENTS

INVALUABLE as is the ordinary barometer, the most valuable instruments are those which are automatic, or self-registering. Prominent among those used in America are the Self-recording Barometer and Meteorograph invented by Prof. G. W. Hough, Superintendent of the Dudley Observatory. Lord Rosse's telescope has not done more for astronomy than will the self-registering barometer do for meteorology.

The diagram, Fig. 7, will illustrate the method of registering the height of the barometer and thermometer on a single sheet, by the use of one set of mechanism in these simple yet complete and consummate contrivances.

Let D be a drum six inches in diameter and seven inches in height, covered with a sheet of ruled paper. This drum is presumed to revolve at any convenient rate, say one inch per day. Let L be an iron or brass bar twenty-four inches in length, mounted on an axis passing through the point *c*. Let P be a steel pen attached to the end of the lever projecting over the centre of the drum. Let P′ and P″ be platinum wires attached to the lever at three inches on either side of the axis *c*. The wire P′ is over the shorter leg of a siphon barometer, and the wire P″ passes into the end of an open mercury thermometer.

Now if the lever L be elevated at the end over the drum, the wire P′ will touch the top of a float resting in the shorter leg of the siphon barometer If then a battery, B, and electro-magnet, E, be arranged as in the diagram, when contact is made with the float, a current of electricity will pass through the circuit, and the electro-magnet E is operated. If then, when the circuit is completed, a

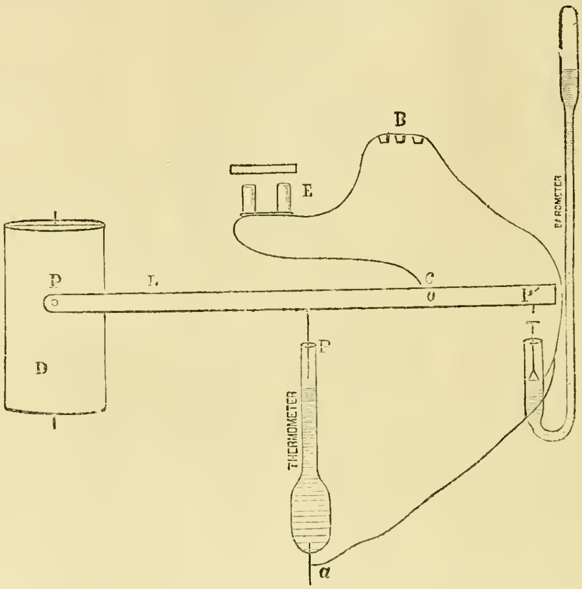


FIG. 7.—REGISTRATION OF THE HEIGHT OF BAROMETER AND THERMOMETER

blow be struck on the pen P, by means of the electro-magnet, or a hammer unlocked by it, the dot on the drum sheet will indicate the height of the barometer at that time. It is obvious that as often as the lever is elevated a record will be made. For the barometer an hourly record will be found to be sufficient.

If the lever L is rigid and firmly mounted, the mere measurement of height by means of electrical contact can be carried to almost any degree of precision.

It was found from numerous experiments made some years since, that the magnetic circuit is not completed for a distance of one ten-thousandth of an inch. Therefore, whatever source of error there may be in the results recorded by this method is due to the barometer itself. In practice, from records extending over nearly one year, it is found that the results are inside the errors of reading from the drum sheet.

A long experience has led to the conclusion that this degree of precision is sufficient for the investigation of barometric changes, and is but little outside the limit of error from reading a standard barometer.

An examination of the diagram will also show at a glance how the height of the thermometer is recorded. It should, however, previously be stated that the thermometer is a little larger than those in ordinary use, and has a platinum wire, *a*, cemented in the bulb, communicating with the mercury in the inside.

The following is a general description of a machine constructed for the Signal Service at the request of the chief signal officer.

It registers hourly the barometer and wet and dry bulb

* We are again indebted to *Harper's New Monthly Magazine* for the continuation of the article by Prof. Maury, and the woodcuts which we reproduce this week.

thermometers, and thus shows the atmospheric pressure, the temperature of the atmosphere, and its hygrometric condition—*i.e.*, its condition of moisture or dryness.

The engraving, Fig. 8, is a perspective view of this instrument. The recording lever, A, is a bar of iron about two feet in length, nearly balanced on the axis, supported by the clock-frame, C. The clock is constructed with rather stronger gearing than an ordinary movement, its office being to elevate and depress the lever A hourly, regulate the drum, D, and raise the two striking hammers, H and H′. It is provided with a half-second pendulum, and requires winding once in two days, the weight dropping in that time about three feet.

The shorter leg of the siphon barometer is shown at B, and the wet and dry bulb thermometers at T′ and T. Directly over the leg of the siphon, as also over the two

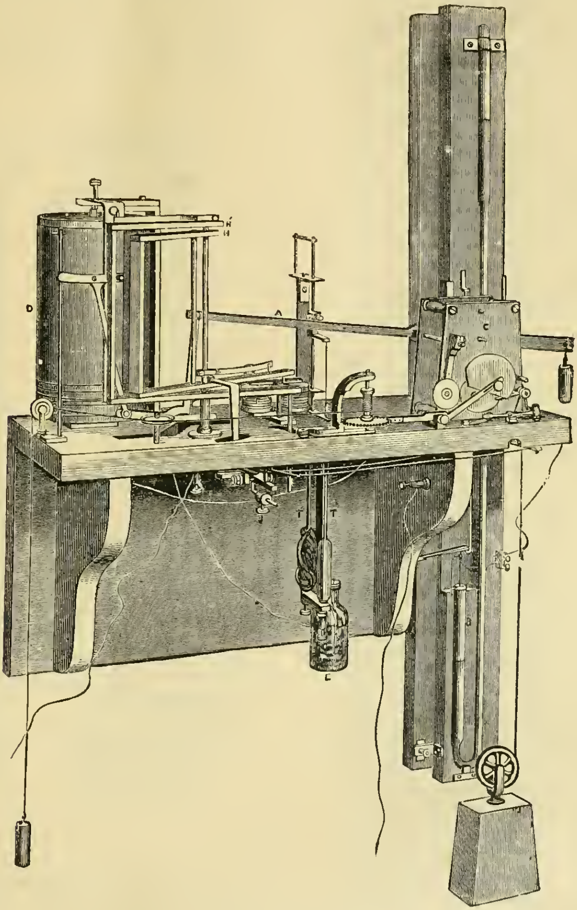


FIG. 8.—THE METEOROGRAPH

thermometers, the lever A supports a carriage, which is depressed or elevated whenever the lever A is in motion. The registering point, G, is connected with the lever, as shown in the diagram; and the curvilinear motion of the end of the lever is converted into rectilinear by allowing G to slide against a vertical steel rod.

To illustrate the action of the machine, we will suppose the lever A has reached its lowest point, the registering pen G being at the bottom of the drum. Now, in order that we may be able to register the barometer on any part of the drum sheet, it is necessary that the striking hammer should be elevated and locked before the upward motion of the lever commences. As the hammers are raised by means of an arm carried by the hour shaft of the clock, at the point where the hammers begin to rise the snail for elevating the lever A is cut away, so that it remains at rest during a period of fifteen minutes, the time required for elevating the hammers H and H′. As soon as this is accomplished, the lever begins to rise slowly by means of the double snail on the hour shaft, the time required for traversing the drum being about fifteen minutes. When the position of the lever is such that the carriage in the rear of the clock touches the float in the shorter leg of the siphon, an electric current is established through the magnet, F, which unlocks the hammer H, causing the pen G to make a record on the drum sheet. After the lever has reached the top of the drum, it remains at rest fifteen minutes while the hammers are being raised, when it is gradually depressed. So soon as the platinum wires—attached to the carriage over the thermometers—touch the surface of the mercury in the thermometer tubes, electric currents are established through the magnets F and J, simultaneously or successively unlocking the hammers, and, as the case may be, making records as before.

A complete double motion of the lever requires one hour. During this time the barometer and wet and dry bulb thermometers have each been recorded once. The records of the barometer and thermometers differ in time about half an hour. The wet and dry bulb thermometers are recorded within about one minute of each other, depending on the difference between them.

One of the most marked and wonderful features of the invention of Prof. Hough is that it prints its own records. And this is done by a single screw, which rises or falls

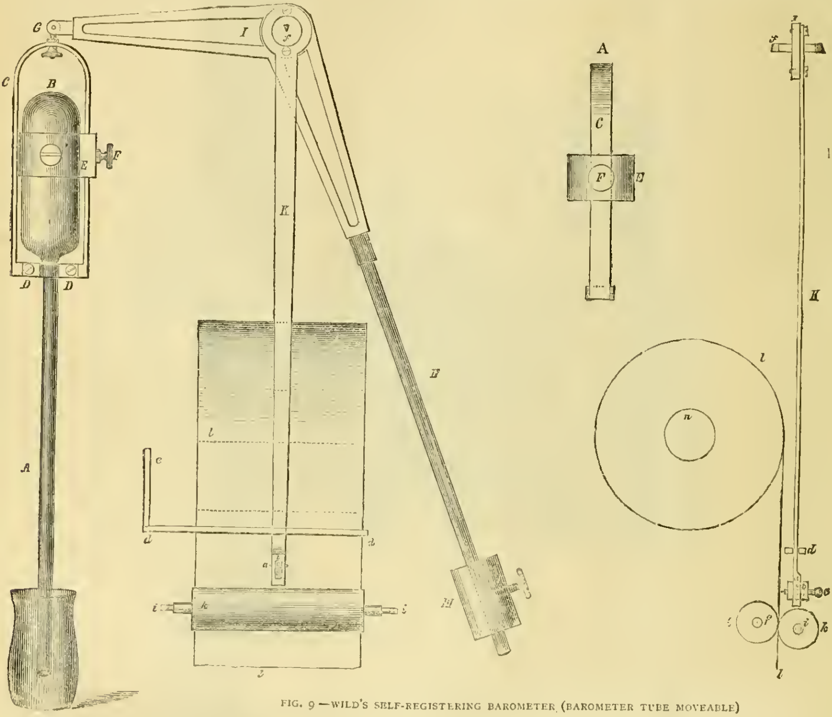

FIG. 9—WILD'S SELF-REGISTERING BAROMETER (BAROMETER TUBE MOVEABLE)

with the mercury in the barometer. This screw carries a pencil, which traces upon a revolving cylinder or roll of paper a line showing the minutest movements of the column of mercury for every minute in twenty-four hours. This same screw also gives motion to a series of wheels which carry types, by which, at the end of every hour, the height of the column of mercury is printed on a slip of paper to the accuracy of the *thousandth part of an inch!*

One of the most beautiful and simple contrivances used is a Wild's Self-registering Barometer, of which we give a cut one quarter the actual size. It scarcely needs explanation, except to say that the tube A is suspended in a cistern of mercury, represented on the left of Fig. 9. As the atmospheric pressure changes, the level of the mercury changes in the cistern, and the tube A rises or falls as the atmospheric pressure increases or diminishes. The weight of this tube as it floats in the mercury, and also that of the arm *I*, which supports it at *G*, is exactly balanced by the arm *II*, to which is attached a sliding weight, *III*, adjustable by a small thumb-screw. *K* is a steel crayon-holder fixed to the balance *I II*, and to which is fixed a crayon, *c*, whose point in seen in B to impinge upon a sheet of paper, *l l*. This sheet s moved by clock-work. When the atmospheric pressure is increased, the tube *A* is forced to rise a little out of the mercury in which it floats, and as it rises at $G$, the arm $I$ is elevated. The crayon holder, being fixed on the balance at the fulcrum, $f$, by two little screws, swings a little to the left, and the crayon which it carries with it makes a mark on the paper beneath it, which mark indicates the rise of the barometer, or the increase of atmospheric pressure. If the pressure decreases, the pencil, of course, moves in the opposite direction, and shows the barometric fall. The roll of paper on which the record is made by this automatic instrument is divided into rectangular parts, each one of which exhibits the atmospheric variations for twenty-four hours. At the end of every day this part of the roll is detached and put by to be bound up in book form in the records of the office in which the instrument is kept.

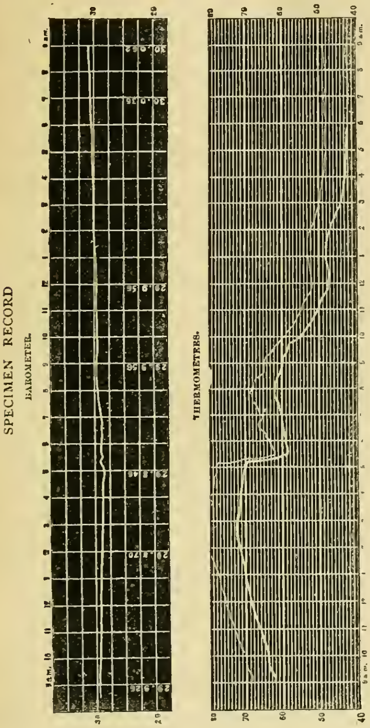


FIG. 10.—DRAPER'S PHOTOGRAPHIC REGISTER OF BAROMETER AND THERMOMETER AT NEW YORK, APRIL 28, 1871
*The upper line of the Thermometer is the Dry Bulb, the lower line is the Wet Bulb*

The roll of paper is on a reel, $n$, passing between two rollers, $g$ and $k$, as seen in B (Fig. 9).

By these perfectly simple devices, instead of obtaining only three daily recorded observations, the observer at every station gets a continuous and perpetual record for every second in the day. That is to say, instead of getting, as by the common barometer (observed three times a day), observations for three seconds in twenty-four hours, he gets them for as many seconds as there are in twenty-four hours, or 86,400. Thus it follows that the value of the self-registering barometer, as compared with the ordinary one, is as 86,400 to 3!

The marvellous accuracy and exquisite nicety with which all the observations forwarded to General Myer by the observers are marked ought to assure the public that nothing is wanting to give reliability to the published results and the "probabilities" issued from his officers. A self-registering barometer, as well as other instruments

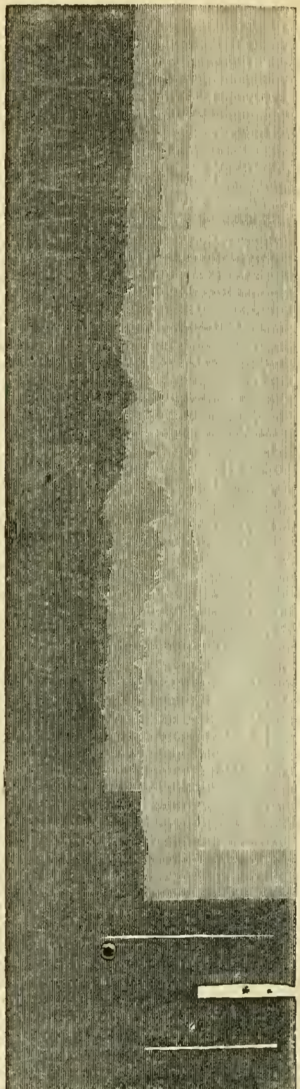

FIG. 11.—PHOTOGRAPH OF A STORM.—(*Print from Photographic Register from Noon, December 11, 1870, half-inch per hour.*) *Two Thermometer and One Barometric Curve*

of equal sensitiveness, will be used by all the observer-sergeants. It is scarcely possible for this invaluable instrument to suffer derangement or to get out of order.

A third most beautiful and sensitive self-registering instrument is that of Mr. Peelor, of Johnstown, Pennsylvania, used with great success and satisfaction by the Signal Service. This needs no battery, no electricity, to work it. A simple clock-work is all that is required, and its operations are as exquisitely accurate and trustworthy as the best navy chronometer.

A barograph and thermograph made by Mr. Beck, of London, similar to those used in the Kew Observatory, are on trial in the Signal Office, and good results are hoped from them. Their beautiful machinery might also be mentioned and described, but our space fails. Indeed, our limits have allowed mention to be made only of the most novel instruments employed by the signal offices. A specimen record of one of these is presented in Fig. 10, showing the synchronous readings, on a given day and at a given place, of the thermometers (wet and dry bulb), the hygrometer, and the barometer, all upon one sheet of paper.

We have already spoken of the beautiful adaptation of Prof. Hough's Meteorograph to the work of printing its own registrations. The mechanics of meteorology have been advanced one step higher than this, and the registrations of the automaton are instantly and perfectly photographed. The sheet of paper, suitably prepared for photographic impressions, is made to slide, by means of clock-work, before a gas flame. The mercury in the tubes protects a portion of the paper from the action of the light of the lamp, while above the mercury the rays of the lamp fall unobstructed upon the paper, and, making their impression, reveal the exact height of the mercury in the tubes.

The "photograph of a storm," Fig. 11, shows the movements of the mercury in the two thermometers and barometer for twelve hours.

This process, by which the weather is photographed, is employed by General Myer, and these necessarily exact records will prove most attractive pictorial representations of the great storms in the atmospheric ocean for the study of meteorologists all over the world.

## *THE INTERNATIONAL EXHIBITION AT VIENNA FOR* 1873

THE Emperor of Austria has appointed an Imperial Commission to carry out the project of an International Exhibition to be held at Vienna in 1873. The members of the Commission held their inaugural meeting in the hall of the Imperial Academy of Sciences at Vienna on Sunday, the 17th of September, under the presidency of the Archduke Rainer.

The Exhibition is intended to be opened on the 1st of May, 1873, under the especial patronage of the Emperor and his brother, the Archduke Charles Louis. The Commission, which is composed after the models of the English and French Commissions, consists of the Archduke Rainer, president; the Lord Steward of the Imperial Household, Prince zu Hohenlohe-Schillingsfürst; the Imperial Chancellor, Minister of the Imperial House and for Foreign Affairs, Count von Beust; Prince zu Liechtenstein. Prince Schwarzenberg, Count Festetitz, and Count Potocki, vice-presidents; and the Lord High Chamberlain, Count Folliot de Crenneville, and other high courtiers, the Ministers and heads of departments, the Presidents of both Houses of the Reichsrath, the presidents of the chief artistic, commercial, and scientific societies, and a number of gentlemen who have distinguished themselves in the various branches of science, art, and industry.

The entire arrangements have been entrusted to the Austrian Consul-General at Paris, Privy Councillor Baron de Schwarz-Senborn, who has been nominated Director-General of the Exhibition. Local committees are about to be formed in the various provinces of Austria and Hungary, and a special Royal Commission is to be appointed at Pesth. The objects to be exhibited will be classified into 26 different groups, as detailed in the subjoined programme.

One great feature of the Exhibition will be an arrangement for the classification of the productions of all countries in groups corresponding with their geographical position, and great pains will be taken to render the Oriental department in every way worthy of the almost inexhaustible resources of the Indian Empire. The position of Vienna is admirably adapted for this, having, besides the waters of the Danube, a direct communication with all the important harbours of the Levant *viâ* Trieste. The arrangement of the Eastern department will be confided to the Austrian Consul at Constantinople, Dr. de Schwegel, who has already acquired a great reputation for his knowledge of Oriental habits and productions.

A new feature of the Exhibition will be an arrangement by which the treasured collections of the various museums of London, Paris, Berlin, Moscow, Lyons, Munich, Stuttgard, &c., will appear in simultaneous position, and it is further intended to represent a history of inventions, a history of prices, a history of industry, and a history of natural productions, so that the world's progress in arts, science, industry, and natural products, will thus be brought into contrast. The Emperor of Austria has granted the use of the "Prater" for the site of the exhibition, and Mr. Scott Russell is at present in Vienna consulting with Baron von Schwarz as to the design for the building. Chevalier de Schaeffer, Director of the Austrian Consulate General in London, who gained great experience at the London and Paris Exhibitions, has been entrusted with the preliminary arrangements respecting the contributions to be sent to the Exhibition from Great Britain.

The objects to be exhibited will be classified in the following twenty-six groups:—1, Mining and Metallurgy; 2, Agriculture and Forestry; 3, Chemical industry; 4, Articles of food as industrial products; 5, Textile industry and clothing; 6, Leather and india-rubber industry; 7, Metal industry; 8, Wood industry; 9, Stone, Earthenware, and Glass industry; 10, Hardware industry; 11, Paper industry; 12, Graphical Arts and Industrial Drawing; 13, Machinery and means of transport; 14, Scientific instruments; 15, Nautical instruments; 16, Military accoutrements; 17, Maritime objects; 18, Architectural and Engineering objects; 19, Cottage houses, their interior arrangements and decorations; 20, Peasant houses, with their implements and arrangements; 21, National domestic industry; 22, Representation of the operation of Museums of Art and Industry; 23, Ecclesiastical Art; 24, Objects of Art and Industry of former times, exhibited by amateurs and collectors; 25, Plastic Art of the present time; 26, Objects of Education, Training, and Mental Cultivation.

Arrangements will be made for temporary exhibitions of such articles which by their nature do not admit of an exposition of long duration.

During the time the Exhibition is held, International Congresses are contemplated for the discussion of important questions to which either the Exhibition itself may give rise, or which may be specially suggested as themes suitable for international consideration.

The arrangement of the Exhibition will be geographical, that is to say, according to countries, but in such a manner that the different territories of production shall appear as nearly as possible in the same order as they are situated naturally in the direction from the west to the east.

## *SCIENTIFIC USE OF THE MONT CENIS TUNNEL*

AT the sitting of the French Academy of Sciences on the 18th inst., M. Elie de Beaumont read an elaborate paper on the scientific instruction which may be derived from a close examination of the collection which is to be exhibited in the School of Mines in Paris of specimens of the strata obtained from the Mont Cenis Tunnel. This collection, which consisted originally of only 127 specimens, has received 69 new specimens, which brings the total number to 196 altogether.

The total vertical thickness of the strata explored was more than 7,000 metres. The general colour is grey, or, rather, black, and the colouring matter is mostly carbon. Very few fossils were met with, having been destroyed by the subsequent crystallisation.

The disturbances which have created Mont Cenis and made it emerge from the bottom of the sea have produced many cracks and faults. But all these faults have been filled up with quartz in a perfect manner in relatively modern times. The infiltration amounts practically to nothing. The only spring which was discovered is situated near Modane, and gives only seven gallons per minute. The water is cold. The contractors were obliged to send to Modane and Bardonnèche for the water required for drinking, and for grinding the stone.

Mont Blanc, although 4,800 metres above the level of the sea, is only 3,500 above its own base. The vertical section of the perforated strata is thus equal to two Mont Blancs; and it is something like one whole Himalaya. M. Sismonda, Professor of Geology at Turin, presented to the Royal Academy of Sciences, Turin, in the sitting of the 5th of December, 1866, a paper entitled *Nuove osservazioni geologiche sulle rocce anthracitifere delle Alpi*, at the end of which was printed a map drawn by M. Sismonda twenty-five years ago, and exhibiting the theoretical succession of strata. Everything was found in the place where it was supposed to be by M. Sismonda.

No artesian well has ever given an opportunity of comparison with the perforation of Mont Cenis, as the deepest bored by European engineers is only 1,000 metres, and by the Chinese only 3,000 metres.

The Academy listened during more than an hour to the lecturer. M. Faye presented to the learned Perpetual Secretary the hearty thanks of the Academy, and expressed a wish that a series of pendulum experiments should be conducted on the top of Mont Cenis as well as in the central part of the tunnel, to test the effect of the mass of the mountain on the time of the oscillations.

## *NOTES*

WE believe that the stations to be occupied by the Eclipse Expedition are Baikul, Gunote, and Manantoddy, near the Malabar coast; Poodoocottah, near Trichinopoly; and Jaffa, in Ceylon. These arrangements are necessitated by the information received as to the weather chances from the Viceroy of India and the Governor of Ceylon, who are taking the warmest interest in the intended operations. It is hoped that Prof. Stokes will take charge of the Expedition, and in this hope we venture to join very warmly. As our leading physicist, as Secretary of the Royal Society and potential President, as one who has so closely studied solar physics and the methods of attack contemplated—on each and all of these accounts it is obviously for the advantage of science that the Expedition should be under his command. The Committee has communicated by telegram with Prof. Peirce, expressing a hope that the Expedition may be strengthened by the addition of some American observers who are all veterans in eclipse matters. Prof. Respighi has also been invited to accompany the party. We must not omit to add that Lord Lindsay has supplied the Committee with many valuable instruments, and is aiding in other ways.

DR. CARPENTER arrived at Malta in the *Shearwater* last week, and has been engaged in conjunction with Captain Nares, commanding that vessel, in a series of researches on the Gibraltar current, intended to complete the inquiries made last year in the *Porcupine*. The existence of the outward undercurrent, which was indicated by the experiments of last autumn, has now been conclusively demonstrated. Dr. Carpenter will accompany the *Shearwater* to Egypt for the purpose of prosecuting in the eastern basin of the Mediterranean the physical and zoological inquiries which he carried on last year in the western basin.

MR. HIND informs us that on Friday night last he secured observations of an extremely faint nebulosity, which he believes will prove to be Encke's Comet, though the predicted time of perihelion may be eight or nine hours too late.

September 22 at $11^h\ 37^m\ 21^s$ Twickenham M.T.
☌ R.A. . . . . . $1^h\ 58^m\ 45^s{\cdot}9$
☌ Decl. . . . . . $+31°\ 22'\ 1''$

This position depends on comparison with one of Bessel's stars extending over more than an hour, during which motion in the right direction for Encke was apparent. Though very faint, it is hardly more so than Mr. Hind expected to find it from previous experience. We may look for a fine telescopic object in November, and one just visible to the naked eye after the middle of the month and in the first days of December, before it gets too near the sun's place to be observable.

THE College of Physical Science at Newcastle-on-Tyne has issued the prospectus of its first session, to open October 7. The following is the programme of studies:—Prof. Aldis will conduct three classes in Mathematics; two for junior students, and one for any who may enter with sufficient knowledge to enable them at once to take up subjects which will in future be ordinarily read by students in their second year. There will also be an Exercise class for all the students. In Experimental Physics, Prof. Herschel will have two classes; the advanced class will contain experimental illustrations of Practical Mechanics, and of Heat considered especially with reference to its application in mining, manufactures, and to the steam-engine; but there appears to be no provision for practical laboratory work. In Chemistry, Prof. Freire-Marreco will conduct both the course of lectures and the practical course in the laboratory. Prof. Page will deliver a course of lectures on Geology, and will accompany the students in field excursions or visits to museums. As yet there is no immediate prospect of a chair of Biology, though we understand its creation is a settled thing as soon as funds allow. The medical degrees at Durham University can now be taken without residence, but an additional year must in that case be made at Newcastle. The College has taken off the hands of the College of Medicine the new lecture room, built expressly for Chemistry and Physics, and the laboratories.

THE Chargé d'Affaires of the Japanese Government in this country, who has looked with longing eyes on Messrs. Cooke's great equatorial, the gem of the present International Exhibition, should also inspect some meteorological instruments for transmission to Japan. The following extract from a little illustrated news-sheet now being hawked about Yedo, giving an account of the late typhoon in the inland sea, and a picture of the appearance of Kobe Bay after it, will show that the Japanese have as yet quite elementary notions in meteorological science:—"The Great Storm-Wave in Kishiu, Idzumi and Setsu.—The sudden changes and movements of heaven and earth are caused by the commingling of the female and male elements, and the contention of wind and rain. Alas! not even can the influence of the gods of Buddah prevail to govern them. It was on the night of the 18th day of the fifth month of the fourth (goat) year of Meiji, and about ten o'clock, that the wind and rain became exceedingly violent, and a great storm-wave arising, not only the steamers, but also about 700,000 large and small boats were thrown ashore at Kinohana and Kumanoura in Kishiu, at Sakai, and at Tempozan off Osaka. At the frightful destruction, old and young, males and females, wept and howled; and the sound thereof was most pitiable. The number of the dead was in proportion to the size of the places (visited by the storm). It was a wonderful event, not heard of in former generations. On the following morning the rain ceased, and the mad wind became quiet, and then for the first time men felt at ease in their heart."

WE have to record the death, in his seventy-ninth year, of the Rev. William Hincks, F.L.S., for many years Professor of Natural History and Director of the Museum in University College, Toronto, and previously Professor of Natural History in the Queen's College, Cork. Mr. Hincks, who had but just resigned his professorship owing to the infirmities of age, was an accomplished and enthusiastic botanist, and had also devoted much attention to certain departments of zoology. He possessed a wide range of scientific knowledge, and through a long life had done much for the diffusion of scientific tastes and culture. Almost to the very close of life he was an enthusiastic student, actively engaged in pursuing his favourite researches, and alive to all that was passing in the scientific world. He published many papers on natural history and other subjects, some of which were specially devoted to the Fauna and Flora of Canada, chiefly in the "Journal of the Canadian Institute." To the Museum connected with University College, of which he was the director, he devoted much time and labour, and rendered it very valuable service. He was an active member of the Canadian Institute, and one of the Editing Committee, which is charged with the publication of the "Journal." In 1869 he was elected president, and was re-elected the following year.

MR. SAMUEL SOLLY, F.R.S., expired suddenly on Sunday last. Mr. Solly was deservedly well known from his numerous contributions to the advancement of science, especially by his work on the "Human Brain," "Surgical Experiences," an "Analysis of Müller on the Glands," and by his various papers and lectures on surgery in the medical journals.

A GERMAN translation of Mr. E. B. Tylor's "Primitive Culture" is in progress. Dr. Spengel is the translator, and the publishers Winter and Co. of Leipzig.

OUR old friend *Cosmos*, the publication of which has been suspended since September last, reappeared on the 10th inst. in a new form, under the title of *La France Scientifique*, still edited by M. Victor Meunier. The new journal takes for its motto, "Régénérer la France par la Science, et la Science par la Liberté." The three numbers already received contain articles original and selected on science and education.

*Les Mondes* for September 23rd contains an account of an invention by M. Corbin, a sugar manufacturer of Lizy-sur-Ourcq, of a portable railway, which can be laid down daily in any position, and can be used for facilitating agricultural operations, causing a great reduction in the amount of labour required. The invention could evidently be applied only in the case of farms consisting of very large fields, as occurs in some parts of the East of England

THE Zoological Society of London have just received a fine living example of a species of Cassowary new to their collection. It is a young bird, but is probably referable to the *Casuarius uniappendiculatus*, described some years ago by Mr. Blyth from a specimen observed alive in Calcutta, although there are at present no traces of the single throat-wattle, from which the species obtained its name. In general appearance the new acquisition resembles the Mooruk or Bennett's Cassowary (*C. Bennettii*) rather than the Common Cassowary of Ceram (*C. galeatus*). It is said to have been captured on the coast of New Guinea, near the Bay of Geelvink, and was, we believe, obtained by the Zoological Society from one of the sister institutions of the Continent.

IN reference to our article last week on the Smithsonian Institution, we hear that quite recently the learned societies and public libraries of Holland have undertaken to co-operate with the Institution in this enterprise, by forming a Central Scientific Bureau of the Netherlands, at which the packages intended for transmission to America are to be collected, and forwarded from time to time to the Smithsonian Institution, which will distribute them to the parties addressed. The Bureau also proposes to establish special agencies in different parts of Europe, and has already announced the firm of MM. J. B. Baillière and Son, of Paris, as the agents for France, to whom all French institutions are requested to address such copies of their works as may be intended for the Netherlands.

WHEN ocean cables were first submerged, various apprehensions of probable injury were entertained, some of which have proved to be well founded, and others less so. It was supposed that worms or mollusks would burrow in the substance of the envelope, and ultimately penetrate to the centre of the wires; or, again, that the attachment of barnacles, mollusks, or other marine animals on the exterior would invite the attacks of the sharks, rays, and other fish of powerful jaws, and induce them to subject the bunch of matter to such a mastication as should produce serious harm to the cable. To what extent any accidents have happened from this source it is perhaps difficult to say; but we now learn from *Harper's Weekly* that the Florida cable between Punta Rosa and Key West has been injured in numerous places, as supposed by sea-turtles biting through or crushing it in their teeth, to such an extent as to destroy its continuity. It is, perhaps, a question whether the turtle be chargeable with these operations; and we think it is quite as probable that, under the circumstances, some ray or other fish has attacked it, and for the reasons already suggested.

A CORRESPONDENT requests us to state that the valuable specimens of *Stagonolepis Robertsoni* and other reptilian remains from the upper Elgin sandstones, which Prof. Huxley has lately examined, are to be found in the Elgin Museum, and not in that of Dundee, as mentioned in our last week's "Notes."

WITH reference to the earthquake recorded in our last number as having occurred in Chile and Peru in June last, a correspondent informs us, that being at that time in Madeira, a perceptible shock was felt there on the 20th about 6 P.M.

M. STROUMBO, a Professor in the University of Athens, has suggested the substitution for some scientific terms in ordinary use of others etymologically more correct. He proposes in particular saccharometer for saccharimeter, eidoloscope for kaleidoscope, rheumatometer for rheometer, rheumatostat for rheostat, apochrose for achromatism, phasmoscope for spectroscope.

AMONG recently started magazines deserving a word of commendation is *The Traveller*, a monthly international journal for England and America, devoted to international topics, real estate and agriculture, and to universal travel. It contains original articles by well-known writers on the various subjects included under the above headings, some of which are illustrated, reviews, notes and queries, correspondence, &c., of a character calculated to interest a variety of readers; and the price at which it is published brings it within the reach of all.

PART II. vol. ii. of the "Transactions of the Entomological Society of New South Wales" is occupied by the first portion of a description, by Mr. Macleay, of a collection of over 1,100 Coleoptera, brought from Gayndah a town on the Burnett River in that colony. Many of both genera and species are new. In this paper Mr. Macleay makes an innovation which he thus refers to in his introductory remarks:—"I have always hitherto, in describing new genera and species, adopted the system most usual with English entomologists of giving these descriptions in Latin. On this occasion I intend to depart from that rule, as I believe that many of those who take an interest in Australian entomology will infinitely prefer the descriptions given in plain and intelligible English."

THREE important papers are reprinted by Mr. V. Ball, from the *Journal of the Asiatic Society of Bengal*—"Notes on the Geology of the Vicinity of Port Blair, Andaman Islands;" "Notes on Birds observed in the neighbourhood of Port Blair during the month of August, 1869;" and "Brief Notes on the Geology and on the Fauna in the neighbourhood of Naucowry Harbour, Nicobar Island."

THERE has been issued, under the auspices of the Accident Insurance Company, an admirable little manual of instruction for the prompt treatment of accidents and emergencies, by Mr Alfred Smee, the eminent surgeon. It is clear, comprehensive, and portable, and the reader is guided in the more important curative processes to which it relates by well-executed and instructive woodcut illustrations.

"HUMAN Locomotion, how We Stand, Walk, and Run," is the title of a lecture delivered last December at Cornell University, by Prof. B. G. Wilder. Dr. Wilder's lecture was profusely illustrated by diagrams and interesting practical experiments. Among other matters he noticed the curious fact that a person never goes in a perfectly straight line for any distance, but always turns to one side or the other, and at last describes a circle and returns to the point from where he started. The deflection is generally if not always from right to left, and is accounted for on the principle that one side of the body tends to outwalk the other. It is a received opinion among American hunters and woodmen that people who lose themselves in forests or extensive plains thus travel in a circle turning to the left.

WE have received a pamphlet on the Economical Production of Peat and Peat Charcoal, as carried on at the works of the Peat Engineering Company, Redmoss, near Bolton, Lancashire. The peat is extracted from the bog, macerated, and moulded by machinery. It is also transformed into a superior quality of charcoal. That the manufacture is a profitable one is apparent from the fact that an acre of peat bog of the average depth of ten feet, will yield sufficient to make a thousand tons of charcoal, which, in competition with wood charcoal, can be sold at such a profit as would alone produce the value of the land from which it is extracted. It is stated that in 1852 the actual annual consumption of raw and carbonised peat in France amounted to 359,319 tons, a consumption which has since largely increased.

THE *Révue Universelle* says that the German Confederation, in acquiring an extended frontier from France, has traced it, not upon a topographical plan, but, in all probability, on a geological map edited at Berlin. In fact, it is to be observed that the new boundaries between France and Germany absorb, for the benefit of the Confederation, all the rich deposits of the mines of oolitic iron in the basins of the Moselle and the Meurthe, with the exception of the Longwy group. Save this, which has been reserved, Germany has made herself mistress of the major portion of the best part of the most important mineral beds in France. These beds extend under the vast plateau which forms the east of the departments of Moselle and Meurthe, and crop out in the valleys from Longwy, in the north, as far as Pont-Saint-Vincent (Meurthe), in the south, and comprise a full quarter of the mineral riches of France. The new determination of frontier will have the effect of introducing into the productive industry of Germany, according to the statistics of 1867, "twenty-three blast furnaces, producing 205,000 tons of metal; 9,000 hectares of iron country, yielding 500,000 tons of ore; fourteen works manufacturing 127,000 tons of iron; and 22,000 hectares of coalfield concessions, yielding 180,000 tons of coal."

THE Maharajah of Bhurtpore has established workshops in which steam is the motive power for the industrial instruction of his people.

## ON THE STUDY OF SCIENCE IN SCHOOLS *

BEFORE we commence our regular and systematic study of science, I wish to say a few words to all of you who will hereafter take part in these studies, concerning the nature and character of experimental science, and certain matters connected with the pursuit of it. It will be well to discuss these subjects under the following headings:—

Firstly. The rise and growth of the sciences we are about to study, and their distinguishing features.

Secondly. The objects and aims of the experimental sciences, and the reasons why we study them.

Thirdly. The methods we shall follow for the acquirement of a knowledge of science.

Fourthly. The attitude of mind most favourable to such studies.

As to the first of these divisions, I may mention that the boundary lines of the experimental sciences are very clearly defined. For we find at the extreme limit in one direction the mathematical sciences, mathematical astronomy, mathematical mechanics, and so on; and at the other extremity the classificatory sciences: zoology, botany, and so on. Our course lies between the two limits, where we find the physical sciences proper: statics, dynamics, mechanics, hydrostatics, hydrodynamics, pneumatics, acoustics, heat, light, magnetism, electricity. Chemistry is usually distinguished from these, both on account of the magnitude of the science, which necessitates separate and distinct treatises, and because it concerns the intimate structure or composition of matter, while the physical sciences proper are concerned with unaltered matter. But the term experimental sciences includes both the physical sciences and chemistry, and is hence the most convenient for our purpose.

Most of the physical sciences partake somewhat of the character of the mathematical sciences, while chemistry is on the verge of the mathematical sciences. The physical sciences relate rather to dead matter, to inorganic, unorganised matter, while the classificatory sciences relate to organised living matter: the former to the mineral kingdom (as it used to be called), the latter to the animal and vegetable kingdoms.

Although isolated facts belonging to many of the sciences were known to the ancients, no science can be said to have existed in anything like a complete form for more than 200 years, and several of them are less than a century old. The science of *Statics* treats of the balance of forces, of the relation of the various forces which act upon solid matter at rest. The derivation of the name from ἵστημι is sufficiently obvious. The science commenced with Archimedes (who lived in the third century B.C.), and was greatly developed by Galileo, Bernouilli, and Lagrange. When the equilibrium of fluids is discussed, it is called *Hydrostatics* (from ὕδωρ), and the equilibrium of gases is described under the head of *Pneumatics* (πνεῦμα). Hydrostatics owes its origin to Archimedes; you will remember the story of his weighing the crown of impure gold in water, and detecting the imposture; and thus arose that which to this day is called the "law of Archimedes," which asserts that when a body is immersed in a liquid, it loses a portion of its weight equal to the weight of the liquid which it displaces. Stevinus of Bruges, who wrote in the sixteenth century, and Pascal contributed much to the advancement of this science. The reverse of rest is motion, thus there are sciences relating to the motion of solids, liquids, and gases. *Dynamics* (δύναμις) treats of the motion of solid bodies, and of the relation of the forces which produce motion. It originated as a science with Leonardo da Vinci, who, besides being the greatest painter of his day, was an eminent mathematician, engineer, musician, and natural philosopher. He showed that if two forces are represented in magnitude and direction by the two sides of a parallelogram, the resultant is represented by the diagonal of the parallelogram. This is the important principle of the "parallelogram of forces." Galileo added the laws regarding falling bodies; while Newton and Huyghens investigate the laws which regulate centrifugal forces. *Hydrodynamics* treats of the motion of fluids, and bears the same relation to dynamics that hydrostatics bears to statics. The motion of gases is discussed under the science of pneumatics; we have no sciences of pneumastatics and pneumadynamics. Pneumatics dates from the discovery of Torricelli in 1642 that the air possesses weight. Eight years later, Otto von Guericke, of Magdeburg, invented the air-pump, and the science was then developed with great

* A Lecture delivered at Marlborough College as an introduction to the commencement of Science teaching, by G. F. Rodwell.

rapidity. Before the end of the century various treatises on pneumatics had appeared, and perhaps no science so speedily reached maturity. The above sciences, it will be noted, relate to the properties of matter in its three forms of solid, liquid, and gas, when at rest and in motion. We come next to certain sciences which treat of the more subtle and intimate motion of the particles or molecules of matter, with various velocities and in various directions. Beginning with *Acoustics*, we have the vibratory motion of particles across a position of rest resulting in the production of what we call sound. The science of sound, although more or less linked with the art of music, has existed as an experimental science for less than a century. Vibratory movements of the same character taking place in a subtle kind of matter called the ether or interstellar medium, constitute *Heat* and *Light*, the difference being one of velocity, and thus of degree rather than of kind. Finally, we may assume that *Magnetism* and *Electricity* are conditions of matter perhaps not differing much from those which constitute light and heat.

The science of *Light* is certainly one of the older of the sciences. Euclid endeavoured to explain the laws of vision; Ptolemy, the astronomer, wrote a treatise on Light; the reflection of light by mirrors, and its refraction by lenses, were well known facts in the time of Archimedes. Various treatises on the subject appeared during the Middle Ages. The *Ars Magna lucis et umbræ* of Athanasius Kircher, published in the seventeenth century, is a great folio, full of plates. Not long after its publication Newton made the important discovery of the decomposition of light, and treated various optical problems with great precision by mathematical means. Our term *light* is related to the Sanskrit *lok*, to see. Heat has not existed as an experimental science for a century. The science has made great progress during the last thirty years. Heat was once believed to be an entity, a kind of matter, which passed from one substance to another, and which effected certain changes during its transference. We now know that it is simply a kind of motion akin to that which constitutes light, so that it ceases to be matter, and becomes an attribute of matter. It is strange that the term *heat* should be far more appropriate now than it was when heat was regarded as matter, although it was in use long before any theory or science of heat existed. The term appears to be derived from the Sanskrit *indh*, to kindle, through the Greek αἴθω, the Latin *æstus*, and the old High German *eit*. "*Æstus*," says Vossius," est commotio vel in aqua, vel in igni, vel in animo, omnis autem commotio fervorem gignit." And the result of modern research has been to prove that what we call heat is, indeed, due to a commotion of particles of matter. Certain properties of heat were well known to the ancients, although the science itself is so young. Thus, Pliny states that the sacred fire of Vesta was kindled by reflecting the rays of the sun by mirrors. The story of Archimedes and the Roman fleet is well known to you. Lenses were known and were used as burning glasses. Aristophanes clearly alludes to the use of a glass lens for obtaining fire; a lens was found among the ruins of Nineveh, and is now in the British Museum. Lactantius states that fire may be kindled by passing the rays of the sun through a glass globe filled with water.

Magnetism has existed for about 270 years as an experimental science. A few magnetic experiments are mentioned by Lucretius, and by Pliny, and one or two Middle Age writers allude to the effects. Of course the mariner's compass, which was known in Europe in the twelfth century, called attention to the existence of the so-called magnetic force. The birth of the science dates from the publication by Gilbert of Colchester of a treatise entitled "De Magnete," in 1600.

Thales, of Miletus, observed that amber when rubbed acquired the property of attracting light substances, and as the Greek for amber is ἤλεκτρον, and the effect had not been observed in other substances, a new science arose called Electricity; but the science has scarcely existed for more than 200 years. The inventor of the air-pump, Otto von Guericke, was also the inventor of the electrical machine. Thus Pneumatics and Electricity were called into existence at almost the same time. Note how essential the invention of apparatus has been to the different sciences. Until experiments could be tried, and until instruments were devised for trying them, the natural sciences made no progress. Voltaic Electricity, or Galvanism, dates from the commencement of this century, and electro-magnetism and dia-magnetism are yet later developments.

We learn from the above remarks that, although some of the fundamental facts of various sciences were known to the ancients, they never developed them. In fact, there was no experimental science among the ancients, they by chance lighted upon a few solitary facts, and with these they were well content. There could be no experimental science among them, for the fundamental feature of this kind of knowledge is, that it depends upon the action of the mind upon matter, while the ancients preferred to exercise their intellects upon things not external to themselves. Physical philosophy is distinguished from mental philosophy by the fact that the former is based upon observed results obtained by the action of the mind aided by experiment, upon external matter, while the latter is based upon the actions of the mind upon itself according to definite laws instituted by the unaided intellect. The ancients elaborated the most admirable systems of mental philosophy, but they refused to have anything to say to experimental philosophy. We may take the following remarks of Seneca as to some extent an exemplification of the spirit in which the ancients regarded Natural Philosophy:—"The astronomer tells me of Saturn and Mars in opposition, but I say, let them be as they will, their courses and their positions are ordered them by an unchangeable decree of fate. Either they produce and point out the effects of all things, or else they signify them. If the former, what are we the better for the knowledge of that which must of necessity come to pass? If the latter, what does it avail us to foresee what we cannot avoid? So that whether we know or not know, the event will still be the same;" as if he said in the language of more modern science, "I am assured that the specific gravity of iron is somewhat more than that of manganese, and somewhat more than that of copper, but I know they are immutable, and it hence matters not how they differ." Or again, "I am told that there are iron and sodium in the sun, but I can never be there to verify it, therefore it cannot concern me." The ancients were content with the truths which they possessed, and cared not to seek for the discovery of new truths. Thus, as I before said, they possessed no system of experimental science.

You will perhaps ask me why physical truths cannot be discovered by means of the unaided intellect. Why is experiment necessary? We must remember that our senses, although infinitely more perfect than our most delicate and refined scientific instruments, are limited in their capabilities. They are devoted to the service of our organisms, and exist for the purpose of enabling us to fulfil all the conditions requisite for the maintenance of life, and to make us cognisant of the external actions of the material world. But this latter function they exercise only to the necessary extent. There are numberless phenomena beyond the direct cognisance of the senses; there is, if I may so express it, light which is unseen by the eye, sound which is unheard by the ear, heat which is unfelt by the nerves of touch. I mean there are physical actions of the same nature as those which constitute light, sound, and heat, which we cannot directly recognise. It then becomes necessary to call in the aid of experiment and of various instruments to assist and exalt the action of the senses. We have a familiar example of this in the microscope. A speck which the unaided eye recognises with difficulty, is seen by exalting the capabilities of the eye in one particular direction to be a perfectly organised being, possessing many of the functions of creatures far higher in the scale of animal life. One of the Infusoria measures about the twenty-two thousandth of an inch in diameter, and can only be seen by the aid of a powerful microscope, yet it is a perfectly-organised creature. So also, when we wish to examine the various properties of matter, it is absolutely necessary for us to aid the intellect and the senses by means of instruments and experiments. The properties of matter were utterly unknown to the ancients, because they relied upon the unaided intellect, and disdained experiment. Numberless effects in nature reveal themselves only when an unnatural and forced condition is imposed upon matter. "Occulta Naturæ," says Francis Bacon, "magis se produnt per vexationes artium quam cum cursu suo meant."

Although many observers existed before the seventeenth century, there were but few experimenters. Observation, experiment, and reasoning, must go hand in hand, before experimental science can progress. We first find this combination in a very marked degree in Galileo, a professor in the University of Pisa, who was born in 1564, and wrote in the early part of the next century. He invented the telescope and thermometer; demonstrated the theory of Copernicus, which asserted that the sun is the centre of our system, and that the earth moves round it; discovered the satellites of Jupiter and the spots on the surface of the sun, and, in a word, made the first real progress in many of the sciences. Galileo is often called the "Father of the Experimental Sciences;" it is certain that he was the first experimental philosopher worthy of the name. The science work of the seventeenth century was altogether prodigious; at no time has so much been effected; indeed the greater number of the sciences sprang into existence at this time.

Science was greatly promoted by the establishment of Scientific Societies about the middle of the century. Literary Societies had existed in Italy long previously; these consisted of a number of members who met together at stated intervals for the discussion of literary matters, the recitation of poetry, and the reading of essays. The names of some of these societies were sufficiently curious; thus we find, among others, the following:—the Grieved, the Fiery, the Dispirited, the Solitary, the Rough, the Unripe. Baptista Porta founded the first scientific society in 1560, and called it the "Academy of the Secrets of Nature;" but on account of the privacy of the meetings, and the prevalence of occult and forbidden arts at this time, it came to be believed that the members used magical and diabolical influences, and the society was dissolved by the Pope. Shortly afterwards Porta published his "Natural Magic," in which he endeavours to prove that the magic of Nature is as wonderful as the magic of Art; in a word, that we find in the phenomena of Nature results quite as wonderful as those produced by professed sorcerers. After the dissolution of Porta's Academy, we find no scientific society until the formation of the Academy of Cimento in Florence, in 1567. This Society was not founded for the discussion of theoretical, or even simple observational science: "our sole design is to make experiments and to relate them," says the secretary at the commencement of the proceedings. Consequently, although the Society flourished for no more than ten years, a volume of "Experiments made in the Academy of Cimento" appeared in 1667, and from its importance it was speedily translated into Latin, and into most of the languages of Europe. It contains a number of experiments relating chiefly to pneumatics and heat.

About the year 1658, a few Oxford men, interested in science, agreed to meet in each other's rooms once a week for the trial of experiments, and for the discussion of scientific matters. The number of members increased, and after a while the meetings were removed to London, and were held in Gresham College. Soon afterwards the society was incorporated by Charles the Second, under the name of the "Royal Society for Promoting Natural Knowledge." Note the significance of the term *Natural* as here employed. There was so much unnatural science in the world, so much magic, witchcraft, false knowledge, that the society thought it well to specify "Natural Knowledge." We find traces of the magical lore of the age in the accounts of early meetings of the Society; thus we find in the minute-book of the Society the following entries under the year 1660:—

"June 5th. His Grace the Duke of Buckingham promised to bring into the Society a piece of an unicorne's horn.

"July 14th. A circle was made with powder of unicorne's horn, and a spider set in the middle of it, but it immediately ran out several times repeated. The spider once made some stay upon the powder.

"June 26th. Dr. Ent, Dr. Clarke, Dr. Goddard, and Dr. Whistler, were appointed curators of the proposition to torment a man presently with the sympathetical powder.

"June 10th. The fresh hazell sticks were produced, wherewith the divining experiment was tried, and found wanting."

This Society continues to meet weekly, and in its Transactions may be found all the most important scientific memoirs which appear in this country. The Académie des Sciences was founded in Paris a few years after the Royal Society of London.

The influence of scientific societies on the influence of experimental science has been, and still is, very considerable. Towards the end of the seventeenth century they were very generally dispersed throughout Europe, and experimental results accumulated at a rapid rate. They were embodied in text books, and were soon introduced into the Continental universities, and thus became incorporated with general learning. No place in the world has taken so prominent a part in the furtherance of experimental science as the University of Leyden. Its professors during the seventeenth century were renowned throughout Europe, and students flocked from every part of the Continent to the University. Again, it is a noteworthy fact that the first text book of physical science, and the first text book of chemistry, both issued from this university:—the *Physices Elementa Mathematica* of S'Gravesande, and the *Elementa Chemiæ* of Boerhaave. They each consist of two well-illustrated quarto volumes, and were published during the first half of the last century. The greater number of the sciences are made up of the discoveries of the last two centuries, and these will come under our notice when we study the special science itself. I may, therefore, safely leave our brief survey at this point.

(*To be continued.*)

## *SCIENTIFIC SERIALS*

THE *Revue Scientifique* Nos. 8—12 has been to a large extent occupied by a report of the most important papers read at the recent meeting of the British Association; but we find in addition the following valuable articles:—A report of the very important course of lectures delivered by M. Claude Bernard at the Collége de France "On the Action of Heat on Animals;" report of a course of lectures by M. Gréhant "On the Renewal of the Air in the Lungs," largely illustrated by woodcuts; a paper by M. Onimus on *Les nerfs trophiques;* and a number of other papers chiefly bearing on physiological subjects, either translated from the English, or extracted from the proceedings of learned societies. Copious extracts from Mr. Darwin's work "On the Descent of Man" are also translated from time to time.

*Der Naturforscher*, Nos. 31—34, August 1871. This journal is entirely made up of articles and abstracts from German, French, English, and Italian serials. Some of the latter are especially interesting to us, as being less known in this country. In the first number we find some researches by Prof. Nobbe of Tharand on the function of potassium salts in the nutrition of plants. The experiments were made on buck-wheat and rye; they led to the conclusion that potassium is quite indispensable to the assimilation of plants; without it no starch is formed in the chlorophyll-granules, and the weight of the plant remains constant, exactly as in pure water. Neither sodium nor lithium can replace potassium, the lithium being positively pernicious. An article giving the results of the second German Arctic Expedition describes the climate of East Greenland, where the ground appears to be for three months free from snow, and covered with abundant herbage, fed upon by the reindeer and the musk-ox. The latter was not before known to inhabit this region. From an account of the water supply and soil of the town of Zurich, we learn that in the cholera epidemics of 1855 and 1867, no confirmation could be found of Pettenkofer's theory with respect to the connection of cholera and "Grundwasser." Prof. Nöbius of Kiel, discusses the nutrition of deep-sea animals, especially in relation to the organic "slime," which he believes to be chiefly of vegetable origin. Prof. Karsten related to the Austrian Pharmaceutical Conference in Vienna his personal experience of the poisonous properties of the famous manchineel tree (*Hippomane manzanilla*) of the West Indies and tropical America, which have been doubted by some naturalists. Being engaged for some hours in collecting its juice, Karsten was attacked with burning sensations of the skin, swelling of the face, eyes, &c., which compelled him to pass three days in total darkness. He attributes these effects to a volatile poison given off by the tree. Other papers are: Nyland "On the Phenomena of Discharge of Induced Currents of Electricity;" Fritsch "On the Geological History of the Santorin Group;" Meunier "On the Cosmical Relations of Meteorites, and the Black Colouring Matter of the Meteorite of Tadjera;" Secchi "On the Solar Protuberances;" Young "On the Corona;" Klocke "On the Growth of Crystals," &c. The following papers on physics are from journals little read in England:—"The Heat given off by Incandescent Platinum," by Prof. Garibaldi of Genoa. He used, as Tyndall in his experiments on the electric light, a thermopile, but let the rays pass through a dry vacuum tube closed with thin plates of rock-salt, and absorbed the light rays by a solution of iodine in carbon disulphide. In this way the errors arising from the passage of the rays through a moist atmosphere and through prisms and lenses of rock-salt are avoided. He finds the ratio of visible and invisible rays given off from white hot platinum 1 : 25; but there was still some loss of the dark rays. (Il nuovo Cimento; ser. 2; tom. iii.) In another research, Garibaldi has investigated the power of absorption for heat of the constituents of the atmosphere. The source of heat was heated platinum, and radiation took place through a closed vacuum, thus avoiding some of the errors of other experimenters. The power of absorption possessed by aqueous vapour was found to to be 7,937 times that of dry air. A valuable paper by Kundt ("Würzburger Verhandlungen." Neue Folge, vol. iii.), discusses the anomalous dispersive power for particular parts of the spectrum possessed by certain coloured substances as hæmatin, chlorophyll, sandal wood, litmus, &c.

## SOCIETIES AND ACADEMIES

### Bristol

**Observing Astronomical Society.**—"Report of Observations to August 31." *Solar Phenomena.*—Mr. T. W. Backhouse, of Sunderland, reports as follows:—"A fine group of spots passed the sun's centre in the southern hemisphere on the 17th August. I made the following measurements of its chief spot:—

| Date | H. | M. | Umbra Length | Penumbra Length | Penumbra Width |
|---|---|---|---|---|---|
| August 11 | 21 | 20 | —— | 82,000 | 46,000 |
| ,, 13 | 21 | 20 | —— | 71,000 | abt. 18,000 |
| ,, 14 | 3 | 30 | 14,500 | —— | —— |
| ,, 14 | 20 | 0 | 16,500 | 66,000 | —— |
| ,, 15 | 21 | 15 | 16,500 | 65,000 | 34,000 |
| ,, 18 | 3 | 30 | 9,500 | 59,000 | 39,000 |
| ,, 20 | 21 | 0 | —— | 75,000 | —— |
| ,, 21 | 21 | 20 | smallish | divided into four | —— |

The penumbra had a more ragged appearance than is often the case. If this group has returned to this side of the sun it contains no important spots this month. It generally contained two or three large penumbræ, of which I made several measurements, and on the 18th they were united at 3h 30m, making a penumbra 78,500 miles long and 41,000 wide at its widest part, and at 21h 10m 84,000 miles long. By the 25th all its spots were so reduced as to be quite small."

*The Moon.*—Mr. Albert P. Holden, of London, writes:—"Shortly before last quarter of the moon in August I observed the unilluminated portion unusually bright with earthshine. A few prominent craters could be traced, whilst the whole of the dark outlines of the *Mare Serenitatis* were easily recognised. The darkest object was the *Mare Crisium*, which appeared almost black, and very considerably darker than any other of the great plains. It does not seem improbable that the depth of colour seen in the *Mare Crisium* and other planes may be due to a covering of alluvial earth, to which vegetation may at times give the greenish tinge occasionally observable.

*August Meteors.*—These phenomena were observed by the Rev. S. J. Johnson, at Crediton, and Mr. William F. Denning, at Bristol. On August 10 Mr. Johnson witnessed the appearance of shooting stars at the rate of twenty-six per hour. Mr. Denning maintained a watch during three evenings, and the average number seen per hour was as follows: Aug 9, 18; 10, 28; 11, 46. The maximum number was seen on the latter date. He observed 260 meteors altogether during the above dates, and the sky was under observation for a period of $8\frac{3}{4}$ hours. The most brilliant meteors were observed at 12h 23m on Aug. 10, and at 10h 44m and 12h 50m on Aug. 11. Very nearly all the meteors observed radiated from the small star $\beta$ in Camelopardalus. Nearly all of them were accompanied by trains, which became extinct immediately after the disappearance of the meteors themselves.

*The Nebula in the Pleiades in Taurus.*—Mr. Albert P. Holden has again endeavoured to pick up this object with his 3-inch refractor, but without success. "With good eye-sight and a clear atmosphere I have failed to find the slightest trace of the nebula on all occasions. I have no hesitation, therefore, in saying that in instruments of 3-inch aperture and under, the object is utterly invisible. I beg some member of the society to search for this object with larger instruments, so that the question as to its actual disappearance may be beyond dispute. It is important that this question should be set at rest at once, because in the event of the nebula brightening we should certainly regret not having decisively established the fact of its disappearance.

### Dublin

**Royal Irish Academy,** June 12.—The Rev. President Jellett in the chair. Profs. Sullivan and O'Reilly read "Notes on the Great Dolomite Bed of the North of Spain in connection with the Tithonic Stage of Prof. Opel." (This paper was erroneously referred to as read on the 22nd of May, vide *ante*, p. 136, where for "Opal" read "Prof. Opel.")—Dr. Sigerson read some additions to the "Flora of Botanical District No. 10 (Ireland)," and on an anomalous form of the Corolla of Erica.—Bryan O'Looney read "Notes on Lebor na h-Uidhri," and G. J. Stoney and J. E. Reynolds read a paper on the "Absorption Spectrum of Chlorochromic acid." The following were elected members:—W. A. T. Amhurst, D.L. Norfolk, Captain R. Cooper, Rugby, Whitley Stokes, Calcutta, and Colonel Tyrrell, J.P., Londonderry.

June 26.—Rev. President Jellett in the chair. Dr. Sigerson read "Note on the Remains of Fish in the Alluvial Clay of the River Foyle."—Rev. Dr. Reeves read a paper on the "Topography of the County of Armagh."—Mr. G. J. Stoney, F.R.S., read "Notes on a New Form of Spectroscope."—Mr. W. H. Hennessy read a paper on the "Tale of the Brudin Da Derga contained in the Lebor na h-Uidhri," and Dr. Hayden read "Notes on the Respiration of Compressed Air."

### Paris

**Academie des Sciences,** Sept. 18.—M. Faye in the chair. It was stated that the total amount of money in the hands of M. Janssen for the scientific expedition to Sumatra to observe the solar eclipse in December next will reach to 1,580*l*.—Several gentlemen sent letters describing the earthquake which was felt in Burgundy on the 9th of September, 7.45 A.M. At Tranant a number of fences which were piled together were overthrown in a straight line, making a deviation of 27° W. from the magnetic needle.—M. Combary, director of the Constantinople observatory, sent a note to describe the extraordinary cold felt in last May. In Yorkshire it was felt on the 12 h, at Paris on the 15th, at Constantinople on the 18th. The perturbation, which lasted for some days, was felt also in Arabia, where the torrid deserts were affected by cold.—M. Leverrier read a letter from Barceloneta describing the observations, which were made with more care than anywhere else in France, on the falling stars of the November display of 1869 and 1870.—Several communications were made relating to analogies exhibited by spectra of different substances belonging to the same family of chemical substances.—M. Delaunay read a note on the discovery of a new planet observed in the Marseilles Observatory by M. Borelly on the 12th of September, 1871. It is the 116th, and is to be called *Lomia*. M. Borelly had discovered already the 91st, 99th, and 110th, and has given to them respectively the following names: Egine, Dike, and Lydia.—Communications relating to the cholera were three in number, and were all sent to the committee for the Bréaut prize, which is a sum of 4,000*l*.—A table placed before the chair was covered with samples of rocks extracted from Mont Cenis Tunnel, and arranged in a systematic collection, which will be exhibited in the museum of the School of Mines. M. Elie de Beaumont, the perpetual secretary, read a very long paper on the instruction conveyed by this collection, the most important portions of which will be found reported in another column.

## BOOKS RECEIVED

English.—Experimental Mechanics R. S. Ball (Macmillan and Co.).—The Lichen Flora of Great Britain: Rev W. A. Leighton (Shrewsbury, printed for the Author).—Miscellanies of John A Symonds, M D: Edited by his Son (Bristol, J. Arrowsmith)—The Soldier's Pocket-book for Field Service: Col. Sir G J. Wolseley (Macmillan and Co).

Foreign.—Archiv für Anthropologie, 4er Band.

## CONTENTS

THURSDAY, OCTOBER 5, 1871

## *OBSERVATIONS UPON MAGNETIC STORMS IN HIGHER LATITUDES*

THE extension of the telegraph into the more northern latitude of the Shetland Islands, between 59° 51′ and 60° 51′ 30″ N., has afforded a much better opportunity of observing the frequency and variation of the magnetic and auroral storms that have of late excited some attention and discussion in these pages.

Some of the earliest recorded observations upon the strength and direction of these atmospheric storms, date from the time when the extension of the telegraphic wires over England rendered the phenomenon visible by the disturbance of the magnetic needle placed in circuit with the wires, and to a certain extent rendered possible the mapping down of the position and direction of the magnetic storm over certain tracts of Great Britain.

On the 24th September, 1847, remarkable magnetic disturbances were observed in London, and the direction and deflection of the magnetic needle noted. The effects of this magnetic storm were carefully observed at Dawlish, Norwich, Derby, Birmingham, Rugby, Cambridge, Tonbridge, Wakefield, Edinburgh, and York. The magnetic disturbance appears to have commenced about $1^h$ $5^m$ P.M. on the 24th, and continued with variable intensity until $7^h$ $30^m$ A.M. on the 25th.

It may be interesting to give some of the galvanometer readings recorded as indicating the rapid oscillation and deflection of the galvanometer needle. In the period of time between $4^h$ $17^m$ P.M., and $5^h$ $48^m$ P.M. on the 24th, or in about one hour and a half, the direction of the current had changed no less than ten times, showing a maximum swing of the needle over an arc of 50°.

| H. M. | deg. | H. M. | deg. |
|---|---|---|---|
| 4.17 | 15 left | 5.5 | 15 left |
| 4.20 | 20 right | 5.11 | 12 „ |
| 4.25 | 1 „ | 5.16 | 10 right |
| $4.25.30^s$ | 18 „ | 5.22 | 18 left |
| 4.35 | 6 „ | 5.25 | 14 right |
| 4.38 | 12 „ | 5.28 | 13 left |
| 4.45 | 20 „ | 5.32 | 20 „ |
| 4.50 | 10 left | 5.34 | 26 „ |
| 4.51 | 17 „ | 5.42 | 29 „ |
| 4.55 | 0 „ | 5.48 | 30 „ |
| 4.56 | 8 right | | |

During this magnetic storm, the variation of the dipping needle which was observed in London every $30^m$, ranged between 69° 30′ and 67° 50′.

In some cases these magnetic storms were so severe as to impede the working of the railway signals. On the 18th of October, 1841, a very intense magnetic disturbance was recorded, and amongst other curious facts mentioned is that of the detention of the 10.5 P.M. express train at Exeter sixteen minutes, as from the magnetic disturbance affecting the needles so powerfully, it was impossible to ascertain if the line was clear at Starcross. The superintendent at Exeter reported the next morning that some one was playing tricks with the instruments, and would not let them work.

It will be fresh in the memory of many of our readers that during the month of October last year, very remarkable and brilliant "auroræ" were observed in London, chiefly of a deep blood-red colour, spreading from the zenith over a great portion of the heavens.

It is, however, in the more northern latitude of the Orkney and Shetland Islands that the grandeur of these wonderful electrical phenomena can be observed, and that reliable data can be obtained from which hereafter some practical result may be deduced.

As observed in Orkney and Shetland, the aurora, as a general rule, appears to concentrate and emerge from behind a dense mass of dark cloud lying low down in the horizon towards the north. The edge of this cloud-bank is serrated and jagged, as if the mass were electrically in a high state of tension. From behind this cloud-bank "dark" streamers will appear to start up high into the zenith, appearing as if attenuated portions of the edge of the cloud-bank had been dragged by some invisible power, these dark auroral rays being at the same time transparent as regards the power of transmitting the light of the stars, which shone through with undiminished splendour. At the same moment that these dark rays are emicant, brilliant green, violet, crimson, and white rays appear to stream upwards towards the zenith, but always with a less persistence of duration. These coloured scintillations change with greater rapidity than the black rays.

During the month of December of last year, some very vivid prismatic tints were observed from the Island of Eday. From careful observation it was then remarked that the red coloured rays appeared generally to be of a partially opaque nature, and it could be readily seen that the light of a star, when viewed through the red scintillation, was dimmed as compared with the brilliancy of the same star when observed through the scintillations of another colour.

In some of these displays, the most vivid and varied colouring was exhibited. These were noted down as visible to the eye at the same time, and as the colours were observed in contrast, the distinctiveness and brilliancy of the tint became the more decided. Black, pale yellow, strong yellow, white, violet, pale blue bright green, crimson shade fading into a reddish pink, pale orange, and a delicate sea-green tint. So far nothing approaching to the indigo hue has been noticed. With this exception, the entire prismatic colours and blending tints may be said to have been perfectly developed in the rapid electrical scintillations of the aurora. The colours fade away and change with astonishing rapidity, and this variation in tint will take place without apparently any great electrical disturbance in the special ray observed, beyond a slight flickering motion. In these regions, where the atmosphere is so perfectly still and at times calm, repeated observation has determined the existence of very appreciable sound to the ear, as an accompanying phenomenon [to the rapid rush of the auroral streams towards the zenith. The intensity of the sound emitted varies considerably. At times, it greatly resembles that of the rushing noise caused by the firing of a rocket into the air when reaching the ear from a distance. At other times it has a strong resemblance to the sound produced by the crackling of burning embers, but wanting in any very distinctive sharpness.

In all these cases of auroral displays the inductive effects upon the telegraph wires are very strongly marked; currents of varying intensity and direction flowing unceasingly through these metallic circuits.

The result of observations made in Shetland during the months of September, October, November, and December last year, tend to show that these auroral disturbances attained their maximum effect upon the wires between $8^h$ $30^m$ and $9^h$ $30^m$ A.M., and between $8^h$ $30^m$ and $10^h$ $30^m$ P.M.; and such is the unstableness of these induced auroral currents, that frequently in five minutes the electromotive force will vary from very much less than that of a Daniell cell to a current of such intensity that a brilliant stream of light will flash across the points of the lightning conductors with sharp detonating reports, the electromotive force of which would be scarcely equalled by 500 Daniell cells.

In January last very curious electrical phenomena were observed at Lerwick through the day-time, in connection with the N.E. gales so prevalent at that period of the year. In Shetland these gales are almost without exception accompanied with very severe hail-storms. The day begins bright and fine, a clear sky, the barometer rapidly rising; low on the horizon may be observed dense and angry-looking clouds. One by one these clouds travel fast towards the zenith, when all at once a fearful gust of wind, accompanied with the most violent hail-storm, will apparently break out of the cloud, and continue for about fifteen minutes. The wind then subsides, and the day appears as fine as before. In half an hour's time a second cloud will have appeared, and there will be a repetition of the temporary tornado and hail-storm. The remarkable circumstance attending these successive storm clouds is that they appear to be a purely electrical phenomenon. The moment that the icy discharge takes place from the cloud with its accompanying "crack" of wind, an induced electrical current appears upon the wire, so strong that it attracts firmly down the armatures of the telegraph Morse apparatus. The moment, however, that the hail ceases, the current passes off, but with this result, that each successive cloud storm appears to induce a current flowing in an opposite direction from the last, that is to say, the currents appear to be (using conventional language) positive and negative in their effects.

That these storms are "electrically excited" there is no disputing, and that they occur during the prevalence of the chief auroral displays is also a matter of observation, but so far their connection with aurora has not been sufficiently determined to permit any opinion to be expressed.

The recent successful completion of the telegraph circuit to Shetland, and the extensions immediately to be carried out one hundred miles farther north, will afford much greater facilities for auroral observation than has hitherto existed. It is also proposed to institute a careful spectroscopical examination of the coloured scintillations; and now that the Meteorological Society are about to establish an observation station in Shetland, there is every prospect of some valuable data being collected on this interesting subject, which may hereafter guide our meteorological students in arriving at some satisfactory conclusion regarding the laws of electrical storms and auroral induction. At present we are only able to record a few carefully observed facts.

## THE LIGHT OF JUPITER'S SATELLITES

*Ueber die Helligkeitsverhältnisse der Jupiterstrabanten*, von Dr. R. Engelmann, Observator der Sternwarte zu Leipzig. (Leipzig; London: Williams and Norgate. 1871.)

OF all the satellite systems which so essentially enrich the retinue of the sun, none, when we have left our own moon behind us, promises such a reward for investigation as that of the planet Jupiter. The remoter ones may be, and probably are, intrinsically of a more remarkable character, but they are, and ever will remain to a great extent, beyond our reach; while the attendants of the largest among the planets are numerous enough to interest by individual peculiarities, which their comparative proximity enables us to study with advantage. Yet it is readily observable that though ordinary telescopes of good quality would have done much towards elucidating their phenomena, very little progress has been made in the inquiry, especially in this country; and the work now before us is the first attempt to collect and to make serviceable the scattered observations which exist, of which we are sorry to remark how few are due to the astronomers of England.

The especial object of the eminent observer at Leipzig has been not the theory of the motions of these satellites, but simply their physical aspect in regard to the variable light which they have long been known to reflect, and to this investigation the author, notwithstanding constant engagement in important zone observations, has contributed far more than all who have preceded him. The instrument which he employed was the astrophotometer of Zöllner. In this ingenious contrivance, the light of the object to be examined is referred to that of one or more known comparison stars, by means of an artificial star produced by a petroleum flame, adjustable for brightness and colour by a Nicol prism, and a "colorimeter," or revolving wheel of tinted rock-crystal. But in order to eliminate the effect of unequal areas, so as to ascertain, not merely the absolute amount of light reflected, but the "albedo," or reflecting power of each surface, it is, of course, necessary to obtain reliable measures of these minute specks of light; and in order to decide the interesting question whether or not their rotation and revolution are, as with our own satellite, synchronous, their anomalies, or orbital positions relative to their primary, have to be taken into account. All this has been done with most praiseworthy care; the whole is discussed and reduced with scrupulous and exemplary attention to every possible source of accidental error; and the result is given to the eye in several elaborate diagrams. We shall merely specify some of the conclusions, which will be found of considerable interest to astronomers. The absolute brightness was found by the author, as it has been by all previous observers, very variable; and from the irregularity and occasional rapidity of its changes, it becomes impossible to decide, in the case of the three interior satellites, whether the periods of rotation and revolution are identical. This, however, appears to be decidedly the fact with the outermost. Herschel I. had extended the inference to all of them; but such a result could not now be accepted; and it seems probable that the spots which must occasion these variations, and which have been repeatedly noticed when the satellite has been on the disc of Jupiter, and by Dawes and Secchi even in other positions, may be of changeable character. At a mean II. is relatively the most, IV. the least luminous. As to their micrometrical measurement, every one who is acquainted with the telescopic aspect of these minute discs will readily comprehend its difficulty. It has, however, been attempted in various ways, but not by the double-image micrometer, which does not seem to have been used; the results, as may be expected, present considerable discrepancies, but the final values obtained by a combination of different methods in the hands of various observers are as follow:—I., 1″·081; II., 0″·910; III., 1″·537; IV., 1″·282; or, in English miles, 2,498, 2,102, 3,551, 2,962, the solar parallax being taken as 8″·90. These values, all things considered, differ so little from those given by Lockyer (Guillemin's "Heavens")—namely, 2,440, 2,192, 3,759, 3,062—that we may consider ourselves possessed of a very fair approximation to their real magnitudes.

As to the "albedo" of their surfaces, I. shows no great variation; it falls, according to Zöllner's estimate of the reflective power of terrestrial materials, between that of marl and white sandstone; II. has the greatest variations of albedo, which at a mean somewhat exceeds that of white sandstone; III., the variations of which are smaller and more regular, comes between marl and quartzose porphyry; IV., which varies least, equals that of moist arable land. It will probably be thought, however, that curious as these comparisons may be, the standards are much too uncertain to give any satisfactory result. As to colour, Dr. Engelmann, after citing the elder Herschel's estimates—I., white; II., white, bluish, and ash-coloured; III., white; IV., dusky, dingy, inclining to orange, reddish, and ruddy—specifies as the determination of other observers: I., yellowish; II., white or yellowish; III., intensely yellow with low powers; IV., in achromatics a distinct dusky blue. (These colour-values at any rate afford no countenance to the common impression that Herschel had a bias for red tints.) To the writer, whether with two achromatics, or a nine-inch silvered mirror, this satellite has always appeared ruddy when its colour has formed the object of notice; in such discrepancies something may be instrumental, something subjective. It is pleasant to see here a very full appreciation of the laborious perseverance and honest accuracy of the labours of Schröter, to whose merit time seems to be doing tardy justice; no notice is taken, however, of the observations of Gruithuisen, who twice appears to have seen spots on III on the background of the sky; nor is reference made to the irregular shape of that satellite remarked by Secchi and his assistant; nor to the apparent discrepancy which has often been noticed between the magnitudes of the satellites and their shadows. Still, the treatise may be considered as very nearly an exhaustive one; and a most important and acceptable contribution to planetary astronomy. It may be added that it contains a very valuable determination of the telescopic magnitude of Jupiter, from the average of eleven observers; the result being, with the double-image micrometer 37″·609 for the equatorial, 35″·236 for the polar diameter; with the wire micrometer, 38″·312 and 35″·914: the former values, which he seems to prefer, exhibiting a flattening of $\frac{1}{15\cdot82}$.

T. W. WEBB

## OUR BOOK SHELF

*Transactions of the Geological Society of Glasgow.* Vol. III. Supplement. On the Carboniferous Fossils of the West of Scotland: their Vertical Range and Distribution. By John Young, Vice-President. With a General Catalogue of the Fossils and their Mode of Occurrence, and an Index to the Principal Localities. By James Armstrong, Honorary Secretary. (Glasgow, 1871.)

THIS catalogue of fossils will doubtless be of great use not only to local geologists, but to others at a distance, who may desire to compare the treasures of English and Irish Carboniferous strata with what the equivalent beds in Scotland have yielded. So far as they go, the lists appear to be drawn up with considerable care, and Mr. Armstrong is to be congratulated upon the result of what must have been somewhat laborious work. But we are sure he will be the first to admit that much, very much, still remains to be done before the Scottish Carboniferous flora and fauna can be satisfactorily compared with those of other countries. We are constantly being reminded throughout this catalogue that not only in private collections, but also in public museums in the West of Scotland, there are numbers of specimens under almost every class waiting to be identified, amongst which there is every reason to believe that not a few are species new to science. This, it seems, is specially the case with the plants, the rich flora of the Carboniferous period being represented in the catalogue by only ninety species. But Mr. Carruthers, we are told, has several undescribed specimens in hand, of which we shall, no doubt, hear by-and-by. The fishes, it would appear, also need looking after. There are eighty-four species, under forty genera, named in the catalogue; but a large number in various collections have never been correctly identified with described species, and Mr. Young expresses a hope, in which we cordially join, that Prof. Young will be induced to prepare a special catalogue of these and the Reptilia, of which only seven species are given by Mr. Armstrong. The other classes are represented as follows:—Foraminifera, 2 genera, 4 species; Hydrozoa, 1 g. 2 sp.; Zoophyta, 22 g. 59 sp.; Echinodermata, 6 g. 15 sp.; Annelida, 4 g. 7 sp.; Crustacea, 19 g. 71 sp.; Insecta, 2 g. 2 sp.: Polyzoa, 11 g. 36 sp.; Brachiopoda, 15 g. 50 sp.; Lamellibranchiata, 28 g. 127 sp.; Pteropoda, 1 g. 1 sp.; Gasteropoda, 15 g. 75 sp.; Cephalopoda, 6 g. 46 sp. From these numbers it will be seen that the collectors have not been idle, and, no doubt, Mr. Armstrong's catalogue, with its minute index to localities, will be the means of sending many to hunt in quarters which they have not already visited. Let us hope that they will note something of the conditions under which the fossils are distributed, and not content themselves simply by bringing away good bags full. Collectors cannot be too often reminded that it is of more importance, in the interests both of natural history and geology, to know one limited district thoroughly, than to go roving over half a country merely for the purpose of picking up finely preserved specimens. Each should mark out for himself some practicable area, and make it his endeavour to search every bed, even the most unpromising, noting not only the fossils he meets with, but the character of the strata in which they occur. He should also observe what effect a change in the character of a bed has upon the fossils it may happen to contain; whether they increase or decrease in numbers, whether they individually gain in size or become dwarfed, and, should certain species disappear, what others, if any, are substituted for them. It is only by marking carefully such points as these that we can ever hope to acquire an adequate conception of the natural history of the old carboniferous lands and seas. Mr. Young is quite sensible of the shortcomings of the collectors in this matter, and gives them some seasonable advice, which it may be hoped they will take to heart. If collectors paid better heed to these matters they would assuredly derive greater pleasure and profit from their pursuit, and do much more towards the progress of science. Mr. Young himself, however, notwithstanding the good advice he gives, is not always careful in drawing conclusions, geological evidence being sometimes quite overlooked. Thus, we find him stating that the coal-measures (meaning, of course, the whole series of strata above the Millstone Grit) are "evidently of land and fresh-water origin," because they have yielded no marine organisms, save in one thin local bed near the top of the series. The occurrence of this stratum with its marine remains, indicates, as he believes, the return for a short time of the sea, which had for a very long period "been completely shut out by barriers." Mr. Young is welcome to his belief. If every bed or series of beds in which no marine organisms occur must necessarily be of fresh-water origin, the lakes of old must have been something worth seeing. There are several points suggested by the catalogue that we should like to have taken up, but our space is exhausted, and we can only conclude by strongly recommending Mr. Armstrong's work to the notice of our geological readers. J. G.

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### On the Solution of a Certain Geometrical Problem

A WRITER in the number of NATURE for September 21, Mr. R. A. Proctor, in the course of a letter on the state of geometrical knowledge in the university, alludes manifestly to the solution of a problem which I have adopted in my edition of Euclid. The matter is of small importance in itself, but nevertheless as some points of interest are incidentally involved, I request you to allow me the opportunity of offering a few remarks.

The problem is this: to describe a circle which shall pass through a given point and touch two given straight lines. Your correspondent considers that in giving a solution which depends on the sixth book of Euclid, instead of one which depends only on the third book, I exhibit signal geometrical weakness.

The problem, I need scarcely say, is very old; indeed, so old that a writer who had been long engaged in teaching could not pretend to solve it afresh, for he would certainly have in his memory one or more solutions which had become quite familiar to him. The solution by the aid of the third book is well known, for it occurs in several of the collections of geometrical exercises. The solution which I have adopted is also old, but seems not so well known. It is, I think, conspicuous for simplicity, elegance, and completeness. The demonstration is of the best and most impressive kind, requiring no laborious effort to understand and retain it, but being almost self-evident from the diagram. Even if the problem be treated as an isolated exercise, the solution which I have preferred will sustain a favourable comparison with that which more commonly occurs.

But the determining cause of my choice was the position which the solution occupies as one of a connected series. I have just before treated a similar problem by the third-book method, so that if the same method had been used for the present problem, there would have been only repetition without any substantial increase of knowledge; whereas by the course adopted the student is introduced to fresh and valuable matter. The principle of similarity and the notion of a centre of similitude are most instructively involved, and the student is prepared for a subsequent investigation, which is similar but more complex. To sum up, the third-book method would have constituted no advance in the subject, where the sixth-book method takes a step important in itself and in its consequences; and therefore, following the example of an eminent geometer, I adopted the latter method. I may perhaps venture on the strength of my own experience as to the utility of the solution, to recommend it to the attention of other teachers.

It is very important to bear in mind the distinction between what I may call absolute and relative merit which I have just exemplified. The solution of a single problem furnished by a candidate under examination, or by a contributor to a mathematical periodical, is very different from the investigation of one out of a chain of propositions in a mathematical treatise. In the former case there are no antecedent or subsequent conditions to regard; in the latter case we have to consider what agrees best with the whole scope of the work, with what is to follow as well as with what has gone before. A writer, after arranging a paragraph or a chapter in what seems the best manner, may find himself constrained at a subsequent stage to make changes which would have been unnecessary, perhaps even undesirable, if the earlier portion had stood alone. Then, if a reader opens the book at random and criticises a passage without any regard to the author's sense, the criticism may very naturally be quite inappropriate.

There is, however, a very important consideration of another kind which has been frequently disregarded, but which is pressed upon our notice by the interest at present felt in geometrical studies. Let us determine the reason which leads us in some, or in many, cases, to prefer a solution which involves only the third book of Euclid to a solution which depends on the sixth book; this, I apprehend, is merely a persuasion that Euclid's order is a natural order, so that in a well-arranged system the propositions of the third book ought to precede those of the sixth book. I am of this persuasion myself; I think that no scheme can be perfect, and, on the whole, I am well satisfied with Euclid's. But there are places where Euclid is strong, and there are places where Euclid is weak; and the position which he has assigned to the last three propositions of his third book, must rather be classed with the latter than with the former. His object, of course, must have been to lead up to his construction of a regular pentagon, and we cannot be surprised at the introduction of that remarkable process. But I have always envied the advantage in this respect to be claimed for the non-Euclidean systems, which transfer these propositions and place them after the doctrine of similar triangles; thus the long and rather artificial treatment which they receive from Euclid is superseded, and the propositions become almost intuitive. Hence, in fact, if we have recourse to the sixth book of Euclid when we might have accomplished our end by the aid of the first thirty-four propositions of the third book, we may be fairly liable to the charge that we have not adopted the simplest and most natural method; but the last three propositions of the third book are quite different in kind from the others, and instead of using them, it may be really as simple and as natural in many cases to use the principle of similar triangles.

I shall be obliged to any person who may be skilled in practical geometry if he will state what he considers the best method of *actually* solving the problem, supposing that both circles are to be determined which satisfy the conditions. I assume that we have the aid of compasses and also of one of the ordinary contrivances for drawing parallel lines. This is a matter of some interest, though of course unconnected with the theoretical solution of the problem.

I should be glad to make some remarks on the general subject which led to the notice of the particular problem I have discussed, but at present I have not sufficient leisure. I must content myself with having shown that the course into which I am supposed to have drifted by geometrical incapacity, was adopted deliberately under the guidance of reasonable geometrical knowledge.

I. TODHUNTER

St. John's College, Cambridge, Oct. 2

### Structure of Fossil Cryptogams

IT was unfortunate that at the recent meeting of the British Association, Prof. Williamson's paper had to be discussed in a very hurried manner, and he is, no doubt, justified in taking care "that there shall be no misunderstanding as to the real point at issue." I do not think that he has brought it out very plainly in his paper in NATURE, and perhaps, as he mentions me as an opponent of his views, I may be allowed to state precisely in what respects I differ from him.

First, as to matters of fact. Prof. Williamson speaks of the central structure of the stems of the extinct Lycopodiaceæ as a "vascular medulla," by which he explains that he means a "structure containing vessels," and that there shall be no misapprehension he adduces *Nepenthes* as possessing it; the instance is a well-known one, and leaves no room for doubt as to Prof. Williamson's meaning. Now from the examination of specimens, and of the drawings of them published by Mr. Carruthers (the accuracy of which I believe Prof. Williamson does not dispute) I am quite satisfied that the central structure consists wholly of scalariform vessels, and that there is in fact nothing medullary or medulla-like about it.

Outside this central structure is what Mr. Carruthers terms the investing, and Prof. Williamson the vascular woody cylinder. I believe that Mr. Carruthers is right in looking upon this as belonging to the central axis, which is therefore composed of two parts.* I find, which I did not sufficiently appreciate at the time, that Prof. McNab regards this investing cylinder as homologous with the cylinder of wood cells surrounding the central axis of fibro-vascular bundles which is met with in many recent Lycopodiaceæ. From this I certainly dissent for two reasons; (1) because I think its equivalent is to be found in the central axis itself, and not outside it; (2) because it is not composed of wood cells but of scalariform vessels.

Secondly, as to opinions. The terms Exogen and Endogen, as is pretty well known, were founded upon a mistake. A great deal too much has been made of the difference implied by them; in fact, if we compare a one-year-old dicotyledonous shoot with a monocotyledonous stem, we find that it does not exist. If Prof. Williamson will look at the stem of the common artichoke, he will find it difficult to convince himself that he is examining an "exogenous" plant at all.

The imagined characters which were implied by these terms are, nevertheless, as everyone knows, correlated with others, which in the aggregate enable phanerogamic plants to be divided into two satisfactory groups; but this is certainly not equally the case with the groups into which Prof. Williamson would divide the vascular cryptogams. These groups, I think, most botanists will agree in considering in the highest degree unnatural, inasmuch as, assuming the vegetative distinction upon which they are founded to exist, it is a wholly artificial ground for classificatory purposes. Nor is it any argument that one vegetative character must be good because others are in use, since the simple answer is that these coincide with natural divisions, while Prof. Williamson's does not.

I shall not dispute Prof. Williamson's position that our living Lycopodiaceæ should be interpreted by the more complete extinct types. To do this, however, the extinct types must be thoroughly understood; when we are dealing with imperfect material, comparison with the more perfect but less highly developed existing plants is not only justifiable but necessary.

It is obvious that the great development of the stem in the Lycopodiaceæ of the Coal Measures was correlated with their arborescent habit. I am inclined to think with Prof. Williamson that the stem increased in thickness; it is certain that *Lepidodendron* was branched, and not improbably also *Sigillaria*. The branches as they were gradually developed must have been the cause of an increasing strain upon the stem; it seems to me more congruous with known laws of the response of structure to circumstances, to conclude that the stem was proportionately developed as the strain increased, than that the stem should have been produced once for all of its maximum thickness without reference to the crown of branches that was finally to surmount it.

I am quite prepared therefore to admit that the investing cylinder may have increased by external additions, and probably did do so; this would of course imply the existence of a cambium layer outside it. There is some analogy for this in the recent *Isoëtes*, where we have a "slight woody mass which occupies the longitudinal axis of the stem, but encloses *no pith*." † Outside this we have a "bark-forming" cambium (which also adds, but more sparingly, to the wood mass); in *Sigillaria* and *Lepidodendron* we might have had a cambium not merely renewing the bark but adding to the central axis.

In whatever way the increase took place, it was, as I think, nothing more than an incident in the life history of a particular race of plants, nothing more than an adjustment to an arborescent habit dropped when the arborescent habit was lost, but showing a lingering ancestral tendency in *Isoëtes*. Comparing a simple stemmed palm with *Dracæna*, we have a parallel instance of the strengthening of the stem *pari passu* with the continued development of a system of branches; only in *Dracæna* it is the circumferential part of the stem alone which developes.

If I am right in regarding a stem gradually developing in size as the necessary correlate of a large system of branches, Prof. Williamson's view practically amounts to the old division of plants into trees and herbs. I cannot see how it can afford any safe ground for a re-arrangement of the vascular cryptogams.

W. T. THISELTON DYER

London, Sept. 26

* *Monthly Micro. Journ.*, 1869, p. 169.
† Hofmeister, Higher Cryptogamia, pp. 356, 361

### The Solar Spectrum

MAY I venture to suggest that quite possibly something of value might be obtained by observing the sun during totality with a spectroscope of reasonable dispersive power (say four or five prisms) *without a collimator*, or even simply with one of the so-called meteor spectroscopes.

If the bright rays and rifts are really and simply (or even mainly) composed of the green-line-giving substance, they will give a well-defined green image; if they are formed by reflection (either at the sun or in our atmosphere) of ordinary sunlight, they would be so dispersed as to be invisible or nearly so, and if formed by the reflection of chromosphere light they would give several images, the red (C) and blue-green (F) being most conspicuous.

C. A. YOUNG

Hanover, N.H., U.S., Sept. 13

*** Arrangements have already been made for carrying out a similar suggestion to this by the Eclipse Committee; and the corona will also be observed with an open slit.—ED. N.

### Eclipse Photography and the Spectroscope

THE endeavour of the Eclipse Committee to secure some uniformity in the photographs from different stations next December does not appear to be duly appreciated, it being contended that immense "personality" shown in various photographers' manipulation must frustrate the good intention. I submit that in this case the personality is greatly over-estimated; that a number of competent photographers taking the same subject would probably produce, under any ordinary circumstances, pictures bearing considerable resemblance; while by using like apparatus and giving exposure of the same duration, we might safely predict a similarity of result amply sufficient for comparative purposes, and for the identification of structural peculiarity should it exist.

Among others there is a possible advantage to accrue from uniform work by the philosophers which I have not seen or heard noticed. Supposing the outer corona, rays, streamers, or any portion of the apparently luminous matter be terrestrial, is it unreasonable to expect that photographs, taken at stations more or less widely separated, will, when properly combined in the stereoscope, give clear ocular proof of the sublunary situation of such luminous matter?

HENRY DAVIS

### Phenomena of Contact

MR. STONE can safely be left to meet the arguments specially addressed to him in Prof. Newcomb's letter; but as the subject relates to the only point of importance touched on in Prof. Newcomb's criticism of my chapter on the sun's distance, I crave permission to meet his general argument.

I submit that he tries to prove too much.

He admits that the phenomenon of irradiation exists in the case of a disc. The sun's disc, then, must be to some extent enlarged, and the dark disc of Venus must be to some extent reduced by the effects of irradiation. Now this being so, *what becomes of the cusps*, when Venus is all but wholly on the sun's disc? Either the irradiation is diminished near the cusps or it is not. If it is diminished there must be distortion, because the disc of Venus is then not uniformly reduced: if the irradiation is not diminished a ligament must appear.

Let any one draw a large circle (say a foot in diameter) on paper, and a small one (say an inch in diameter) extending very slightly (say by the twentieth of an inch) beyond the boundary of the first; and let him blacken the smaller circle as well as all the space outside the larger one. He has then a space representing the disc of the sun with a very large Venus upon it near the time of internal contact. Now let him conceive the whole of this space (a sort of exaggerated crescent) slightly enlarged as by irradiation, the enlargement-fringe extending outside the boundary of the large disc and inside the boundary of the small black (incomplete) disc. He will find the conception of this enlargement exceedingly easy everywhere save near the cusps; but here there is a difficulty in determining how the fringe outside the larger disc is to be joined on to the fringe inside the smaller disc. If he can conceive these two fringes meeting in such sort as to leave the reduced outline of the small disc completely circular up to the very points in which it meets the enlarged outline of the large disc, he will have done what Prof. Newcomb's theory requires. But note, this must be done for the case when the fringe of enlargement is wider than the twentieth of an inch, by which the small disc overlaps the large one. When this is the case, the task will be found to be impracticable; but even when the overlap of the small disc is greater, the task can only be achieved by actually making new cusps out of the irradiation fringes. (A figure would make this explanation much simpler.)

Prof. Newcomb says that he is decidedly of opinion that the irradiation of an extremely minute thread of light is not the same with that of a large disc. He does not seem to notice that if this is so, Venus just before, at, and just after internal contact, *must* be distorted. This even if—admitting the enlargement of the sun's disc—he denies that the disc of Venus is reduced by irradiation.

He fails also to observe that a peculiarity such as distortion, or the formation of a ligament, may escape the notice of inferior or not very attentive observers, and so all his negative observations be explained. It is no proof of superior skill in observation to see no signs of an illusory effect. Until we have observers who recognise no traces of irradiation when looking at the solar disc, we must believe that (as Mr. Stone has, I think, already asserted) the non-recognition of distortion or ligament formation is due to inattention, or want of observing skill. That this should be more common than close and careful scrutiny is not a very surprising circumstance, and proves nothing. RICHARD A. PROCTOR

### Oceanic Circulation

IN NATURE of August 17, I have just seen the report of the discussion on Dr. Carpenter's paper on the above subject read at the late meeting of the British Association.

Dr. Carpenter, explaining the movements on thermodynamic principles, states that he has "found the *primum mobile* of this circulation was not in equatorial heat but polar cold," and explains that "(1) As each surface-film cools and sinks, its place will be supplied, not from below, but by a surface influx of the water around; and (2) the bottom stratum will flow away over the deepest parts of the basin, while, since the total heat of the liquid is kept up, there will be an upper stratum which will be drawn towards the cold area, to be precipitated to the bottom and repeat the action. Apply this principle to the great oceanic area that stretches between the equator and the poles, we should expect to find the upper stratum moving from the equator towards the poles, and its lower stratum from the poles towards the Equator. That such a movement really takes place is indicated, as it seems to me, by various facts."

It does not appear, however, that Dr. Carpenter has well established his claim to the theories in question, while, in a pamphlet on the same subject, published in 1869 by Dr. Adolph Mühry of Göttingen, we find such passages as the following:—"As the cause of the latitudinal circulation we have assumed the difference of temperature in the water between the equator and the pole." He honestly gives Arago the credit of being, perhaps, the first to put forward this view in 1836; and after remarking (p. 11) that it might be considered doubtful whether it is the upper warm current from the equator or the under cold one from the pole that ought to be considered the primary, he says (p. 12) "For us the primary 'arm' is the heavier, *i.e.*, the colder polar stream, which, in obedience to gravitation, falls in a horizontal direction toward the lighter water of the hot zone; and the secondary 'arm' is the returning antipolar. It moves to replace what flows away, and is, therefore, the compensation-arm."

Here, without following Dr. Mühry any further, we find the thermodynamic theory advanced by Dr. Carpenter, and his *primum mobile* as well; but by giving him credit for ignorance of Dr. Mühry's work, we may excuse him for laying claim to what is there put forward, and accepting therefore the commendation of others as unknowing as himself. J. B.

### Ice Fleas

DURING a recent ramble upon the Morteratsch Glacier, I also observed a large number of the minute black creatures described by Prof. Frankland in NATURE, No. 100. My attention had been directed to them ten years ago by Lord Anson on the "snow-bones," near the summit of the Ægischorn. They are only nominal "cousins" of the flea (*Pulex*) of civilised life, and are not at all related to *Dapnia*, the "water flea," but are closely allied to the minute insects which are often seen on the surface of stagnant water, resembling grains of gunpowder, and skipping partly by help of their forked tail, folded under them so as to serve as a foot, hence their name *Podura*, or "skip-tail." They have been named by Agassiz *Desoria saltans*. Their food, I conjectured with Prof. Frankland, consists of "red snow" and other microscopic algæ. Not being myself within reach of a good library, I can only furnish your readers with a key to further information. C. A. JOHNS

IN NATURE of 28th September, Prof. Frankland, in introducing the ice flea to the readers of NATURE, uses the expression "if known at all," and concludes by asking information about it. The glacier flea, *Desoria glacialis*, was noticed and described by Prof. Agassiz as far back as 1845, in his Ascent of the Wetterhorn on the 29th of July of that year. Not having Agassiz's work at present beside me, I cannot refer to it, but these fleas are noticed in an extract translated from an account of the ascent, and published in *Hogg's Weekly Instructor* for Dec. 1845, vol. ii. p. 221. On the Aar Glacier they are described as being scattered over the "surface of the snow in millions," elsewhere, "as being collected in masses under the stones on the ice." R. C.

### The New Dynameter

THE letter from the Rev. T. W. Webb in your last number is a very tantalising letter. He tells us, and we could not wish to have a better authority, that a new dynameter has been invented by the Rev. E. Berthon, but he does not tell us how it is constructed or where it can be obtained.

I may take this opportunity of mentioning a makeshift dynameter which I have found to answer very well when extreme accuracy is not required.

I have a pocket telescope fitted with a Cavallo micrometer, *i.e.*, a slip of finely divided mother-of-pearl screwed to the diaphragm next the eye-glass. Unscrewing the two last draws of this telescope the end of the second is applied to the eye-piece of the telescope of which the power is to be measured, and the first draw pushed in till the image of the object-glass comes sharp upon the mother-of-pearl. The diameter of the image is thus given in divisions on the mother-of-pearl, the value of which, in hundredths of an inch, has been previously ascertained. W. R.

### Notaris on Mosses

WITH reference to the notice of De Notaris' book on Mosses, I am informed by Dr. Dickie that the genus *Habrodon* was discovered in Great Britain several years ago by the late Mr. McKinlay, of Glasgow, and that he had received from Mr. Wilson about two years ago from his district *Conomitrium julianum*. Dr. Dickie sends specimens of *Habrodon Notarisii* gathered at Killin by Dr. Stirton. M. J. BERKELEY

*** In the review referred to, Prof. De Notaris was erroneously described as of Geneva, instead of Genoa.—ED. N.

### "Newspaper Science"

MY attention has just been called to a letter from Mr. David Forbes which appears in NATURE, Sept. 21, under the head "Newspaper Science," and in which that gentleman, writing from Boulogne, pathetically describes the emotions with which he read a recent "*article*" in the *Globe* on "Krupp's" Gun-manufactory at Essen. I need hardly say how deeply I deplore the shock which I have unwittingly been the agent of inflicting on your distinguished correspondent. It will be some small satisfaction if you will allow me to express the hope that the "desired result" of Mr. Forbes's "reluctant" compliance with the advice of his "medical man," and most wise resolve "to eschew everything scientific for the next few weeks at least, in order to recruit before the winter labours commenced," may not be utterly defeated by the perusal of "a specimen of English scientific opinion," of which I am unhappily the author. It would be a terrible reflection indeed, that a stupid error on my part had, perhaps, imperilled the accuracy and success of Mr. Forbes's "winter labours." The blunder (or rather blunders) occurred as follows:—I, too, was "knocked up with work," but being myself a "medical man" naturally only in part carried out my own prescription. I would, for the sake of Mr. Forbes, and the credit of "English scientific opinion" in the estimation of his "French acquaintance," I had exercised a little more discretion. However, unfortunately, I stumbled on the Krupp factory, and all forgetful of my dilapidated mental condition, wrote a note-paragraph (I never write "articles"), which I vainly imagined might have been innocent and interesting. It is not always possible to compress even the manuscript necessary for a paragraph on to a single sheet of paper, and I grieve to say that after my paper had passed the editorial eye three words forming the connecting link of a sentence must have been dropped. What I intended to say, without the slightest notion of giving a "technical or scientific" opinion, was, "The iron is alloyed *in crucibles*, *formed* with certain clays and a preparation of plumbago." The words italicised disappeared in some mysterious way. The next of my idiotic sentences goes on to talk about the crucibles, or "*creusets*," as, to the great scandal of Mr. Forbes, I ventured to call them. If I could stop here, an humble apology for my fault might, perhaps, serve my purpose, but, alas! I have more to answer for. Vaguely dreaming of the foot-pound, I actually *wrote* kilometre for kilo*gram*metre, when speaking of the power of the new steam hammer; and, worst of all, I also WROTE "Sheffield Gun Metal."

Can I ever hope to be forgiven when thus I write myself down an ass? MEDICUS

P.S.—As to the question whether Krupp invented the process employed at his factory, I offer no opinion "scientific" or ordinary. I only repeat the impression which prevails.

## *FURTHER NOTES ON CERATODUS*

SINCE the article on *Ceratodus* (published in NATURE, Nos. 99 and 100), was written I have examined a mature female, transmitted, with other examples, by the Trustees of the Sydney Museum to the National Collection, and am enabled to make the following additions:—

1. The oviduct in its developed conditions is, with regard to its internal structure, surprisingly similar to that of *Menopoma*.

2. The ova are expelled through the oviduct, and not through the peritoneal slits; they receive in the oviduct a coating of an albuminous substance as in Batrachians.

3. The caudal termination of the vertebral column is subject to individual variation. In one example the neural and hæmal elements are continued far beyond the notochord, and are confluent into a tapering band, which is segmented, as is the case in some specimens of *Dipterus* or *Ctenodus*. ALBERT GÜNTHER

## *ON THE BENDING OF GLACIER ICE* *

MR. MATTHEWS and Mr. Froude had supported long rectangles of ordinary ice at the two ends, weighted them in the centre, and thus caused them to bend. The ice employed, if I recollect right, was of a temperature some degrees below the freezing point, and in my little Alpine book recently published I expressed a hope that similar experiments might be made with glacier ice. I have been trying my hand at such experiments. The ice first employed was from the end of the Morteratsch Glacier, and when cut appeared clear and continuous. A little exposure, however, showed it to be disintegrated, being composed of those curious jointed polyhedra into which glacier ice generally resolves itself when yielding to warmth. Still, when properly supported and weighted, a long stout rectangle of such ice showed, after twelve hours, signs of bending.

I afterwards resorted to the ice of the sand cones, which, as you know, is unusually firm. From it rectangles were taken from three to four feet long, about six inches wide, and four inches deep. Supported and weighted for a considerable time, no satisfactory evidence of bending appeared; the bars broke before any decided bending took place. Smaller bars were then employed. Two of these were placed across the mouth of an open square box, their ends being supported by the sides of the box. They formed a cross, and a clear interval of at least an eighth of an inch existed between them where they crossed. The upper one was carefully weighted with a block of ice; after two hours it had sunk down, and was found frozen to the under one. They were then separated, and one of them was allowed to remain supported at the ends and weighted by ice at the middle. In a few hours it had bent into a curve, the versed sine of which from a chord uniting the two ends was, at least, two inches. In fact, *when the rectangles are thin*, and the weight carefully laid on, flexure commences very soon, and may by cautious manipulation be rendered very considerable. I think Mr. Froude told me that in his experiment the molecules were "in torture," and that they in great part recovered their positions when the weight was removed. In the foregoing experiments the flexure was *permanent*.

I tried to bend the rectangle just referred to back again by reversing its position and weighting it with the same block of ice. But whether owing to my want of delicacy in putting on the weight, or through the intrinsic brittleness of the substance itself, it snapped sharply asunder.

I left in your hands when quitting London an exceedingly interesting paper by Prof. Bianconi, in which are figured the results of various experiments on the bending of, I think, lake ice. The foregoing experiments on glacier ice confirm his results.

August 4 JOHN TYNDALL

I may add that various experiments were subsequently made, and a means discovered of rendering the bending very speedily visible. I hope before long to return to the subject.—J. T., September 28

* The following is an extract from a note addressed to Prof. Hirst, and sent from Pontresina in the hope that it would reach Edinburgh in sufficient time to be communicated to Section A of the British Association. It was a few hours too late.—J. T.

## *THE MIGRATION OF QUAIL*

THE fact of this little bird having visited England this year in such numbers appears to have attracted the attention of naturalists as well as sportsmen. In the columns of the *Field* may be found a census giving particulars of this migration. And it will appear a curious coincidence when I mention that there has been here a greater migration of quail this year than ever remembered before. Where they come from is somewhat mysterious. They have been shot in hundreds in some paddocks, and found as numerous as ever in ten days. I can only account for it by stating that it has been a most remarkable year for grass, and consequently cover was good; and this does not appear conclusive, for the grass has been good all over the country for hundreds of miles towards the north, from which direction some appear to think they come. They are found generally in paddocks, where thistles grow. Can there be any common cause affecting these facts?

Melbourne, August 10 AUSTRAL-ALPINE

## *JARDIN D'ESSAI, ALGER*

IN 1832 the then French Government conceived the idea of forming near the town of Algiers a botanical garden, in which all plants likely to be easily grown in Algeria, and which might be useful either for their ornamentation, or from their economic value, should be kept for distribution or for sale. A portion of ground situated between the sea and the public road, and occupying the place of an old *hamma* or marsh, was selected for this purpose, which is about two miles from the town. In 1867 the Emperor of the French conceded this establishment to the "Société Générale Algérienne," under whose auspices, but under the direct superintendence of M. Auguste Rivière, the gardens at present are.

In addition to the level swamp, the gardens now also occupy the slope of a low hill on the opposite side of the road. The level ground is laid out in alleys which open out into a circular boulevard which surrounds the whole garden. Carriages are admitted to the circular drive only, foot passengers to the cross walks. A stream of fresh water runs through the grounds, forming in one place a small lake.

One fresh from the Botanical Gardens of Europe is astonished at every step taken in the Gardens by the wondrous vegetation which is shown by all the semi-tropical plants. Descending a few steps from the circular drive, a great palm avenue is entered. This avenue was planted in 1847, and is formed of about eighty trees of the date palm, nearly as many of the *Latania Borbonica*, and about 150 of the dragon's blood tree (*Dracæna draco*). The avenue is about ten yards wide, and between every two of the date palms there are two of the dragon's blood tree and one Latania. It terminates in a clump of palm trees which are planted almost to the border of the sea. When it is borne in mind that the date palms are from twenty to fifty feet high, the Latanias averaging about twelve, and the Dracænas about eight feet in height, the general effect of this splendid avenue may be imagined. All the trees were in December last in full flower or fruit, the golden trusses of the date palm contrasting well with the more brightly-coloured clusters of Latania berries. It would require more space than is at our disposal to describe the contents of all the various small avenues that branch off from the main one. The most remarkable smaller avenues are, perhaps, the one formed of bamboo (*Bambusa arundinacea*), planted in 1863, and forming an immense mass of foliage, the stems supporting which are from forty to fifty feet high, and that formed of about 100 plants of *Chamærops excelsa*, each about ten feet in height. But remarkable as are these charming sub-tropical alleys, the visitor is more than surprised when on going towards the portion of the garden where the plants are grouped somewhat according to their natural orders, he finds specimens fifteen feet high of *Caryota urens* and *C. Cumingii*, growing with vigour and covered with fruit; of *Oreodoxa regia*, from Cuba; several plants upwards of twenty-five feet in height; and a plant of *Jubæa spectabilis*, which is twelve feet high; and then just a few steps more and a parterre alloted to the natural family of the Musaceæ comes to view. As both the plantain and banana are grown in large quantities for their fruit in another portion of the grounds, the family is here chiefly represented by such genera as *Strelitzia* and *Ravenalia*. Magnificent specimens of the latter genus, with stems nine to ten feet high, exhibited great combs of flowers. We are not aware if the Traveller's tree has flowered in Europe, and we were not prepared to find it in full flower in Algiers. It has not, however, matured its fruit in this garden. Near this grand parterre stood another with many fine specimens of Yucca, also a magnificent plot of Aralias, *A. papyrifera*, in full fruit and very handsome; the fine *A. leptophylla* and *A. præmorsa*, thickly covered with spines, and the very ornamental *A. farinifera;* and then one's attention is caught by a large tree (*Carolinea macrocarpa*) from Brazil, with a couple of dozen of its fruit, each as big as a cocoa nut; by a small forest of *Anona cherimolia* in full fruit, which is nearly as good as that of the closely related species which yields the custard apple. Near these is an immense tree some thirty feet in height, covered with fruit of the Avocado pear (*Persea gratissima*); and at its feet is a quantity of guava trees (*Pisidium Cattleyanum*) crowded with its perfectly ripe, large, pear-shaped, golden fruit. Growing up into the trees, and forming numerous and never-ending festoons, were some specimens of Cacti, chiefly species of Cereus. Some of these were of great size, and one specimen, which had completely strangled a plantain tree some twenty-five feet, was said to have been covered in the autumn with 600 to 700 flowers. It must have been a sight worth a long pilgrimage to see.

Enough has been said to show what a surprising number of semi-tropical fruits luxuriate in the beds of this well-watered garden, and we might add many well-known vegetables to the list, as sweet batat, yam, papaw; but all this while we have been writing of the great level portion of the garden. Outside of this, and on the other side of the roadway, there is a small hill, two or three hundred feet in height, which slopes towards the garden and the sea, and is traversed by several ascending walks. This is the New Holland district of the garden, and certainly not the least interesting portion of it. In one section of it are different species of Acacia, many of them large trees, twenty to twenty-five feet in height. Of the Proteaceæ there were magnificent trees; of the genera *Banksia*, *Hakea*, and *Grevillea*, the collection of species was very large, all of them just bursting into masses of bloom. The most important of the trees growing in this corner of the hill was probably *Eucalyptus globulus*, of which some trees, now about forty feet in height and over four feet and a half in circumference, were planted in 1862, and were then only a few inches high. Young well-established seedlings, of about ten inches in height, are sold for 20*s*. a hundred, and large numbers of them have been planted from time to time throughout Algeria by the French Government. This species grows in Algeria with most surprising rapidity, under very favourable circumstances growing eighteen to nineteen inches in height each month. Its wood appears to be hard, close in the grain, and it is largely used in the construction of quays, bridges, and railways. This tree seems to do so well on the southern side of the Mediterranean that we think its culture ought to be successfully attempted in the south of Spain, in Sardinia, in Sicily, and the southern parts of Italy. In districts subject to heavy winds it requires for some years—owing to its rapid growth—some protection, but in places sufficiently warm for it, it ought to repay well for any little extra care it might be found to need.

Among the few species that we noticed that did not succeed in these gardens, we may mention the *Cedrus deodara;* but *Casuarina equisetifolia* was flourishing, and one tree of *Araucaria excelsa* was about sixty feet in height, and measuring a little over nine feet in circumference at its base.

The object of the Society in keeping up these Gardens is, as we said, to introduce into Algeria all useful and ornamental plants likely to grow there. In addition they grow enormous quantities of young palms and other ornamental plants for exportation to Europe, and some few plants interesting to the botanist for exchange with other establishments. In a place so favoured by nature and so easily accessible to Europe, it would be, we venture to think, well worth the while of the director of these Gardens to considerably enlarge the last portion of the Society's design. How many tropical plants are yet unknown to the large collectors of Europe, and what a vast percentage of deaths occur among the collections sent from the tropics at any season of the year to our shores! But with Gardens like these at Algeria, situated on the sunny side of the Mediterranean, to act as a half-way house, the resources of the Botanical Gardens or establishments of the North would be indefinitely increased. Another purpose for which these Gardens might be made most useful is for forming a collection of specimens of plants or fruits of economic interest. Many of the fruits, stems, &c., which ripen in these Gardens as easily as cherries or potatoes with us, are not to be seen in some botanical collections, and are not, in Europe at least, to be purchased. How gladly would some botanist buy such as we here refer to if they were on sale, say at the depôt of the Algerian Society in Paris; and the expense of putting up such in salt and water would be a mere nothing. The same remarks would apply in many cases to portions of the roots of remarkable genera, and also to flowers. In calling attention to these Gardens, we venture to suggest these hints to their well-known director, and also to that indefatigable botanist who, more than any other, now represents science in connection with the Algerian Society, Prof. Durando of Algiers.

E. P. W.

## THE TEMPERATURE OF THE SUN

THE increase of the volume of atmospheric air, under constant pressure, being directly proportional to the increment of temperature, while the coefficient of expansion is 0·00203 for 1° of Fahrenheit, it will be seen that a temperature of 3,272,000° Fah. communicated to the terrestrial atmosphere would reduce its density to $\frac{1}{6643}$ of the existing density. Accordingly, if we assume that the height of our atmosphere is only 42 miles, the elevation of temperature mentioned would cause an expansion increasing its height to 6643 × 42 = 279,006 miles. This calculation, it should be observed, takes no cognizance of the diminution of the earth's attraction at great altitudes, which, if taken into account, would considerably increase the estimated height. Let us now suppose the atmosphere of the sun to be replaced by a medium similar to the terrestrial atmosphere raised to the temperature of 3,272,000°, and containing the same quantity of matter as the terrestrial atmosphere for corresponding area. Evidently the attraction of the sun's mass would under these conditions augment the density and weight of the supposed atmosphere nearly in the ratio of 27·9 : 1; hence its height would be reduced to $\frac{279{,}006}{27 \cdot 9} = 10{,}000$ miles. But if the atmosphere thus increased in density by the sun's superior attraction consisted of a compound gas principally hydrogen, say 1·4 times heavier than pure hydrogen, the height would be 10 × 10,000 = 100,000 miles. The pressure exerted by this supposed atmosphere at the surface of the photosphere would obviously be 14·7 × 27·9 = 410 pounds per square inch, nearly. Unless, therefore, the depth greatly exceeds 100,000 miles, and unless it can be shown that the mean temperature is less than 3,272,000° Fah., the important conclusion must be accepted that the solar atmosphere contains so small a quantity of matter that notwithstanding the great depth it will offer only an insignificant resistance to the passage of the solar rays. Now, the assumed mean temperature, 3,272,000°, so far from being too high, will be found to be considerably underrated. It will be recollected that the temperature at the surface of the photosphere, determined by the ascertained intensity of solar radiation at the boundary of the earth's atmosphere, somewhat exceeds 4,035,000°. Consequently, as the diminution of intensity caused by the dispersion of the rays, will be inversely as the convex areas of the photosphere and the sphere formed by the boundary of the solar envelope, viz., 1·52 : 1, the temperature at the said boundary will be

$$\frac{4{,}035{,}000^\circ}{1 \cdot 52} = 2{,}654{,}600^\circ$$

The true mean, therefore, will be 3,344,800°, instead of 3,272,000° Fah., a difference which leads irresistibly to the inference that, either the solar atmosphere is more than 100,000 miles in depth, or it contains less matter than the terrestrial atmosphere, for corresponding area. It will be demonstrated hereafter that the retardation of the rays projected from the border of the photosphere consequent on the increased depth of the solar atmosphere (supposed to be the main cause of the observed diminution of energy near the sun's limb), cannot appreciably diminish the intensity of the radiant heat. The ratio of diminution of the density of the gases composing the solar atmosphere at succeeding altitudes, is represented by Fig. 5, in which the length of the ordinates of the curve *a d b* shows the degree of tenuity at definite points above the photosphere. This curve has been constructed agreeably to the theory that the densities at different altitudes, or what amounts to the same, the weight of the masses incumbent at succeeding points, decreases in geometrical progression as the height above the base increases in arithmetrical progression. The vertical line *a c* has been divided into 42 equal parts, in order to facilitate comparisons with the terrestrial atmosphere, the relative density of which, at corresponding heights, is obviously as correctly represented by this diagram as that of the solar atmosphere. It is true that, owing to the greater height of the latter compared with the attractive force of the sun's mass, the upper strata of the terrestrial atmosphere will be relatively more powerfully attracted than the upper strata of the vastly deeper solar atmosphere. The ordinates of the curve *a d b* will therefore not represent the density quite correctly in both cases. The discrepancy, however, resulting from the relatively inferior attraction of the sun's mass at the boundary of its atmosphere, will be very nearly neutralised by the increased density towards that boundary, consequent on the great reduction of temperature—fully 1,380,000° Fah.—caused by the dispersion of the solar rays before entering space. It may be well to add that, in representing the relative height and pressure of the terrestrial atmosphere, *a c* in our diagram indicates forty-two miles, while *b c* indicates a pressure of 14·7 pounds per square inch; and that in representing the solar atmosphere, *a c* indicates 100,000 miles and *b c* 410 pounds per square inch. Bearing in mind the high temperature and small specific gravity, the extreme tenuity in the higher regions of the solar atmosphere will be comprehended by mere inspection of our diagram. Already midway towards the assumed boundary, the density of the solar atmosphere is so far reduced that it contains only $\frac{1}{152000}$ of the quantity of matter contained in an equal volume of atmosphere at the surface of the earth.

Let us now consider the diminution of intensity occasioned by the increased depth through which the heat rays pass which are projected from the receding surface of the photosphere. Fig. 6 represents the sun and its atmosphere extending $\frac{1}{4}$ of the semi-diameter of the photosphere, *m h*, *c g*, &c., &c., being the heat rays projected towards the earth. The depth of the solar atmosphere at a distance of $\frac{19}{20}$ of the radius from the centre of the luminary, will be seen to be only 2·0012 greater than the vertical depth. Now, careful actinometer observations enables us to demonstrate that when the zenith distance is under 60°, the radiant energy of the sun's rays in passing through the terrestrial atmosphere is very nearly in the inverse ratio of the cube root of the depth penetrated (see the previously published table). The increase of depth resulting from atmospheric refraction, it may be well to observe, is too small at moderate zenith distances to call for correction; nor does the atmospheric density vary sufficiently during bright sunshine to affect the radiant intensity appreciably. The table adverted to shows that an increase of the sun's zenith distance of 5′ in 60° occasions a diminution of temperature hardly amounting to 0·044° Fah. Adopting the same rate of retardation for the solar atmosphere as that observed in the terrestrial atmosphere, it will be found that the loss of radiant energy of the solar rays at $\frac{1}{20}$ of the radius from the border of the photosphere will be only 1·26 greater than at its centre. According to the researches of Secchi and others, the loss is fully three times greater than that established by the rate of diminution which we have adopted. This circumstance, in connection with the extreme tenuity of the solar atmosphere, rendering any considerable loss improbable, points to the fact that some other agency than increased depth is the true cause of the diminution of the temperature under consideration. Accordingly, the writer some time ago instituted a series of experiments with incandescent cast-iron spheres, for the purpose of ascertaining practically if the reduction of temperature could be accounted for solely on the ground that the obliquity of the rays diminishes their energy. Previous experiments had demonstrated that the accepted doctrine is quite incorrect, which teaches that heat rays emanating from the surface of incandescent radiators are projected with equal energy in all directions. It was found during those

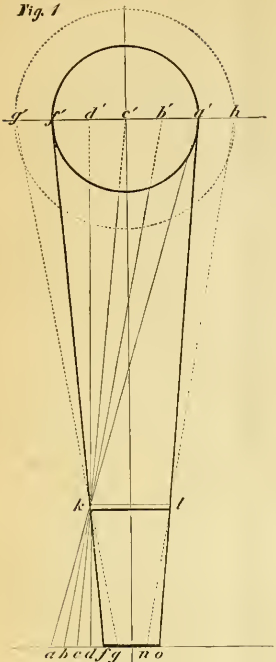


Fig. 1

Fig. 2

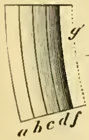


Fig. 3

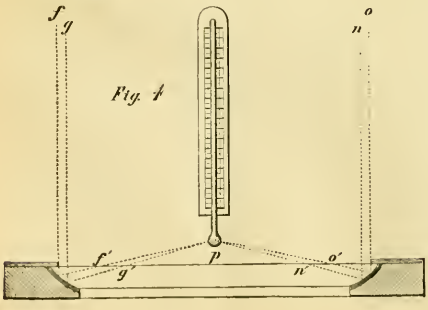


Fig. 4

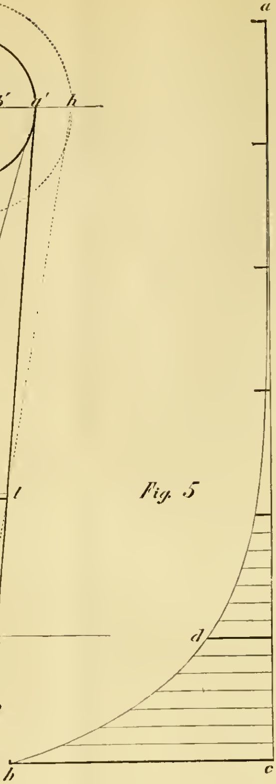


Fig. 5

experiments that the ratio of diminution of radiant heat transmitted to a stationary thermometer by an incandescent circular disc of cast-iron, turning on appropriate journals, is directly proportional to the sines of the angles formed by the face of the disc and lines drawn to the centre of the bulb of the stationary thermometer. It was clearly shown that those heat rays only which are projected at right angles to the face of the incandescent radiator, transmit maximum energy. The important bearing of this fact with reference to temperature transmitted by the heat rays of the photosphere from points near the border, is self-evident. The small angle formed by the ray *c g*, Fig. 6, and the tangent *c f* of the surface of the photosphere at *c*, explains satisfactorily why the radiant heat at a distance of $\frac{1}{20}$ of the radius from the sun's border, is considerably less than at the centre. It will be perceived that the angle *f c d* diminishes very rapidly as the border of the photosphere is approached, and that when the extreme point is reached, the radiant

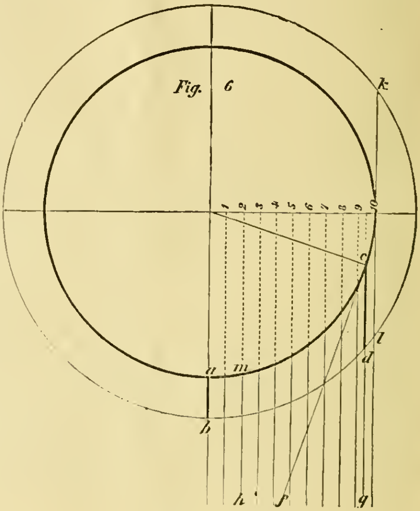


Fig. 6

*a b* = 1·0000  *c d* = 2·0012

heat transmitted would be infinitesimal if the irregularity of the surface of the photosphere did not present a series of inclined planes capable of projecting heat rays in a direct line with *k l*.

Laplace, in the famous demonstration by which he proves that "if the sun were stripped of its atmosphere, it would appear twelve times as luminous" (Mecanique céleste, tom iv., pp. 284—288), commits the grave mistake of assuming that all rays emanating from a radiant surface possess equal energy. This assumption leads him further to the erroneous conclusion that the rays projected from the retreating surface of the sun near the limb, act as rays from a lens, being crowded together in consequence of the obliquity of the radiant surface, thereby, he supposes, acquiring increased intensity—hence the monstrous assertion of the great mathematician that, but for the interference of the solar atmosphere, the luminosity would be eleven times more intense.

The important question whether the solar atmosphere possesses any appreciable radiant power, and whether the high temperature of the attenuated matter of which it is composed exercises any marked influence on the sun's radiant energy, may unquestionably be answered practically. An investigation, based on the expedient of concentrating the heat rays of the chromosphere by means of a parabolic reflector, has been conducted by the writer for some time. The method adopted is such that only the heat rays, if such there be, from the chromosphere and exterior solar envelope, are reflected; while the rays from the photosphere are effectually shut out. Fig. 1 shows the general arrangement; $f'$ $a'$ represents the photosphere, and $g'$ $h$ the boundary of the surrounding atmosphere; $k$ $l$ is a circular screen exactly 10 inches in diameter, placed 53·76 inches above the base line $a$ $o$. This distance obviously varies considerably with the seasons. Assuming that the investigation takes place when the sun subtends an angle of 32′ 1″, the screen $k$ $l$, if placed at the distance mentioned, will throw a shadow, $f$ $o$, exactly 9·5 inches diameter; hence objects in the plane $a$ $o$ placed within $f$ $o$, will be effectually shut out from the rays projected by the photosphere, while they will be fully exposed to the rays, if any, emanating from the chromosphere and outer strata of the solar envelope. It should be observed that, owing to diffraction in connection with the extreme feebleness of the sun's rays projected from the border, the shadow thrown by the screen $k$ $l$ extends considerably beyond the circular area defined by $f$ $o$. Fig. 3 exhibits a *full size* segment of this shadow as it appears round $f$ $o$, the section coloured black in Fig. 2 being a photometric representation of the strength of the said shadow from $f$ to $a$. Special attention is called to this photometric representation, as it shows that objects placed within the circular area defined by $f$ $o$ are absolutely screened from the rays of the photosphere. It is evident that a parabolic reflector of proper size placed immediately below $f$ $o$, will concentrate the radiant heat, if any, transmitted by the rays $f'$ $f$ and $g'$ $g$ and the intermediate rays. Fig. 4 represents a section of the parabolic reflector which has been employed during the investigation. It consists of a solid wrought-iron ring lined with silver on the inside, turned to exact form and highly polished. An annular plate 9·5 inches internal diameter, is secured to the top of the wrought iron ring to prevent effectually any rays from the photosphere reaching the reflector. The prolongation of the rays $f'$ $f$ - $g'$ $g$ and $h$ $n$ - $a'$ $o$ are shown by dotted lines $f$, $g$ and $n$, $o$; also the reflected rays directed towards the bulb of the focal thermometer, marked respectively $f'$, $o'$ and $g'$, $n'$. The investigation not being yet concluded, the following brief account is deemed sufficient at present. Turning the reflector towards the sun, without applying the screen $k$ $l$, a narrow zone of dazzling white light is produced on the black bulb of the focal thermometer, the mercurial column commencing to rise the moment the rays strike the reflecting surface. With a perfectly clear sky, the column during an experiment on August 29, 1871, reached 320° Fah. in thirty-five seconds. The screen $k$ $l$ being applied, after cooling the thermometer, a zone of feeble grey light appeared on the black bulb nearly as wide as the one produced by the rays from the photosphere, but situated somewhat lower. The column of the focal thermometer, however, remained stationary, excepting the oscillation which always takes place when a thermometer is subjected to the influence of the currents of air unavoidable in a place exposed to a powerful sun. It is proper to remark that owing to the stated oscillation, it cannot be positively asserted that there was no heating whatever produced by the reflection and concentration of the rays which formed the zone of grey light adverted to. But the recorded oscillations prove absolutely that the heating did not *exceed* 0·5° Fah. Assuming that such a temperature was actually produced by the reflected concentrated heat emanating from the solar envelope, the following calculation will show that the energy thereby established is too insignificant to exercise any appreciable influence on the sun's radiant power. Theoretically, the temperature transmitted to the bulb of the focal thermometer by the rays $f$ and $o$, Fig. 4, is inversely as the foreshortened illuminated area of the reflector to the zone of light produced on the bulb. Obviously these areas bear nearly the same relation to each other as the squares of $f'$ or $o'$ to the square of the radius of the bulb $p$. The length of $f'$ being 4·77in., while the radius of the bulb is 0·125in.; calculation shows that the temperature transmitted by the ray $f$ would be increased 1,456 times if the reflector did not absorb any heat. Allowing that 0·72 of the heat is reflected, the augmentation of intensity by concentration will amount to $0·72 \times 1,456 = 1,048$ times the temperature transmitted by the rays $f$ and $o$. The records of the oscillations of the mercurial column during the experiments show, as stated, that the temperature resulting from concentration cannot exceed 0·5°, hence the temperature transmitted by the rays emanating from the heated matter of the solar envelope will only amount to $\frac{1}{2 \times 1084} =$ 0·00047° Fah. The observations having been made when the sun's zenith distance was 32° 15′, a correction for loss occasioned by retardation amounting to 0·26 will, however, be necessary. This correction being made, it will be found that the heat actually transmitted by the rays from the solar envelope during the experiment of August 29, did not exceed 0·00059° Fah., a fact which completely disposes of Secchi's remarkable assumption that the high temperature of the photosphere is owing to the "radiation received from all the transparent strata of the solar envelope" (see his letter to NATURE, published June 1, 1871). But we are not discussing the cause; the degree of temperature at the surface of the photosphere is the problem to be solved.

It was stated in the previous article that the radiant power of incandescent metals and metals coated with lamp-black and maintained at boiling heat, is directly proportional to the temperature of the radiator. A series of experiments with flames just concluded, proves positively that under similar conditions a given area of flame of uniform intensity transmits the same temperature as incandescent cast-iron. Secchi's assertion, therefore, that the photosphere, if composed of incandescent gases, "may have a very high temperature and yet radiate but very little," is wholly untenable. The diminution of intensity attending the passage of the heat rays from the photosphere through the surrounding atmosphere, is the only point which can materially affect the question of temperature. We have shown that on a given area, the quantity of matter contained in the solar atmosphere cannot greatly exceed that of the terrestrial atmosphere; hence the retardation cannot be great. True, the depth of the solar envelope is vast compared with that of the earth's atmosphere, but distance *per se* does not affect the propagation of radiant heat. Admitting, however, the retardation to be as the cube root of the depth—the ratio observed in the terrestrial atmosphere—it will be found that the loss of energy produced by retardation of the heat rays is not important. The solar atmosphere being $\frac{100,000}{42} = 2381$ times deeper than the earth's atmosphere, the retardation caused by the former will be 13·3 times greater than that of the terrestrial atmosphere, which, as we know, diminishes the radiant intensity 17·64° on the ecliptic. Accordingly we are justified in asserting that $13·3 \times 17·64° = 234·6°$ Fah. will be the greatest possible diminution of temperature caused by the retarding influence of the matter composing the solar envelope. The admission in the previous article, that the retardation under consideration might be 0·01, was based on the extreme assumption that the obstruction is directly proportional to the depth of the sun's atmosphere. At first sight the loss of 234·6° appears to be a trifling reduction of energy; yet if we consider the mechanical equivalent which it represents, we cannot doubt its adequacy to supply the motive force expended in producing the observed movement of the attenuated matter within the solar atmosphere. Dividing the temperature of the photosphere, 4,035,000°, by 234·6, it will be found that the computed, apparently insignificant, retardation exceeds $\frac{1}{17,000}$ of the entire dynamic energy developed by the sun—an amount fully 15,500 times greater than the solar energy transmitted to all the planets of our system! Making due allowance for the extreme attenuation, and the small quantity of matter to be moved, the most exaggerated computation of the probable expenditure of mechanical energy called for in keeping up the currents of the solar atmosphere, fails to establish an amount at all equal to that capable of being generated by utilising 234° of the radiant heat emanating from the photosphere.

In view of the foregoing statements and the demonstrations contained in the previous article on solar heat, we cannot consistently refuse to accept the conclusion, that the temperature at the surface of the photosphere is very nearly 4,036,000° Fah. J. ERICSSON

## *NOTES*

THE Regius Professor of Physic at Cambridge (Dr. Bond) has issued a schedule of lectures on subjects connected with the study of medicine which will be delivered during the Academical year 1871-2. The following are the arrangements for this Term: Prof. Liveing will lecture on practical chemistry on Tuesdays, Thursdays, and Saturdays, at 1 P.M., commencing October 10. The Linacre lecturer will deliver a course of medical clinical lectures on Fridays, at 10 A.M., commencing October 13. The Professor of Anatomy (Dr. Humphry) will lecture on practical anatomy on Mondays, Wednesdays, and Fridays, at 6 P.M., commencing October 16. Mr. C. Lestourgeon, M.A., will on October 19 commence a course of surgical clinical lectures, and will continue the same on each Thursday during Term, at 11 A.M. Anatomy and Physiology will be the subject of a course by the Professor of Anatomy, commencing October 21, at 1 P.M., and continued on Tuesdays, Thursdays, and Saturdays at the same hour. The Professor of Zoology and Comparative Anatomy (Mr. A. Newton) will lecture on those subjects on Mondays, Wednesdays, and Fridays, at 1 P.M., commencing October 23. Special departments in chemistry will be the subject of lectures by the professor of that faculty on Tuesdays, Thursdays, and Saturdays at noon, commencing October 26. Practical histology will form a separate course under the superintendence of Dr. Humphry, commencing October 28 at 11.30 A.M., and continued each succeeding Saturday until its completion.

THE Franklin Institute of Philadelphia announces the following synopsis of lectures for 1871-72. The regular course will comprise a series of forty lectures, divided as follows:—1. "On Physics and Mechanics," by John G. Moore, M.S. 2. "On General Physics and Acoustics," by Prof. Edwin J. Houston. 3. "On Guns, Gunpowder, and Projectiles," by Lieut. C. E. Dutton. 4. "On the Chemistry of the Earth and of the Vital Process in Animals and Plants," by Prof. Samuel B. Howell, M.D. 5. "On the History of Alchemy," by William H. Wahl, Ph.D. 6. "On the Metallurgy of Iron and Steel," by Thos. M. Brown, Ph.D. Besides the lectures enumerated, the Institute has arranged with a number of eminent lecturers for the delivery of a popular course of scientific subjects, and it is believed that the plan here indicated, of offering a series of lectures brilliantly and largely illustrated, will go far towards attracting the attention and interest of the public to these most important subjects.

THE Managers of the London Institution, Finsbury Circus, announce the following programme of lecture arrangements for the coming season. The courses of educational lectures will be as follows:—First course, commencing Monday, October 30: Eight lectures "On Elementary Physiology," by Prof. Huxley. Second Course, commencing Monday, January 15, 1872: Eight Lectures "On Elementary Chemistry," by Prof. Odling. Third Course, commencing Monday, March 11, 1872: Six lectures "On Elementary Music," by Prof. Ella, director of the Musical Union. Fourth Course, commencing Monday, April 29, 1872: Six Lectures "On Elementary Botany; with special reference to the Classification of Plants," by Prof. Bentley. A Course of Four Lectures, adapted to a juvenile auditory, "On the Philosophy of Magic," by Mr. J. C. Brough, principal librarian in the London Institution, will be commenced on Thursday, December 21. A Course of Two Lectures "On Science and Commerce; illustrated by the Raw Materials of our Manufactures," by Mr. P. L. Simmonds, will be commenced on Thursday, November 23. This course will be illustrated by a large collection of beautiful and interesting specimens of animal and vegetable products. The following lectures will probably be delivered at the Conversazioni of the coming season:—"The Teachings of the Spectroscope," by Dr. William Huggins; "The Homing, or Carrier Pigeon; its Natural History, Training, and Exploits," by Mr. W. B. Tegetmeier; "The Sun," by Mr. J. Norman Lockyer; "Two Years' Gleanings in Syria and Palestine," by Captain Richard F. Burton; "The Haunts of Old Londoners," by Mr. Thomas Archer; "On Colour," by Prof. Barff. The evening class for elementary chemical analysis will commence work, under the direction of Prof. H. E. Armstrong, on Tuesday, November 7.

IN his address at the recent opening of the new Mechanics' Institute at Bradford, Mr. W. E. Forster, M.P., remarked that when institutions of this kind were first established they were intended to give to mechanics scientific knowledge; but it was discovered that that was impossible, except in rare cases, because mechanics had no elementrary teaching on which could be grounded scientific knowledge, and consequently these institutes were obliged to be turned very much into elementary schools and night schools, rather than into the teaching of science and higher literature, which we had hoped to give to our mechanics. A conviction, however, is now gaining ground that an essential portion of this elementary teaching consists of instruction in the rudiments of science, which would be of material advantage to none more than to the working classes.

THE open Scholarship in Natural Science, established this year at St. Mary's Hospital, has been gained by Mr. E. J. Edwards. This Scholarship is worth 40*l.* a year for three years. The Exhibition of 20*l.*, awarded at the same time, has been gained by Mr. Giles. Both gentlemen are students at the University of London.

THE Ettles Scholarship at the University of Edinburgh, which is annually awarded to the most distinguished graduate, has been given to Dr. Urban Pritchard, a student of King's College, London. Dr. Pritchard also gained a gold medal for original researches on the structure of the organ of Corti, conducted by him in the physiological laboratory of King's College.

THE vacancy in the Botanical Department of the British Mseuum, caused by the promotion of Mr. Carruthers, has been filled by the appointment of Mr. James Britten, late assistant in the Royal Herbarium, Kew.

MR. ROBERT ROUTLEDGE, a scientific graduate in honours of the University of London, has been appointed conductor of the classes in Chemistry and Physical Science at the Manchester Mechanics' Institute. These classes are intended to encourage technical education among the working classes, and consist of courses on applied mechanics, steam and the steam-engines, acoustics, light and heat, magnetism and electricity, inorganic chemistry, and practical chemistry, held in the evening, and fully illustrated by experiments, diagrams, and models. The fees for members of the institution are, with the exception of the class of Practical Chemistry, one shilling per session.

WE regret to hear from German advices, of the death of Prof. Schweigger-Seidel, of Leipzig, assistant Professor in Histology to Prof. Ludwig. Prof. Schweigger-Seidel was well known for his careful and accurate researches on several difficult points of histology, especially connected with nerve-endings in the salivary glands, the lymphatic system, and the cornea.

THE *Geological Magazine* records the death, at the age of thirty, of Dr. Georg Justin Carl Urban Schloenbach, Professor of Geology of the Polytechnic Institute of Prague. Previously to receiving this appointment, Dr. Schloenbach had resided in Vienna, where he was an active and energetic member of the k. k. Geol. Reichsanstalt. It was whilst engaged for this Institute, travelling in Servia, that his constitution broke down, under the tremendous fatigue which geologists in these parts have sometimes to undergo. Camping out in what is by no means a tropical latitude brought on rheumatism, and shortly afterwards congestion of the lungs ended his life, after a painful but short illness.

GOOD progress is reported from the Hartley Institute, Southampton, both the day and evening classes being in a very flourishing condition. During the past year as many as 420 students attended these classes. As Science forms a large proportion of the instruction given, there can be but little doubt that the value of the technical knowledge so disseminated will be very great.

THE next Actonian Prize or prizes offered by the Royal Institution, will be awarded in the year 1872 to an essay or essays illustrative of the wisdom and beneficence of the Almighty. The subject is "The Theory of the Evolution of Living Things." The prize fund is two hundred guineas, and it will be awarded as a single prize, or in sums of not less than one hundred guineas each, or withheld altogether, as the managers in their judgment shall think proper. Competitors for the prize are requested to send their essays to the Royal Institution, Albemarle Street, on or before June 30, 1872, addressed to the secretary, and the adjudication will be made by the managers in December 1872.

THE First Commissioner of Works and Public Buildings announces that he intends again to distribute this autumn, among the working classes and the poor inhabitants of London, the surplus bedding-out plants in Battersea, Hyde, the Regent's, and Victoria Parks, and in the Royal Gardens, Kew. If the clergy, school committees, and others interested, will make application to the superintendents of the parks nearest to their respective parishes, or to the director of the Royal Gardens, Kew, in the cases of persons residing in that neighbourhood, they will receive early intimation of the number of plants that can be allotted to each applicant, and of the time and manner of their distribution.

A ROYAL Commission has been appointed at Melbourne for Foreign Industries and Forests, the members being the Hon. S. H. Bindon, Chairman; the Hon. G. W. Cole, M.L.C.; the Hon. R. Hope, M.D., M.L.C.; Mr. R. Ramsay, M.P.; Mr. J. F. Levien, M.P.; Mr. W. Witt, M.P.; Mr. T. M. B. Phillips, M.P.; Mr. F. Von Mueller, C.M.G., F.R.S; Mr. Thos. Black, President of the Acclimatisation Society, M.D.; the Rev. J. I. Bleasdale, D.D.; Mr. Paul de Castella; Mr. C. Hodgkinson; Mr. R. Brough Smith, F.G.S.; Mr. John Hood. The objects of the Commission are to consider and report how far it may be practicable to introduce into that country branches of industry which are known to be common and profitable among the farming population of Continental Europe; to specify which of such industries are most suitable to the soil, climate, and circumstances; to report on the best means of promoting their introduction into Victoria; to report how far the labour of persons at the disposal of the State may be advantageously used for that purpose; to further consider and report on the best means of promoting the culture, extension, and preservation of State forests in Victoria; and to report on the introduction of such foreign trees as may be suitable to the climate and useful for industrial purposes.

THE Government of India have resolved to organise a statistical department for the purpose of ascertaining and conserving the internal resources of India. Dr. Hunter will be the first Director-General of this new department.

IT seems hardly credible that no public monument exists in this country to the discoverer of the circulation of the blood This defect is now likely to be remedied, and preliminary steps have been taken at Folkestone, Harvey's birthplace, to mark the tercentenary of his birth by the erection of a suitable public monument. At a meeting convened by influential requisition—the Mayor of Folkestone in the chair—Mr. George Eastes, M.B., with whom the movement originates, read an interesting sketch of his life, labour, and character. Dr. Bateman, Dr. Bowles, and other local gentlemen, moved resolutions appointing a numerous committee, nominating Dr. Bence Jones, F.R.S., treasurer, the Town Clerk of Folkestone and Mr. George Eastes, M.B., London, as honorary secretaries.

AT the last sitting of the French Academy, an important paper was read on the results of M. Pasteur's long and patient researches into the causes and the best mode of extirpating that terrible disease of the silkworm, the *pébrine*. His efforts appear to have been eminently successful in checking the epidemic, by the simple means of destroying the eggs from all moths which can by any possibility have become tainted. The yield of healthy eggs is now again increasing rapidly in the south of France; and in a few years the disease will probably be all but exterminated. It is hoped that when the National Assembly again meets, some public recognition will be made of M. Pasteur's eminent services.

THE *Observer* comments with great justice on the disproportion between the emoluments for divinity, and for legal, mathematical, and classical instruction at Oxford—"While the salaries of five legal professors, in the aggregate, reach 2,000*l.*, those of the Latin and Greek professors reach 1,100*l.*; those of three professors of metaphysics, &c., reach 1,100*l.*; and those of three mathematical professors reach 1,400*l.*—showing an average of about 480*l.* for each professor; the six professors of divinity enjoy the munificent income of upwards of 1,000*l.* a year each, with houses into the bargain." It adds, "That Oxford should pay 6,300*l.* a year for doctrinal divinity, and only 500*l.* a year for Greek, is a quaint anomaly, to say the least." If, however, our contemporary had included statistics of the remuneration for science, it would have strengthened its case considerably.

THE *Journal of Botany* states that a great desideratum in botanical literature is shortly to be supplied. Considerable progress has been made in printing a second edition of Pritzel's "Thesaurus Literaturæ Botanicæ," a catalogue of all works ever published in all departments of botanical literature, now twenty years old.

WE have received from Mr. Marshall Hall a history of the cruise of the *Norna*, giving in a pleasant chatty form the main results of the expedition as they would interest the public at large. The more important zoological details will be found in another column.

WE are glad to observe that the conductors of the *Scottish*

*Naturalist* are able to announce that with the next number the size of the magazine will be increased to 40 pages. Several important and interesting contributions are announced for 1872; and we hope that this useful magazine will meet with the support and circulation that it deserves.

PROF. J. LAWRENCE SMITH, in the September number of the *American Journal of Science*, gives the following analysis of the meteorite stone which fell near Searsmont, Maine, on the 21st of May of this year:—

| | |
|---|---|
| Nickeliferous Iron . . . . . . . | 14·63 |
| Magnetic Pyrites . . . . . . . . | 3·06 |
| Olivine . . . . . . . . . . . | 43·04 |
| Bronzite, a hornblende with a little albite or orthoclase and chrome iron . . . . | 39·27 |

IT is stated that a crater of a new volcano has been formed on the mountain near Bivoria in the province of Girgenti in Sicily.

THE cyclone which visited St. Thomas and Antigua on the 21st of August, continued its course towards the Bahamas, and reached Turks Island on the 22nd. The storm occupied about eight hours in travelling from St. Kitts to St. Thomas, 150 miles, and so had a rate of progress of about 18¾ miles per hour, but from St. Thomas to Turks Island the velocity decreased to about 12¼ miles per hour, taking about 31 hours to travel 380 miles.

A SLIGHT shock of earthquake was felt at Kingston, Jamaica, at 4 P.M. on the 3rd of September.

THE star showers of the 10th and 11th of August last were attentively watched in America as in Europe. At Sherburne, New York, according to the American *Journal of Science*, a party of six persons watched between 11.40 and 12, and saw 48 meteors. In the next hour 143 were seen, and in the first eighteen minutes of the next hour 32. The latitude of the radiant point was 1½° less than that of the nebula in Perseus.

*Les Mondes* gives the particulars of a remarkable meteorite observed at Marseilles by M. Coggia, on the 1st of August. It made its appearance at 10h. 43m., Marseilles mean time, at a point situated near the centre of the triangle formed by $\zeta$ Serpentis and $\theta$ and $\eta$ Ophiuchi. The course was remarkably slow, in an easterly direction; at 10h. 45m. 30s. it passed between $\mu_1$ and $\mu_2$ Sagittarii, and at 10h. 46m. 35s. it almost occulted Saturn. The course became then still slower; at 10h. 49m. 50s. it passed a little below $o$ Sagittarii, and at 10h. 50m. 40s. south of the star $f$ of the same constellation. At 10h. 52m. 30s. it passed between $\iota$ and $\theta$ Capricorni, where it remained for a moment stationary, then changing its course, it took a northerly direction, leaving at 10h. 57m. 50s. the star $\nu$ Aquarii 1° 30′ to the west, and again stopping, at 10h. 59m. 30s., a little south-west of $\beta$ Aquarii. Regaining its original easterly direction, it then passed $\beta$ Aquarii, stopping again near $\zeta$ Aquarii, and then fell rapidly in a perpendicular direction near $\delta$ Capricorni, and leaving to the east the almost full moon. It finally disappeared a little north of $\theta$ Pisc. austral. at 11h. 3m. 28s. The diameter, which was at first about 15′, diminished rapidly, was a little over 4′ when it approached Saturn, and finally had scarcely more than the apparent size of Venus. During its perpendicular fall to the horizon, it gave out vivid scintillations.

THE *Times of India* gives the following story:—Advices from Ihangara state that at a place about forty miles distant on the hills, a thunderbolt fell on the 22nd of August after a heavy downpour of rain. The ground was literally cut up in consequence, and the whole of the huts standing there as well as their inmates were swallowed up in the chasm. Such a catastrophe has never been known in Sind. Some fifty or sixty persons perished.

ON the 11th of July a strong shock of earthquake was felt at Valparaiso in Chile, preceded by a loud rumbling noise. On the 20th, at 11 P.M., a very severe shock was felt at Santiago de Chile.

THE following account of a hairy family appears in the *Indian Daily News*:—"The hairy family of Mandalay consists of a woman of about forty-five years of age, a man of twenty, and a girl of eleven, with hair over every part of their faces, forehead, nose, and chin, varying in length from three inches to a foot, and exactly the colour and texture of that on a skye-terrier. The hair of their heads, on the contrary, is just the same as on any ordinary Burman; they appear to be quite as intelligent as the ordinary Burmans. The father of the woman was the first of the hairy progeny. He married an ordinary Burman woman, and the issue of the union was the present hairy head of the family. She married an ordinary Burman, and has issue, a son about twenty-three years of age, not hairy, and the boy and girl alluded to. The Burmese explanation of the phenomenon is, to say the least, curious, and might possibly possess a special interest for Mr. Darwin. These hairy people would be worth a fortune to the enterprising Barnum if he could get hold of them, but the King will not allow them to go out of his dominions."

## SCIENTIFIC INTELLIGENCE FROM AMERICA *

THE fourth Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology has made its appearance, and presents a gratifying picture of the progress of this great establishment. The most important additions during the year have been a collection of stone implements from Cape Cod presented by Mr. Samuel H. Russell, a series of duplicates from the Christie collection of London, and specimens obtained from explorations in Tennessee by Mr. Dunning, and in Central America by Dr. Berendt. These are supplemented by numerous single donations of greater or less value. In the course of some critical observations upon the specimens received by the Museum, attention is called to the great value of a collection of crania and human bones obtained from certain mounds in Kentucky by Mr. S. S. Lyon, in the course of explorations made under the combined auspices of the Smithsonian Institution and of the Peabody Museum. The peculiarities of the crania of the American Indians have already been referred to by various writers, but some curious facts are detailed in the report in regard to other portions of the skeleton. Thus the ulna and radius, as compared with the humerus, were found to be much larger in the mound Indians, while the length of the tibia, as compared with the femur, is longer in the whites. In quite an unusual number of Indian skeletons the two fossæ at the lower end of the humerus were found to communicate, producing a perforation. This feature, rarely met with in the white races, occurs quite frequently in the mound remains, while in the black race it appears to be still more frequent. An additional peculiarity of the mound bones consists in the flattening of the tibia, which, until the date of the present publication, has not been recorded as occurring in America, although remains from the dolmens of France, the quaternary drift of Clichy, and the burial caves of Cro-Magnon and Gibraltar, exhibit this in a very marked degree. As regards the pelvis, the breadth in the Indian races is found to be less than in the whites, while the three diameters of the brim of the true pelvis are greatest in the Indians. The transverse diameter and the size of the outlet of the pelvis are much the largest in the Indian, while the sacrum is less curved, supplying conditions which in the process of parturition are more favourable to the Indian women.—We have already referred at various times to enterprises on the part of the Peruvian Government in exploring the less-known portions of that country, and we find in late South American journals details of a movement looking toward the examination of the regions of the Ucayale and Urubamba. The object of the expedition is to find a port which will open up to the Department of Cuzco a communication with the main branch of the Amazon, and thence to the Atlantic. The work is to be under the direction of Mr. Tucker, favourably known in similar enter-

* Communicated by the Scientific Editor of *Harper's Weekly*.

prises before. The present plan is for Don Raymundo Estrella and another commissioner to start from the port of Illapani in two large canoes, and make their way by the Urubamba to Iquitos, which is the Peruvian naval station on the Amazon. This is for the purpose of obtaining such a knowledge of the rivers as may fit them to serve as pilots to the steamer which is to ascend the Ucayale and explore the Urubamba. They are to make their way back about thirty leagues from Cuzco.—The daily papers of August 29 contain the latest reports from Captain Hall and his steamer *Polaris*, in the form of a telegraphic despatch from the United States ship *Congress*, dated at St. John's, Newfoundland, August 28. It will be remembered that this vessel was detailed by the Secretary of the Navy to carry supplies of provisions and coal to be stored in Greenland for the use of the Arctic expedition. She left St. John's on her outward trip on the 3rd of August, reaching Disco on the 10th, passing hundreds of immense icebergs on the way. The *Polaris* was found at Disco, having reached that place only six days in advance, although she started long before the *Congress*. Captain Hall and his party were in good spirits, and sanguine of success. The *Congress* reports that Captain Hall left Disco on the 17th of August for the north, where communication with him will, of course, be uncertain for some time to come, unless the object of the expedition in reaching the north pole can be accomplished in time to return during the present year. It is understood that instead of going by way of Jones Sound, as was the original intention, Captain Hall will proceed along the eastern side of Smith Sound. By all accounts the water is much more open than for many years past, there being comparatively little drift-ice to bar progress. To the surprise of the officers of the *Congress*, the summer temperature of Greenland was found to be quite elevated, and there was a luxuriant vegetation to be seen around the settlement of Disco.—The Panama papers speak of the great success which several whaling ships are now meeting with in the Bay of Panama, quite a number of whales having been killed there every day for some time past. It is stated that at the time the steamship *Chile* passed Payta, a school of small whales had been there in such abundance that the boats were afraid to leave the harbour.—We have already referred to the hydrographical and other explorations in Alaska by Mr. William H. Dall, under the patronage of the Coast Survey; and we now learn that he left San Francisco for the north at the end of August, bound direct to Iliuliuk Harbour, Oonalaska, there to go into winter-quarters. It was his intention, according to his instructions, to make use of every favourable opportunity to survey the vicinity of that port, and in March to proceed westward, sounding and surveying as far as Kamtchatka, and then turning north and eastward to Cape Romanzoff, to return to Oonalaska, and thence proceed homeward. The vessel obtained for the expedition, although small, is conveniently adapted for its purpose, and can carry provisions for six months; and it is expected that fresh supplies will be forwarded from San Francisco in March next. The party, besides Mr. Dall, consists of Prof. Harrington, the astronomer, Captain W. G. Hall, sailing-master, with two mates and five men.

## ON THE STUDY OF SCIENCE IN SCHOOLS *

### II.

WE now come to the second heading of our discourse, viz., the objects and aims of the experimental sciences, and the reason why we study them. Now the main object of science is the discovery of new truths, and the destruction of old errors. The human mind, much as it loves truth, has in the course of ages given birth to an infinite number of fallacies, specially in regard to the operations of Nature. Fallacies handed down by tradition; fallacies elaborated in the mind of dreamers, and theorists, and believers in magic; fallacies founded upon inaccurate observation, false experiment, perverted reasoning; these have ever been the barriers which have most retarded the progress of true science; and the earlier natural philosophers had to contend against a mass of such pre-existent opinion and superstition. We can scarcely realise in the present day the amount of superstition which existed among all classes even two hundred years ago, and at an earlier period it was far more prevalent. That same Athanasius Kircher, who was before mentioned as the author of a book on light, and who also wrote on magnetism, gives a detailed account of an encounter with a dragon in one of the passes of the Alps, and illustrates his assertion by an exceedingly bold and imaginative woodcut. Metals were believed to be generated in the earth by the action of the sun. Gold had a large proportion of condensed sunbeams. A mine when exhausted was closed, and re-opened after some years in the hope that the metal would have been produced in the meanwhile. Many—among them Cardanus—believed that metals and minerals possessed a kind of life, and that certain changes in them, such as conversion into calx, were the result of their death. The air was peopled with invisible demons, who wrought all kinds of mischief, raised storms and whirlwinds, and warred against the works of man. Witches and wizards were in league with them, and could influence them, and were hence treated with extreme severity. In 1487 there was an unusually devastating storm in Switzerland, and two old women, who were believed to be witches, were arrested on the charge of having caused it. They of course denied the charge, but during the torment of the rack they confesssed they had raised the tempest. They were forthwith executed—"Convicta et combusta." These cases were by no means rare. Witches were believed to exist by the hundred and thousand, and to produce all kinds of supernatural effects. Pope Innocent VIII. issued a manifesto against them in 1488, and appointed inquisitors in all countries, armed with powers of arresting and punishing suspected sorcerers. In Geneva alone, no less than 500 persons were burned in 1515 and 1516. So late as the year 1716, two persons were executed in England for the practice of witchcraft. We can understand all this better if we bear in mind how much superstition still exists in the world. Not to mention those things which appear under pseudo-scientific names, we find in many out-of-the-way villages, specially in Ireland, a very firm belief among the uneducated in the power of charms, and the existence of witches. In a village not far removed from the outer world, a witch has been pointed out to me, and the laming of a horse and other disasters seriously attributed to her charge. Gaule, in his "Magastromancer," gives a list of *fifty-two* forms of divination, and he has omitted at least six which are found in the works of other writers. Among other forms we have divining by ashes, by smoke, by the lees of wine, by cheese, by figs, by knives and saws; you will remember also some of the forms of divination practised by the Romans. But perhaps the delusion which has most militated against the growth and progress of true natural science has been alchemy—a false science which flourished for more than 800 years, and which was firmly believed in by thousands. The alchemists devoted their lives mainly to the search for two palpable impossibilities; the Elixir Vitæ, which was believed to possess the power of conferring perpetual youth, and the Philosopher's Stone, which was believed to transmute everything that it touched into gold. The search for this substance, and the endeavours to make it by artificial means, occupied the attention of many notorious and eminent men. Albertus Magnus, who became Bishop of Ratisbon in 1259, and S. Thomas Aquinas, were particularly addicted to alchemy and magic. We hear most of their magical powers, although their writings on alchemy still remain. Between them they made a brazen statue and endowed it with the faculty of speech; but it was so garrulous that one day Thomas Aquinas, who was in vain trying to work out a mathematical problem, seized a hammer and destroyed it—at least, so say contemporary writers. Albertus Magnus once changed a severe winter into a most splendid summer within the space of his garden. Detailed accounts exist of the transmutation of lead and tin into gold. Raymond Lully states in one of his works that he converted 50,000 lbs. weight of quicksilver, lead, and pewter into gold. Pope John XXII. was a great alchemist, and had a laboratory at Avignon. He wrote a work on the transmutation of metals, and at his death left a sum of eighteen millions of florins, the existence of which according to contemporary alchemists, proved the possibility of transmutation. And thus one might continue to give a long list of known men who devoted themselves to these useless pursuits; and the unknown men could be counted by thousands. Here, then, we have some of the fallacies which it has been the object of science to disprove, and which, so long as they existed in full vigour, effectually prevented the progress of science. The disproval of these could only result in the discovery of new truths. There is an intense satisfaction in the discovery of absolute truth; truth which stands every opposition, which has been weighed in many balances and not found wanting; which has been submitted to every process of reasoning and of experiment, and has come out uninjured. Taking this discovery of new

* Conclusion of a Lecture delivered at Marlborough College as an introduction to the commencment of Science teaching, by G. F. Rodwell.

truths as the first and greatest aim of science, we may, perhaps, take next some of Francis Bacon's more practical ideas about the objects and aims of science ; to increase man's sovereignty over Nature, to compel Nature to be subservient to his will, and to minister to his wants ; to restore his lost sovereignty over Creation. And, indeed, when the new truths are discovered, they are soon applied to practical purposes, and to furthering the material good of mankind ; but to study science with this object alone is usually pernicious, and always to be avoided.

Some of you will ask me the more direct use of science. I fear I cannot tell you much about this ; I would rather refer you to some of the enthusiastic—I hope not exaggerated—articles which have appeared from time to time during the last few years in various journals and magazines. It is directly useful for the purpose of science scholarships at the Universities, which are much on the increase ; also it forms a part of the examinations at Woolwich, and for the Civil Service. Scientific appointments are year by year becoming more numerous in this country and in India. Indirectly, science is useful to every one. I say I cannot tell you much about its direct and practical uses, because I believe that the main use of it is to cultivate a certain set of mental faculties, to induce a certain mode of thought. The modes, and tones, and phases of mental action are as diverse as the modes of bodily action, and just as we exercise one set of muscles by rowing, another by riding, and a third by walking, so do we exercise a certain set of faculties when we study classics, another set when we study mathematics, and a third when we study sciences. The cultivation of this habit of thought engenders among other things a habit of observation and a spirit of inquiry. Questions suggest themselves daily, for an answer to which we must apply to science. Why do winds blow and storms rage? What are day and night, summer and winter, sunshine and frost? Of certain common things we rarely think, or if we do we assign the simplest meaning to them. For how many centuries did not mankind believe the world to be flat, the sun to be a globe of fire quenched nightly in the western sea, the sky to be solid, and the stars set into it like gems! Savages still believe that the firmament is a solid dome, and the sun and moon living creatures who walk across it.

The third of our four divisions concerns the methods we shall follow in our study of the sciences discussed above. Firstly :—lectures. It is essential that you should see the various changes wrought upon or within matter ; not alone hear about them or read of them. You must not only observe, but you must think of the experimental results ; understand them ; understand the means by which they are brought about. It will be well for you to take notes, roughly at first, to be copied out afterward, and extended from memory. It is a mistake to take very full notes during a lecture. They may become an almost verbatim report of the lecture ; the spirit of the matter is lost because the mind is fixed upon a detail. Experiments also are often lost ; and at the end a mass of writing remains, but no knowledge of the work done. It is preferable to write down headings of subjects ; the pith and marrow of the subject matter only ;—in a word, to make merely an outline of the picture, and to fill in the details afterwards from memory. Sketches of apparatus are always desired among the notes, also any general remarks, and queries. At the end of each lecture you will be questioned, and at the commencement of each lecture the matter of the preceding lecture will be recapitulated ; at this time also your own queries will be answered. It is important that you should not allow any subject to be partially understood, or misunderstood. Make a note of any difficulty, and let it be cleared up at the commencement of the next lecture, or at some intermediate time. The misunderstanding of one important fact may render the right understanding of succeeding matter nearly impossible. Then, later in the half, I should like you to read in text-books about the subject of your lectures, and thus to supplement the lecture-work by book-work. The advantage of this will be very apparent when you are examined.

What we desire is that science shall grow up side by side with your other subjects of study, and enter into your daily life. It is thus only that it can possess any real vitality. And if any subject of study possesses not vitality—intense, active, exuberant vitality—it languishes, becomes unhealthy, weak, and ultimately useless. It resembles a tree which loses first one branch, then another, and then dies entirely. And when upon the tree of knowledge a new branch is grafted, we desire to see it growing up side by side with the great branches already there. Our school knowledge—the knowledge which in its entirety fulfils the conditions of that comprehensive word *culture*—must be one and undivided ; hence a new subject can only flourish when it is woven completely into our school life, when it ceases to be regarded as a something extraneous and beyond the pale. I hope none of you are like the doctors of Salamanca when they were confronted with Columbus, or like the cardinals who passed judgment upon Jordano Bruno and Galileo.

I must add one word in conclusion as to the attitude of mind most conducive to a right study of natural science. In the first place it is necessary to free the mind from previous ideas and conjectures, and to neglect the evidence of the senses unsupported by extraneous means ; thus the earth seems to be flat, and the sun to be a glowing disc which moves around it, yet research has proved that our senses here deceive us. Again, how difficult it is to realise the fact that two sounds may produce silence, two lights darkness, until it is experimentally proved that such is the case. It is hard to believe that the force which manifests itself by attracting light bodies when amber is rubbed, is identical with lightning, yet such has been proved to be the fact. We must clear our minds from preconceived opinions before we can profit much by the teachings of science.

Do not be discouraged by the apparent difficulty of science at starting ; all things newly presented to the mind require the exercise of some effort before they can be grasped. If the current of our thoughts is to be diverted into a new channel, it must needs require some time to change it from its old course. Comfort yourselves with the knowledge that at the outset you know more true natural science than did Aristotle and all the great philosophers of antiquity. The very science which you learn almost as soon as you know the alphabet, the fundamental ideas about the earth, the sun, the moon, the air, places you at starting ahead, in the matter of science, of the flower of Middle Age erudition : Professors of the Sorbonne, Doctors of Salamanca, Monsignori of the Sacred College. If, at first, the path of science seems to wind uphill all the way, remember that when the toil is over the view from the summit is very glorious. The sun rises upon a new land infinitely vast, infinitely fertile ; full of streams by the side of which you may wander, and see all nature reflected in their pure depths.

Above all things, I would ask you to study science reverently. Many of our studies concern the works of man, here we are dealing with the works of God, governed directly by His laws. Surely then it behoves us to bow our heads as we enter the portal of Nature, to be possessed of infinite humility, to assume no prying spirit of curiosity, to have no intellectual pride. Some of you no doubt remember Rembrandt's picture of the "Anatomic Lesson," and the calm, reverent, inquiring look of the students who surround the dead man ; a sort of awe in the presence of the wonderful mechanism of the microcosm Man, as we must have awe in the presence of the macrocosm Nature. A something almost akin to the deisedaimonia of the Ancients ; a reverential fear of that which is obscure, and but partly manifest. I know not whether the smaller and more obscure works of God do not convey this even more than those which are immeasurably greater. S. Augustine says, "Deus est magnus in magnis, maximus autem in minimis." We are scarcely more awed by the myriad stars and suns and systems around us than by the myriad atoms of which the smallest mass of matter consists, and which possess functions, attributes, actions, as definite in character, as varied in form, and as absolutely governed by immutable laws, as the members of systems comprising a million worlds, ten million miles away.

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## *ZOOLOGICAL RESULTS OF THE* 1870 *DREDGING EXPEDITION OF THE YACHT "NORNA" OFF THE COAST OF SPAIN AND PORTUGAL* *

AT the last meeting of this Association, held at Liverpool, I exhibited as one of the trophies of the *Norna* Expedition a new silicious sponge, to which I gave the name of *Pheronema Grayi*, or "the Portuguese Bird's-Nest Sponge ;" and on this occasion the following is a brief synopsis of other leading novelties and more general results of the dredging cruise. A few preliminary remarks on the origin and object of the expedition may preface this synopsis.

* Communicated to the Biological Section of the British Association, Edinburgh, August 8, 1871.

To Mr. Marshall Hall, F.G.S., &c., who personally superintended the expedition, are due the thanks of the scientific world for having so generously devoted his yacht *Norna* to the purpose of scientific discovery. This gentleman had early in the year conceived the project of rendering science that service it is to be regretted so few owners of yachts are disposed to contribute; and to him I feel myself under the deepest obligations for the opportunities afforded me during this cruise of acquiring that practical information so keenly appreciated by every working naturalist. Nor must I forget here to associate with his the name of Mr. Henry Lee, F.L.S., the worthy president of the Croydon Microscopical Society, as one of the chief instigators of the scheme, and the person to whom I am especially indebted for my introduction to Mr. Marshall Hall, as one likely to make the most of the opportunities that would be afforded.

Having accepted the last-named gentleman's kind invitation to accompany him as naturalist in a small way to the expedition, it was decided I should memorialise the Council of the Royal Society for a grant to defray the heavier expenses of dredging and collecting apparatus. My application was most favourably received, thanks to the numerous kind scientific friends who supported it, and a sum of 50*l.* was immediately placed at our disposal for the purpose required. My indebtedness to the Royal Society for this liberal assistance has already been acknowledged, though I cannot permit so fit an occasion as the present to pass without once more endorsing it.

By the middle of May everything was prepared, the Trustees of the British Museum, on the especial recommendation of Professor Owen and Mr. Waterhouse, extending me an extra three weeks' leave of absence. The companionship and services of Mr. Edward Fielding were also fortunately secured, whose earlier dredging experiences with Mr. M'Andrew in the Red Sea seemed calculated, as they afterwards proved, to be of the most valuable assistance. Our time being limited, the west coast of Spain and Portugal was decided upon as a locality likely to yield us the most satisfactory zoological results, and on the recommendation of Mr. Henry Woodward we resolved first to proceed to Vigo Bay, where, in company with his lamented brother, Dr. S. P. Woodward, and Mr. M'Andrew, he had in the year 1856 obtained such abundant and valuable material. From thence it was proposed we should work our way down to Lisbon, our particular ambition being to reach the deep-sea fishing ground off Setubal, some twenty miles further south, from whence Prof. du Bocage, the talented conservator of the Lisbon Museum, had obtained specimens of the "Glass Rope Sponge" (*Hyalonema*), and numerous other novel treasures. On starting, we touched and remained a couple of days at Guernsey, and at that spot a few hours spent in shore-collecting rewarded us with the earliest substantial fruits of the expedition; seven more days brought us to Vigo, the point which constituted the first basis of our practical dredging operations.

A detailed list of the numerous species collected throughout the cruise being in course of preparation for the more technical and exhaustive report to be presented to the Royal Society, I here propose, commencing at the lowest animal group, to briefly enumerate some of the more important forms taken, adding such remarks on the characters or connecting circumstances which render them more especially deserving of attention. Of all, the subkingdom of the Protozoa has perhaps furnished us with the most abundant and valuable material, the sponge class in particular contributing many novelties. Before leaving British waters even, the few hours spent in shore-collecting at Guernsey, already alluded to, resulted in the accession of three new species of the genera *Isodictya* and *Hymeniacidon*, which I have placed at the disposal of my kind friend Dr. Bowerbank to be described by him in his supplementary volume of the "British Spongiadæ," now closely approaching completion. The moderate depths within the Laminarian and Coralline zones, from the shore line down to fifty fathoms, at which we collected and dredged in Vigo Bay, and afterwards further south in the neighbourhood of Setubal and the Sado river, proved remarkably productive of species belonging to the same group, as also to that of the Calcarea or calcareous spiculed sponges including *Sycon* and *Grantia*, &c. The most interesting of any, however, were the species belonging to the Hexactinellidæ, or hexradiate spiculed sponges, of which the beautiful *Euplectella* and *Hyalonema* form familiar examples. Nine species belonging to this group were obtained at a depth varying from 400 to 800 fathoms off Cape Espichel and Cezimbra, including *Hyalonema*, *Dactylocalyx*, *Aphrocallistes Bocagii*, *Lanuginella pupa*, and four other species new to science, three out of which necessarily constitute the types of new genera, the residue again furnishing data enabling us better to appreciate the characters and distinctions of those previously made known to us. The form belonging to the same group, and described by myself as *Pheronema Grayi*, and exhibited at the last meeting of this Association, is the most conspicuous among all these on account of its size, and I would here add a few more words in reference to this particular type. Since last year I have been afforded the opportunity of examining and comparing my own with numerous specimens of Prof. Wyville Thomson's *Holtenia Carpenteri* taken in the North Sea and also in the Atlantic, and from an evolutionist's point of view, this examination has led me to regard my specimens as holding rather the rank of a well-marked local variety than of a distinct species as I at first premised. A comparison of the specimens, now placed side by side in the British Museum collection, will, I think, suffice to prove to all those interested in this subject how strongly marked as varieties these two forms are. Meanwhile, the generic name of *Pheronema* adopted by myself I still retain, as I consider both Prof. Wyville Thomson's form and my own to be local varieties of another species first described by Dr. Leidy of Philadelphia as *Pheronema annæ*, and a letter recently received from Dr. Leidy himself more fully convinces me of this, though he has not yet bestowed on it the minute microscopical investigation of its structure needed for the effectual clearing up of this, at present, doubtful point.

In my description of other sponges belonging to this same Hexactinellate group, read before the Royal Microscopical Society, and published in their "Transactions" for November 1870, I have, in creating a new genus and species, *Askonema setubalense*, erroneously associated Prof. Thomson's name with it as having once pronounced the form to be of vegetable and not animal organisation. The mistake arose from the misconception of a name singularly similar in euphony as pronounced to me by Prof. du Bocage, and I here avail myself of the opportunity of rendering Prof. Wyville Thomson that *amende honorable* I feel myself in duty bound to accord to him.

Passing next to the class of the Foraminifera, our gatherings have been remarkably rich both from the coralline and abyssal zones, the latter furnishing us with numerous arenaceous types (*Rhabdomina*, &c.), and the former being notably abundant in species and varieties of *Lagena* and *Cristellaria*. Many of these forms are new to science and await description, and I must not forget to acknowledge here my indebtedness to Mr. Henry Lee for the very great assistance he has rendered me in his skilful preparation of the various gatherings of these minute organisms. To Mr. Henry Hailes also my best thanks are due for similar services.

The Cœlenterate sub-kingdom has likewise furnished several new and rare forms, including among the latter category an example of *Hyalopathes pyramidalis*, M. Edw., one of the Antipathiidæ now represented for the first time in our national collection, if not in this country. In the Alcyonarian group, *Veritillum cynomorium*, first taken sparingly in Vigo Bay, and afterwards abundantly in the Laminarian zone near Setubal, excited our warmest admiration.

Nothing can exceed the beauty of the elegant opaline polypes of this zoophyte when fully expanded, and clustered like flowers on their orange-coloured stalk; a beauty, however, almost equalled by night when, on the slightest irritation, the whole colony glows from one extremity to the other with undulating waves of pale green phosphoric light. A large bucketful of these Alcyonaria was experimentally stirred up one dark evening, and the brilliant luminosity evolved produced a spectacle too brilliant for words to describe. The supporting stem appeared always to be the chief seat of these phosphorescent properties, and from thence the scintillations travelled onwards to the bodies of the polypes themselves. Some of the specimens of this magnificent zoophyte measured as much as ten inches from the proximal to the distal extremity of the supporting stalk, while the individual polypes, when fully exserted, protruded upwards of an inch-and-a-half from this inflated stalk, and measured as much as an inch in the diameter of their expanded tentacular discs.

Numerous polyzoa were also dredged up from the various depths, many of which remain yet to be identified; but the allied group of the Tunicata has perhaps furnished by far the most interesting material of the whole molluscoidan sub-kingdom; surface-skimmings one morning near the mouth of the Sado river having rewarded us with numerous specimens of an *Appendicularia*, which, from notes and sketches made at the time of their capture, I have since found to have presented phenomena seemingly not yet observed by any other naturalist. Hitherto these organisms have been presumed to constitute a distinct genus of

Tunicata *inter se*, or otherwise to be the larval conditions of higher forms. My own observations, however, recorded in the ast July number of the "Quarterly Journal of Microscopical Science," have led me to believe that they are the free swimming reproductive Zooids of higher Tunicates, bearing the same relation to them as many free swimming *Medusæ* do to some stationary hydroid colony. At the greater depth of 600 and 800 fathoms, various species of *Terebratulæ* were taken as representative of the class Brachiopoda.

Ascending yet higher to the subkingdom of the Mollusca, a large variety of interesting species rewarded our researches. Included among these were—*Fusus contrarius*, a common fossil of the Norfolk crag recently discovered in the living state in Vigo Bay by Mr. M'Andrew, and dredged by us in the same locality; also a species of *Cassis*, remarkable from its being more closely allied to *C. Saburon* and other species inhabiting the Japanese and Chinese seas than to any of its Mediterranean or Atlantic congeners. This circumstance of its affinity is the more remarkable when associated with the occurrence of a species of *Hyalonema* (*H. lusitanica*) off the samecoast, likewise scarcely distinguishable from the more familiar Japanese form *H. Sieboldi*.

The Annelida and Crustacea have also furnished a fair quota of new and interesting species, to be reverted to hereafter; and neither taking a step further onwards to the higher vertebrate sub-kingdom has good fortune entirely deserted us Availing ourselves, through the kind assistance of Prof. du Bocage, of the aid of the native fishermen and their appliances, we secured examples of several rare species of the deep-sea ground-sharks frequenting the Portuguese coast line; and among others a fine specimen of *Pseudotriakis microdon*, a species recently discovered and described by Prof. du Bocage and his gifted collaborateur, Felix de Brito Capello.

Generalising from the whole amount of material collected during our cruise off the Iberian coast, our plunder may be separated into two very distinct groups. One of these, including that collected from the shore line down to a depth of 100 fathoms, presenting an interblending of Mediterranean species with those prevalent on our own more temperate coasts. Among these former I may more especially mention the occurrence of *Dendrophyllia ramea*, a well-known Mediterranean branching coral in great luxuriance at the mouth of the river Sado, this being, I think, the first record of this coral being taken so far north, and also from the same locality *Calappa granulata*, *Maia verrucosa*, *Murex trunculus* and *brandaris*, *Cestum veneris*, *Veritillum cynomorium*, and numerous other species belonging to the various Invertebrate divisions usually regarded as confined to the same more southern area. The residue and far smaller assemblage of species embraces those derived from the abyssal depths of from 400 to 800 fathoms, and all these, including many forms new to science, are characterised by their boreal or cold area facies, and in this respect contribute further evidence in support of the deductions arrived at by Dr. Carpenter, from his own more extended researches into the fauna of these same great depths in connection with the important expeditions of the *Porcupine* and *Lightning*, and with which his name and those of his indefatigable colleagues, Prof. Wyville Thomson and Mr. Gwyn Jeffreys, are so worthily connected.

In conclusion, it is my sincere hope that the rich reward attending our own humble efforts may stimulate other yacht owners to follow the example of my esteemed friend, Mr. Marshall Hall, influencing them likewise to devote their craft for one or a portion of a season to the cause of science, and to the exploration of those new deep-sea fields of discovery, now waiting to yield up the richest treasures to each earnest worker. Such men will find themselves more than compensated for the sacrifice of time or other interests by the fascinating nature of the work they undertake, in addition to earning for themselves the lasting gratitude of the scientific world.

Our well-appointed and expensively-fitted-out Government expeditions should explore the remoter depths; but British pluck and private enterprise should esteem it their especial privilege to unfold to us the yet hidden mysteries of the ocean world nearer home; and if, again, all shall not succeed in discovering new phases of animal life, there is much and even more important work to be effected in ascertaining accurately the bathymetrical range and geographical limits and distribution of those forms already known to us.

W. SAVILLE KENT

## PROF. BASTIAN ON THE GERM THEORY*

EPIDEMIC and acute diseases have many characters in common; they constitute a family the members of which are united by a certain bond of unity, though at the same time they are in other respects strikingly different from one another. The "general" character of the symptoms originally gave rise to the notion that these affections were in the main dependent upon changes in the nature and quality of the blood. This view is still the one most commonly entertained, and which seems most likely to be true. And seeing that particular sets of symptoms recur with as much definiteness as individual differences of constitution will permit, we have a right to believe that the changes in the blood—however induced and of whatsoever nature they may be—are definite and peculiar for each of these diseases. The successive changes in the blood which are the immediate causes of the phenomena of small-pox, must be quite different from those giving rise to the morbid state known as typhoid-fever. Variable as these several groups of symptoms are amongst themselves in individual cases, yet is there a general resemblance which suffices to maintain the distinctive nature of each affection. In this broad sense they are undoubtedly entitled to rank as "specific" diseases. They may be presumed to be associated with definite changes in the blood, though we have not a right to infer that such changes of state can only be induced in one way. Many well-known chemical changes are capable of being brought about by more than one agency. And just as there is the best reason for believing that cancer or tubercle may be initiated *de novo* by the operation of irritants upon the tissues of certain individuals, and that such growths may subsequently be multiplied within the body by the contact-influence exerted by some of their disseminated particles; so may we suppose, not only that specific substances (contagia) may be capable of initiating specific changes in the blood, but that certain combinations of circumstances may by their action upon the human body entail similar definite changes and states of blood. Having to do with a perverted nutritive activity and mode of growth in a limited area of tissue, cancer or tubercle may make their appearance; whilst, having an altered nutritive activity and set of changes occurring in the blood, this all-pervading tissue may lapse into the successive states peculiar to one or other of the specific diseases, and so give rise to the symptoms by which they are characterised. This is by no means a forced analogy. Can cancer or tubercle arise in the individual without any pre-existing "hereditary taint"? Can the states of blood peculiar to the several specific diseases arise *de novo*, or independently of contagion? These are questions whose import is really similar.†

One of the great and distinguishing peculiarities of these specific diseases is their "contagiousness." Although very differently marked in the several affections, this property is as interesting as it is important. The fact of its existence seems always to have had a large share in determining the nature of the general views which have been held concerning these affections. Even in remote periods, by Hippocrates and others, they were commonly compared to processes of fermentation; whilst since the time of Linnæus, more especially, attention has been often prominently directed to the many apparent similarities existing between the commencement and spread of epidemic diseases, and the "flight, settlement, and propagation of the insect-swarms which inflict blight upon vegetable life."‡ These

* Extracts from Introductory Lecture on Epidemic and Specific Contagious Diseases: Considerations as to their Nature and Mode of Origin. Delivered at University College, October 2, by H. Charlton Bastian, M.A., M.D., F.R.S.

† This double mode of causation is perfectly familiar to the chemist Particular chemical changes may occur under the influence of certain general determining conditions, which at other times (in the absence of these conditions) may be even more easily initiated by a single specific cause. The introduction of a crystalline fragment into a saline solution, and its determination of the crystallisation of all the isomorphous salts contained in the solution, seems to be exactly comparable with the "contagious" origin of diseases. But, under the influence of certain favouring conditions, crystallisation may occur without the contact of a crystalline fragment—the process may be "spontaneous" in the same sense that the occurrence of the blood-change may be "spontaneous."

‡ Sir H. Holland's "Medical Notes and Reflections," 2nd edition, 1840, p. 584. On the following page, the same author writes:—"Connected with these facts is the observation, seemingly well attested, that the cholera sometimes spreads in face of a prevailing wind, and where no obvious human communication is present—a circumstance difficult, if indeed possible, to be explained, without recourse to animal life as the cause of the phenomenon. No mere inorganic matter could be so transferred, nor is vegetable life better provided with means for overcoming this obstacle." Whilst on the preceding page, the "animal species" had been admitted to be "minute, beyond the reach of all sense."

analogies were seemingly strengthened by the increased knowledge which gradually arose concerning the various parasitic maladies to which man and the lower animals were liable. Writing in 1839, Sir Henry Holland says in his essay "On the Hypothesis of Insect-life as a Cause of Disease," "The question is, what weight we may attach to the opinion that certain diseases, and especially some of epidemic and contagious kind, are derived from minute forms of animal life existing in the atmosphere under particular circumstances, and capable, by application to the lining membranes or other parts, of acting as a virus on the human body." Now, the fact of the multiplication of the virus within the body was the peculiarity of these diseases, which, above all others, caused such an hypothesis to be received with favour. Causes which are specific, and which seem capable of self-multiplication—what can such agents be but living things of some kind, plant or animal? This mode of argument was with many all-powerful. And when, after the discovery of the yeast-plant by Schwann, in 1836, new doctrines concerning fermentation began to prevail, the views of those who believed in the living nature of the specific causes of epidemic diseases were in part strengthened. If all fermentations were initiated by the agency of living organisms, and the specific diseases were comparable to processes of fermentation, then how natural was it that many who were moreover influenced by the other analogies, should be led to imagine that the specific causes of these diseases were also living organisms. Only now, attention became directed to the much lower organisms which are so frequently associated with fermentative and putrefactive changes, instead of to insects "minute beyond the reach of all sense."

Here, then, is the origin of what in modern times has been termed "The Germ-Theory of Disease." Like homœopathy and phrenology, this theory carried with it a kind of simplicity and attractiveness, which insured its acceptability to the minds of many. But, however, it seems to rest upon foundations only a little more worthy of consideration than those upon which these other theories are based. Now, owing to its influence, in combination with the more generally received doctrines concerning the origin of life, there has gradually grown up an unwillingness in the minds of many to believe that these contagious diseases can arise *de novo*. And this being one of those theories which tends to curb inquiry, and to check the possible growth of sanitary knowledge in certain highly important directions, it seems to me necessary to look with scrutinising care to its foundations, not only with the view to the advancement of medical science, but with the direct object of removing all checks which may exist to the growth of sanitary precautions against the origin of these most pestilential affections.

Let us see, then, how far the "theory" fulfils the conditions which all good theories do fulfil—how far it explains a great number of the phenomena in question, without being irreconcilable with others.

The advocates of the "germ-theory" have always rested their belief in it, in the main, because they considered that it offered a ready explanation of the increase of the virus of the contagious diseases within the body of the affected person. This increase they suppose is not otherwise to be explained. All other considerations brought forward in support of the theory are just as explicable by another supposition. Fully admitting that the occurrence of a process of organic self-reproduction would be a very adequate way of accounting for the increase of the infecting material, we must see whether this mere hypothesis can be reconciled with other characteristics of these affections. In the first place, it may be asked, whether such a process is actually known to constitute the essence of any general diseases. Because, if so, those in which it does occur, ought, in the event of the hypothesis being true, to present a close similarity to the diseases in which such a process is supposed to occur.

Now, there are certain general diseases which do undoubtedly depend upon the presence and multiplication of organisms in the blood and throughout the tissues generally. There is the epidemic and highly contagious distemper amongst cattle, known in this country by the name of the "blood," and which excites in man that most dangerous morbid condition called "malignant pustule." The researches of M. Davaine * and others have revealed the fact that this disease is essentially dependent upon the presence and multiplication of living organisms, closely allied to *Vibriones*, in the blood of the animals affected, and that similar organisms are also locally most abundant in the contagiously incited "malignant pustule" of man. Unless this latter is destroyed in its early stages, the contained organisms spread throughout the body and the disease speedily proves fatal. Of late, moreover, attention has also been called * to Pasteur's researches on the subject of the very fatal epidemic which raged for fifteen years amongst the silkworms of France. This affection, known by the name of *pébrine*, is dependent upon the presence and multiplication of peculiar corpuscular organisms, called *Psorospermiæ*, in all the tissues of the body. Both these general parasitic diseases are highly contagious; both are contagious by means of organisms; and in both the virus does increase by self-multiplication within the body of the animal affected. What more suggestive evidence could there be as to the truth of the "germ-theory," say its advocates, than is supplied by the phenomena of these two diseases? Undoubtedly the evidence is irrefragable as to its applicability to these particular maladies; but then comes the question whether they are comparable with the other affections to which the "germ-theory" is sought to be applied. And this question must decidedly be answered in the negative. These parasitic diseases are sharply distinguished from the others by the fact of their almost invariable fatality. Creatures or persons once affected in this way are, under ordinary circumstances, thenceforth on the road to more or less immediate death. Happily, however, no fatality of this kind is characteristic of even such highly contagious diseases as scarlet fever and small-pox, or any other of the maladies with which parasitic organisms cannot be shown to be associated. Doubtless there are other general parasitic diseases amongst animals. In almost all the specific diseases to which man is liable, however, I have invariably failed to discover any trace of organisms in the blood. The experience of many other observers has been similar to my own in this respect. But if living things were really present as causes of these maladies, then most assuredly ought they to conform to that fatal type which is almost inseparable from the notion of a general parasitic disease, and which we find exemplified by the course of *pébrine*, the "blood," and "malignant pustule."† The fact then, that the general tendency in the acute specific diseases, is undoubtedly towards recovery rather than towards death, speaks strongly against the resemblance supposed to exist between them and the parasitic affections alluded to, and also against the hypothesis that they are dependent upon the presence of self-multiplying germs within the body. Such germs, when present, would be sure to go on increasing until they brought about the death of their host.

These considerations alone should suffice to inspire grave doubts as to the truth of the "germ-theory." And such doubts may be reinforced by many others. Thus, the several affections being distinct from one another, this theory demands a belief in the existence of about twenty different kinds of organisms never known in their mature condition, but whose presence as invisible, non-developing germs is constantly postulated, solely on the ground of the occurrence of certain effects supposed to be otherwise incapable of occurring. That, if existent, they are no mere ordinary germs of known organisms is obvious, because the presence of these has again and again been shown to be incapable of producing the diseases in question. Mr. Forster says,‡ "There is not perhaps on the face of the earth a human creature who lives on coarser fare, or to a civilised people more disgusting, than a Kalmuck Tartar. Raw putrid fish or the flesh of carrion—horses, oxen, and camels—is the ordinary food of the Kalmucks, and they are more active and less susceptible to the inclemency of the weather than any race of men I have ever seen."§ It has, moreover, been frequently demonstrated, that the organisms of ordinary putrefactions may be introduced even into the blood of man and animals without the production of any of these specific diseases. ‖ Yet is the "Antiseptic System"

* See *Comp. Rend* 1864 and 1865

* NATURE, 1870, No. 36, p. 181.

† See paper by Dr. Wm. Budd in *British Medical Journal*, 1863.

‡ See *Med.-Chirurg. Rev.*, 1854, vol. xiii., where the supposed connection of diseases with processes of putrefaction is ably considered by the late Dr. W. Alison.

§ The *Bacteria* which are sure to be abundant in such food cannot, therefore, be the much talked-of "disease germs." Such a diet is, of course, by no means recommended. Epidemic diseases are frequently most fatal when they once break out amongst a people whose diet is of this kind (see Dr. Carpenter, in *Med.-Chirurg. Rev.*, 1853, vol. xi. p. 173), and could probably only be borne in certain climates by persons who lead a very active life.

‖ See, amongst others, Davaine in *Compt. Rend*, August 1864, and E. Semmer in Virchow's *Archives*, 1870. Dr. Lionel Beale is well aware of this fact, and he, accordingly, whilst adhering to the germ theory, promulgates it under a new form. He says (*Monthly Micros. Jour.*, Oct. 1870 p. 205):—"Concerning the conditions under which these germs are produced, and of the manner in which the rapidly multiplying matter acquires its new and marvellous specific powers, we have much to learn, but with of treatment (good as it may be, irrespective of the germ-theory on which it has been based) pressed upon our attention on the assumption that the germs of putrefaction and the germs of disease are living organisms similar in nature. The strange persistency with which this view is advocated is not a little surprising, when it entails the obvious contradiction that germs which do, under all ordinary circumstances, develop into well-known organic forms, should, when concerned in the production of the diseases in question, induce all the effects supposed to depend upon their prodigious growth and multiplication, and yet never develop, never become visible. And, whilst *Bacteria* and other organisms with which the unknown disease-germs are compared, flourish and reproduce in the much-vaunted, germ-killing, carbolised lotions;* still carbolic acid continues to be recommended solely on account of its germ-killing powers, and the theory on which the practice is based is thought to derive support from the results obtained by the use of this agent. Surely no theory could be weaker on which to base a successful method of treatment; and if, as its distinguished originator says,† its general acceptance is principally hindered by the "doubt of its fundamental principle," then I would deliberately say that the blame, if any, cannot fairly be said to lie with those "who have opposed the germ-theory of putrefaction." The "Antiseptic System" of treatment needs no support from a germ-theory; it can be surely and unassailably based upon the broader physico-chemical doctrines of Liebig.‡

The last blow, however, seems given to the "germ-theory" of disease, when we are told that the blood and the secretions in sheep-pox are not infective, though this disease is most closely allied to, and even more virulently contagious than, human small-pox. If germs had existed in this general disease, and their multiplication was the cause of it, then most assuredly would they have existed in the blood and in other fluids of the body; and yet, as Prof. Burdon Sanderson tells us,§ "In sheep-pox all the diseased parts are infecting, while no result follows from the inoculation either of the blood or of any of the secretions; the liquid expressed from the pulmonary nodules has been found by M. Chauveau to be extremely virulent—certainly not less so than the juice obtained from the pustules." Now, although in other of these diseases the blood does undoubtedly exhibit infective properties, still the ascertained existence of even one exceptional case amongst maladies so contagious as sheep-pox, seems to be absolutely irreconcilable with the theory of the "germ-theory," more especially when this theory was started principally to explain the phenomena of such highly contagious diseases.‖

vegetable organisms the germs have nothing to do. They have originated in man's organism. Man himself has imposed the conditions favourable to their development. Man alone is responsible for their origin. Human intelligence, energy, and self-sacrifice may succeed in extirpating them, and may discover the means of preventing the origin of new forms not now in existence." This is undoubtedly a very much less objectionable form of the germ theory, though much additional evidence would be needed before we could accept the view that contagious diseases are due to the rapid multiplication of the contagious particles within the body of the creature affected. The non-contagiousness of the blood is as irreconcilable with this as with the other form of the germ theory.

* See "Modes of Origin of Lowest Organisms," 1871, p. 85. And in a recently published paper "On the Relative Powers of Various Substances in Preventing the Generation of Animalcules on the Development of the Germs," Dr. Dougall says: "If, as is alleged, germs are the source of putrefaction, then the strongest preventives *must* be the best antiseptics, and *vice versâ*. Now, as seen in the table, carbolic acid occupies a very mediocre place as a preventive, therefore it is legitimate to conclude that it stands no higher as an antiseptic," p. 13.

† *British Medical Journal*, August 26, 1871, p. 225.

‡ These doctrines do not seem to have been adequately grasped by Prof. Lister. Fragments of organic matter are believed by Liebig to be capable of acting as ferments; he, however, holds that their potency is deteriorated by heat almost as much as are the qualities of living ferments. The experiments with boiled fluids in bent-neck flasks, therefore, upon which Prof. Lister so strongly relies in proof of the germ-theory, prove absolutely nothing as between the two theories of fermentation of Liebig and of Pasteur. Amongst the atmospheric particles there are sure to be dead ferments in the form of mere organic fragments. Now the doubt that previously existed was, as to whether they could initiate fermentation and putrefaction, or whether the presence of living germs was absolutely essential. In the experiments with bent-neck flasks, both fragments and germs must be simultaneously excluded or admitted to the fluids. Prof. Lister's readers might suppose that Liebig had no objection to his ferments being boiled, and that the issue lay between the relative efficiency of oxygen and living germs. (See Gerhardt's *Chimie Organique*, t. iv. p. 545.)

§ Report "On the Intimate Pathology of Contagion," in Twelfth Report of Medical Officer of Privy Council.

‖ Inoculation with the blood of a person suffering from measles has also in several cases failed to reproduce the disease. The different severity of small-pox taken in the ordinary way, and that induced by "inoculation" of the matter of a small-pox pustule, is also quite inexplicable in accordance with the "germ theory."

Dr. Bastian tabulates the whole of the communicable diseases in the following manner:—

| | | | | |
|---|---|---|---|---|
| COMMUNICABLE DISEASES. | | PARASITIC DISEASES AFFECTING: | | |
| | *Many of them capable of arising "de novo."* | External (cutaneous) surface.<br>Internal (mucous) surfaces.<br>Closed (serous) cavities.<br>Tissues of organs or parts. (*Psorospermiæ*, *Cysticerci Nematoids*, *etc.*)<br>Blood. (*Bacteridia* in 'Malignant Pustule,' *Psorospermiæ* in '*pébrine*,' *etc.* | | Caused and propagated by the presence and self-multiplication of living units. |
| | | TISSUE DISEASES.<br>A. *Diseases of Internal Formed Tissues and of Mucous Membranes.* | | |
| | *All inoculable and capable of arising "de novo."* | Fibro-plastic growths.<br>Cancerous growths.<br>Tubercular growths<br>Glanders.<br>Syphilis.<br>Gonorrhœa.<br>Purulent ophthalmia.<br>Diphtheria and Croup | *Principally Sporadic.* | Caused and propagated by chemico-physical agencies, and not by the multiplication of living units. |
| | | B. *Diseases of the Blood (principally).* | | |
| | *All contagious and capable of arising "de novo."* | Erysipelas.<br>Puerperal fever.<br>Surgical fever.<br>Pyæmia.<br>Hospital gangrene.<br>Rabies. | | |
| | *Contagiousness either absent, little marked, or more or less virulent; all probably capable of arising "de novo."* | Rheumatic fever.<br>*a.* Dengue.<br>*b.* Sweating sickness<br>Intermittent fever.<br>*a.* Remittent fever.<br>*b.* Yellow fever. | *Principally Endemic.* | |
| | | Summer diarrhœa.<br>*a.* Choleraic diarrhœa.<br>*b.* Cholera.<br>Dysentery.<br>Influenza.<br>Mumps.<br>Relapsing fever.<br>Typhoid fever.<br>Typhus fever.<br>*a.* Cerebro-spinal menningitis?<br>*b.* Plague.<br>Varicella.<br>Hooping cough.<br>Measles.<br>Scarlet fever.<br>Small-pox. | *Often Epidemic.* | |

## BOOKS RECEIVED

ENGLISH.—The Subterranean World: Dr. G. Hartwig (Longmans and Co.).—On the Use of the Ophthalmoscope: T. C. Allbut (Macmillan and Co.)—Rudimentary Treatise on Geology; Part I., Physical: Ralph Tait (Lockwood and Co.).

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THURSDAY, OCTOBER 12, 1871

## RECENT UTTERANCES

THE Oracle has spoken. In fact several Oracles have spoken. Let us take them seriatim. From the lips of two of the most enlightened members of the Cabinet we have had at last an authoritative expression of the desirability — nay more, of the absolute necessity — of scientific education for the country at large. Addressing his constituents at Bradford on Monday the 2nd inst. in a speech to which we have already alluded, on the occasion of the opening of the new Mechanics' Institute for that town, Mr. W. E. Forster, the Minister for Education, as he ought to be styled, made use of the following emphatic language :—" The old grammar-school teaching was almost framed upon the advantage that Latin and Greek well taught gave to the boys ; now, we find that the boys cannot do without the use of more general knowledge than is given by Latin and Greek , that there must be a knowledge of modern languages. But there may be also a feeling that we ought to know something of the daily facts of life, and the rudiments of Science. There, again, I speak from a sense of my own want, and I have often thought how much more useful I might have been —at any rate, how much stronger I might have been—if I had had given to me a scientific education, such as I think we may now hope that our children will attain." And again : " We now believe that we have taken measures by which we may secure elementary education to all children of all classes in our borough, and throughout the country, and, consequently, those who attend this institution will have the foundation of a training that will enable them to fulfil the original idea of its promoters," that is, " to give mechanics scientific knowledge."

On the following day Lord Granville, the Secretary of State for Foreign Affairs, when presiding at the opening of the Dover College (intended to provide, at a very moderate cost, a first-class English and classical education), took the opportunity to make the following pertinent remarks :—" Then there is the study of Science in its different departments. I believe this to be eminently wise, and a matter to which parents in the present day attach very great importance. I believe the results of this branch of education are of considerable consequence ; for after all, a mere smattering of education is of very little use in any department, but a really scientific mode of studying different branches of Science is one of the best and most useful instruments of education you can use. I remember reading a very remarkable speech, with most of which I agree, delivered by Mr. John Stuart Mill, on the difficulties of a comprehensive education. He said the study of Science taught young men to think, while the study of Classics gave them the power of expressing their thoughts. I own I have thought there is some little fallacy in the distinction drawn between the education taught in these two departments. I believe it is almost impossible for a man to study the ancient languages without himself acquiring great habits of thought, and I daresay you have all had opportunities of hearing some of the most distinguished professors, some now dead and others living, who have conveyed their thoughts to their audiences in such singularly clear and perfectly eloquent language, that I feel there is something in the study of Science which makes a man feel that in what he is talking about, he must eschew all redundant and irrelevant verbiage."

The significance of these outcomes is not to be mistaken, and Lord Granville's remarks are of none the less authority because he does not happen to be our Home Secretary. His knowledge of the state of education in some other European countries has doubtless made him all the more sensible to the lamentable defects of our own. Of the other leading members of the Cabinet, Mr. Gladstone is too far-seeing a man to oppose the manifest tendencies of the age, Mr. Lowe has shown himself ready to respond to every legitimate demand made on the public purse by the proper representatives of the wants of Science, and the Duke of Argyll is himself a writer on Science.

While we cannot but congratulate ourselves that our rulers are at length alive to the importance of making Science the base of all true education, a necessity we have so constantly and earnestly insisted on, we still cannot but inquire how it is that all this has been so long in making itself self-evident to our public men. In the same address from which we have already quoted, Mr. Forster pointed out that the original design of the founders of Mechanics' Institutes was to give a scientific education to the working classes ; but that they soon found that there was an almost universally spread absolute ignorance of even the most elementary facts on which a scientific education could be based. And yet all these years have been allowed to pass, and it is only yesterday, as it were, that any serious attempt has been made to provide a scientific education for the working classes. We are even surprised to find that the first advances made by teachers of science in this direction are met by an eagerness and enthusiasm which will soon outstrip the limited means at command to satisfy its cravings. In the higher strata of society it is the same ; wherever the elements of science, natural or physical, are taught by a competent teacher, they are absorbed by boys and girls, and grown-up men and women too, with a zeal seldom bestowed on their Latin or Mathematics ; there is something in these studies which the human mind finds really to respond to its own instincts. If the next generation of Englishmen does not grow up with more than a smattering of the rudiments of science, it will be the fault of the present teachers of science themselves.

From men of high position but out of the Cabinet, who are clear-sighted enough to discern the wants of the age, we hear the same demands on every side. Sir J. Lubbock the other day, in addressing a meeting of working men at Liverpool, after delivering the prizes in connection with science classes, said that scientific men throughout the country unanimously regretted the manner in which the grants to elementary schools are distributed. Reading, writing, and arithmetic, although the foundations of education, are not education itself, and the schools will never be placed on a sound and satisfactory basis until they take a wider ground. And at the meeting of the Social Science Congress, held during the present week at Leeds, Mr. Joseph Payne, than whom no more practical authority could be found, read a paper on scientific teaching and the advantages of mental discipline for children, approving of the cultivation of the faculties of observation and experiment and direct training from nature. Science teaching, and not literary teaching, he said, ought to be the basis of all other knowledge.

One of the best recent utterances on the relation of the State towards Science is contained in the address of Prof. Huxley, delivered at Birmingham on Monday last, as president of the Birmingham and Midland Institute. In this admirable discourse he spoke of the principles of governing, and the relation of the State to its members, in a manner which enables us to congratulate ourselves that Prof. Huxley is no longer among the advocates of the limitation of State functions. He repudiated the idea of the functions of a Government being confined to those of a protective constabulary. Adopting the definition that the end of Government would be the good of mankind, he said he took it that the good of mankind meant the attainment by everyone of all the happiness which he could enjoy without diminishing the happiness of his fellow-men. The pursuits in which pleasure and happiness could be enjoyed by all, with detriment to none, were those which ought to be smiled upon by the State. If it were beyond the province of the State to interfere directly in commerce and the individual relations of men, it might safely foster these indirectly. He urged that it was the duty of Government to take the initiative in promoting the teaching of Science, leaving local energy, as soon as it could be evoked, to develop the work. The State should understand that local scientific institutions such as those at Birmingham, Manchester, and Newcastle-on-Tyne do not benefit the locality alone, but the nation at large.

With regard to the effects of Government subsidies on private enterprise, Prof. Huxley clearly showed how baseless are the grounds of alarm on this head. There are those who maintain that the State has no right to do anything but protect its subjects from oppression, but even "accepting the proposition that the functions of the State might all be summed up in one great negative commandment, 'Thou shalt not allow any man to interfere with the liberty of any other man,' Prof. Huxley said he was unable to see that the consequence was any such restriction as its supporters implied. If his next door neighbour chose to have his drains in such a state as to create a poisonous atmosphere which he breathed at the risk of typhus and diphtheria, it was just as much a restriction on his just freedom to live as if his life was threatened with a pistol. If his neighbour were allowed to let his children go unvaccinated, he might just as well be allowed to leave strychnine lozenges about in the way of his (Prof. Huxley's) children. And if his neighbour brought up his children untaught and untrained to earn their living, he was doing his best to restrict his (the lecturer's) freedom by increasing the burden of taxation for the support of gaols and workhouses for which he had to pay."

There is nothing new in these utterances, nothing that was not obvious to thinking men years and years ago; but they are of the highest importance nevertheless, for we may now hope that their lead will be followed in our English fashion throughout the length and breadth of the land. It was wisely said not long ago, that one of the most certain ways to make the study of Science national would be to make Science itself fashionable. This is true, and we may now hope that this task will for the future fall on Cabinet Ministers and the like, for scientific men who attempt it are apt to become martyrs to the good cause.

## THE LAWS OF POPULATION

1. *Population: its Laws of Increase.* By Nathan Allen M.D. (Lowell, Mass., 1870.)
2. *Physical Degeneracy.* By the same. (New York: Appleton and Co, 1870.)
3. *The Law of Human Increase.* By the same.

DR. NATHAN ALLEN, in three pamphlets, of which the titles are given above, discusses different aspects of a question of grave importance to American society, and indirectly to other societies also—namely, the comparative infecundity of that part of the population of the United States described as "native Americans." This fact, which seems pretty generally recognised, first came before Dr. Allen as a matter of personal observation, and he gives us more precise information from census returns. It appears that in the State of Vermont, for instance, the birth-rate even of the whole population, including the foreign element, is but three-fifths of what it is in England, while that of the strictly American population taken alone is estimated at only one-half of the English standard. This fact is the more remarkable, since, as Dr. Allen points out, "the comparison is between a people occupying the healthiest part of New England, engaged principally in agricultural pursuits, scattered in settlement, and a population situated as that of England is, living mostly in cities and thickly settled places, as well as composed largely of the extremes in society." Nor was it always so with the same race; for a hundred years ago the number of children under fifteen years of age was, relatively to the adult population, double what it is now. As regards the causes of this difference, Dr. Allen does not assign more than a secondary place either to emigration westward or to prudential considerations. He himself regards the physical weakness of American women, their inattention to the rules of health, and the over-straining of their nervous system, as the chief determining causes of the small number of children in a family. We have the usual complaints of tight-lacing, low dresses, insufficient exercise, and so on, which have been urged by physical moralists in all countries; but more special evils are pointed out in "the excessive use of fine flour bread," and the overstrained intellectual education of girls. To the latter cause Mr. Herbert Spencer has already ascribed the same consequences. At all events the fact of general physical weakness in American women seems to be made out, and is curiously illustrated from one point of view by the estimate of a manufacturer, that more than seven million *feeding bottles* are annually sold in the United States. So many mothers are unable to nourish their offspring!

Dr. Allen further ventures on a general theory of population, which may be stated broadly thus:—That fecundity depends upon the perfect development or harmony of all the organs of the body. The principle thus stated is very vague, and the author cannot be called successful in his attempt to give it precision; but the subject is too large for discussion here. The practical counsels which he addresses to his countrywomen are valuable and judicious, but so long as large families are regarded with disfavour, advice in this direction seems little likely to meet with acceptance. More promising are his suggestions as to the origin of this sentiment. If it be chiefly due, as he implies, to weakness of physical constitution, which causes women to dread the dangers of a large family, while "their delicate organisation breaks down in bringing into the world one, two, or three children," then undoubtedly greater physical vigour might remove some of the moral obstacles to increase of population. We cannot regard moral causes, or, in the words of an American writer, the "feeling that has grown up of late years with respect to offspring," as without importance. Is it possible, for instance, that certain circles of American society have come to resemble the Hungarians, in actually priding themselves on their small families? If any such feeling as this should exist, it is not likely to be expelled but by the supremacy of some stronger and nobler sentiment. Such might be found, one would have thought, in the sentiment of posterity, that pride in the destiny of their race, which occupies the popular imagination among Americans to a greater extent than in any other nation. Are the "native Americans" prepared to surrender the future of their country to foreign immigrants? This must be the case unless the tide should turn. At present, indeed, we hear only of a stationary not a diminishing population, and were such a community standing alone, it might do no more than realise the ideal "stationary state" of the Malthusian philosophers. But, unfortunately, the other elements of population are not stationary, and to stand still in the midst of growth is to be choked. Such a prospect can hardly be a matter of indifference to the race which is thus threatened with extinction; nor is it on several grounds without importance to the world at large. In the first place the New England Puritan stock is one possessed of many noble qualities which the world can ill afford to lose, and, secondly, it is hard to see where this process is to stop. If the influence of the *milieu* has reduced the descendants of a people so mentally and physically vigorous as the English colonists of the seventeenth century, to a state of infecundity and "physical degeneracy" (to use Dr. Allen's words), what are the prospects for later colonists, whether of English, Irish, or German descent? They will soon be "native Americans," and subject, as we must suppose, to the same laws of change. Is transplantation of a race, as Knox and others thought, impossible? This question is neither raised nor answered by Dr. Allen, but it is inevitably suggested by the gloomy pictures which he draws. His pamphlets, in spite of much repetition, and an occasional superficiality of treatment, are worth reading by those who are interested in the important problems which he discusses.

## OUR BOOK SHELF

*National Health.* By Henry W. Acland, F.R.S., &c. (Oxford and London: James Parker and Co., 1871.)

Dr. Acland's pamphlet should be read in connection with the report of the Royal Sanitary Commission, of which he was a member, and some of whose recommendations have already been embodied in a Government measure. Not that it is intended as an exposition or defence of that report, but rather as an exposition of the general principles of sanitary legislation and reform. It would be impertinent to say that in knowledge and enlightenment Dr. Acland is on the level of his important theme, but we may point out as his special qualification for treating a subject of such complex relations, a certain comprehensiveness of mind, which does not allow him to leave untouched either the moral or the material, the scientific or the political, aspects of national health. We think it the more important to draw attention to this valuable quality because it is so often wanting in professional, perhaps especially in medical writers, and the want is so often a source of weakness. Dr. Acland does not forget, in treating of national health, the dependence of disease on poverty or of poverty on over-population; and insists strongly on the often-forgotten principles of Malthus. It is instructive to contrast the dangers he points out with the apprehensions of an entirely opposite kind entertained by Dr. Nathan Allen. On this side of the Atlantic we dread the results of too rapid multiplication; on the other side their fear is lest, among a certain class, this danger should have been too completely averted. But both would agree that the property of fertility does not always belong to those whom we should think best fitted to be the progenitors of the race to come. Dr. Allen laments the decay of the highly cultivated and intellectual New Englanders; while Dr. Acland, quoting Mr. Galton, points out the possibility of "the races best fitted to play their part on the stage of life being crowded out by the incompetent, the ailing, and the desponding," merely in consequence of a reckless system of early marriages. This very fact, we may remark, of the rapid multiplication of the "incompetent and ailing" is of itself fatal to the theory of population advanced by the American physician.

In his remarks on the regulation of public health, Dr. Acland shows the same breadth of view as in treating the more scientific aspect of the subject, and his wise, we might say, statesmanlike advice contrasts with the too absolute and inconsiderate claims put forward by some medical and sanitary reformers. It should never be forgotten that the power of seeing even the plainest evils cannot go beyond the general standard of public enlightenment, and that the power of removing them must be limited by the social and political conditions of the country in which we live. The following quotation appears to us to contain very sound advice:—

"Two things and two only remain to be done.

"First. To continue to interest intelligently the mass of the people in sanitary progress, and to interest them more systematically.

"England must rule herself in these as in all other matters. The time is gone when people can be dragooned into cleanliness and virtue. We hear that the middle class of England is inefficient, the guardians of the poor bad, and the working classes ignorant. If so they are still the people; they and their children pay the penalty of disease and of vice. Show them, truly and without exaggeration, the source of avoidable disease and of destructive vice; they will abate it. Bring the knowledge to their doors, they have heart and will; give the power by enactment, and the work is done.

"Second. To establish such a health department in the metropolis as shall with certainty appreciate the growing wants of the people, as shall bring in bills to meet their wants, and shall disseminate information and advice without stint to every part of the country."

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Local Scientific Societies

The following statement appeared a short time ago in an article in Nature. Throughout the country we find societies, field clubs, local museums, &c., all of which are more or less actively engaged in the pursuit of knowledge, local inquiries, or explorations, &c., I fear this flattering description must have arisen from the writer not having a practical acquaintance with local societies.

In a society in the West of England, consisting of nearly 400 members, I know of but one who does anything for the local museum, or for the advancement of geological science. The society's principal results are archæological; geology and natural history are in the background. Another west country society is divided into innumerable sections, which have their excursions, and an occasional general excursion; but their results in the cause of science are as valuable as those of an ordinary picnic party. This description will, I fear, answer also very well for one with which I am acquainted in Sussex. In all these instances the local museums are such as might be expected from such apathy.

In too many instances the science of the scientific societies begins and ends with the name. There may, perhaps, be one or two members who are active, but feel little encouragement to do much for the public good, or in the way of contributing to the local museum. Of course these societies are composed in a great measure of members who take no interest whatever in science, and who join them without any definite object; but it is a pity that the public should be subjected to such a delusion. There are, of course, some few societies which are fortunately more active, and produce valuable results, but as yet I have seen no good local museum in connection with them, and that is a bad test of the practical nature of a society. I know of but one museum which at all answers the description of a local museum, and that is at Bath, which is due to the genius and energy of Mr. Charles Moore. But as long as members of local societies collect for themselves and not for the public good, their museums must remain at a stand still. Few have any idea of the valuable collections which are made, or the labour spent on their formation, by individuals who are indifferent as to what eventually becomes of them.

It is want of public spirit and self-complacency, which are the great hindrances to all progress. It is to be regretted that the Geological Society of London does not set more of an example to the provincial societies; it ought to exert an influence throughout the country, and take some interest in their progress. The state of the collections at Somerset House is certainly not an honour to any society.

The co-operation of local societies, and having their results published for the benefit of all, *might* have a great effect on the advancement of science. A general contribution for the purpose of a weekly issue of British Journals of Science (in various departments), which should be common to all, would be a step of great importance. I know of no remedy for this state of ignorance and apathy as to the valuable results of which they might be capable, but such a co-operation, combined with a certain amount of union with the scientific societies of London, which might have the effect of keeping the provincial societies up to the mark. This must also be accompanied by a unity of object, as well as of system in the management and arrangement of their museums.

F. G. S.

### Newspaper Science

IN reply to the letter of "Medicus" in last week's NATURE, allow me again to state that the curious details as to Krupp's gun manufactory, with which the public were enlightened in the *Globe* of September 11, appeared in that paper as a leading article, and not as a mere "note-paragraph," as "Medicus," who "never writes articles," evidently desires to be understood. Had they been in the form of the ordinary newspaper paragraph, containing accounts of some wonderful discovery in zoology, chemistry, or mineralogy, such as, for example, some late ones on "the appearance of a gigantic lizard in North Wales," "the extraction of the fixed air from the pea sausage for use in the army," or the "abundance of platinum at Bathgate, in Scotland," which I find copied into the *Times* of to-day, I should not have troubled the readers of NATURE with my letter of September 13.

When, however, we find such "blunders," to use "Medicus's" own word, whilst he admits at the same time that they "had passed the editorial eye," palmed on the public on the authority which should be due to the leading article of a highly respectable and largely circulated newspaper, I think it is high time to protest against technical science being popularised in this style; and *à propos* to style, the peculiarly pleasant and what would vulgarly but expressively be called the "chaffing" style of "Medicus's" communication to NATURE conveys to the reader the impression that its author is more at home in writing for penny newspapers than for scientific periodicals.

In conclusion, the perusal of the letter of "Medicus" will certainly remind metallurgists of the man who, when he felt his feet slipping under him in the water, brought himself altogether out of his depth by imprudent and convulsive struggles to extricate himself. The use of the French word "*creuset*" instead of the plain English "crucible," suggests a French source of information, and not the original German "Schmelztiegel" of Krupp's manufactory at Essen; and when "Medicus" corrects his text, and tells us it should read "the iron is alloyed in crucibles formed with certain clays and a preparation of plumbago" (!) metallurgists will still believe that it was steel not iron which is introduced into these crucibles, and doubt its being alloyed at all, but only melted in them; and will, moreover, be of opinion that if "Medicus" was at home in the subject on which he has been writing, he would have at once explained that when he unfortunately described the steam-hammer as "of the force of 25,000 kilometres" (in plain English, 15,532 miles), that the last word was simply a misprint for kilogrammes (so that the hammer was nearly $24\frac{3}{4}$ English tons), and not grasped at a straw in the shape of the far-fetched and in this instance equally misapplied term kilo*gram*metre!

DAVID FORBES

11, York Place, Portman Square, London, Oct. 9

P.S.—If "Medicus" desires correct information as to the steam-hammers, &c., at Krupp's manufactory at Essen, he will find it in the recently-published official report of the Chamber of Commerce there, a short abstract of which is embodied in my fourth quarterly report (for 1871) to the Iron and Steel Institute, on the "Iron and Steel Industries in Foreign Countries."

### The Cyclone in the West Indies

I THINK others besides me would be glad of an article in your paper on the Cyclone of the 21st of August in the West Indies. The narrow limits of the hurricane are noteworthy. I hear from the West Indies that Nevis, between Antigua and St. Kitts, has escaped, being a little to leeward. Has Saba escaped likewise? To windward Barbuda and Anguilla seem to have been also beyond the storm, as was also Virgin Gorda; the centre of the cyclone passing over St. Thomas (and, I presume, Tortola also) on its way to Porto Rico.

I have exact details only from St. Thomas, which I could, I think, put at the service of any one writing on the matter; but the principal fact in them is, that the main rush of wind, which did the damage, fell on the harbour from N.E. to N., destroying horribly all houses in the N.E. gully which slopes down to the harbour; but so turned right and left by the high hills above the town, that it was impossible for one in the harbour to discern the actual direction of the main current. This blast fell just before the central calm.

I trust that we shall have from some of your contributors somewhat which will throw more light on all hurricanes, from the lessons of this last.

Excuse the interest which one who knows those seas and islands—when he passed through them, blazing in beauty and repose—must needs take in the details of such a tragedy.

Eversley, Winchfield C. KINGSLEY

### On the Solution of a certain Geometrical Problem

I REGRET that the work I referred to should have been so readily identifiable; still more, that Mr. Todhunter should think I intended to imply "signal geometrical weakness" on his part. I should imagine, on the contrary, that few living men surpass Mr. Todhunter in geometrical strength; though I may have inferred from some passages in his works that that special part of his mathematical strength had not been so fully developed by practice as his power in mathematical analysis.

It must be quite obvious to anyone who reads the whole of the appendix to Mr. Todhunter's Euclid, that sooner or later the series of problems on circle-contact (*i.e.* to Prop. 16) would require the introduction of the sixth-book method. This method is also very conveniently introduced in Prop. 7. But the omission of all mention of the third-book method* would certainly lead the student to infer that the sixth book must be employed. If it led me to infer that Mr. Todhunter happened not to know

* Especially as but three lines would be needed to indicate the method. Thus: From the given point A draw a perpendicular A D to the bisector of the angle between the given lines; produce A D to E so that D E is equal to A D; a circle described (by the preceding proposition) through D and E to touch either of the given lines will obviously touch the other also.

of the third-book method, I can readily show that such an inference was by no means so absurd as might be inferred from his remarks about the oldness of the problem. For I remember distinctly an occasion on which the solution of this problem was required during a lecture at King's College, London, at which my friends Baily (second wrangler in 1860) and Hudson (third wrangler in 1861) were present. Three of the students at once submitted to the lecturer the solution by the sixth-book method (which no one can well miss), and the lecturer (a second wrangler), while admitting that the solution was not very pleasing, was unable at the moment to suggest a better; he added, jokingly, that the best way to solve the problem would be to describe a parabola having the given point as focus and either of the given lines as directrix. Now, he had not been long engaged in teaching, and it may be perfectly true that one who had been so engaged "would certainly have in his memory one or more solutions of this problem;" but this would depend on the subjects he had been engaged upon. If he chanced to be one of the most eminent mathematical professors at Cambridge, it is probable that no problem in the higher analysis would be unknown to him, but the odds would be rather against than in favour of his being familiar with the best solutions of geometrical problems, just as the odds would be against his being proficient in the rules for "Barter," "Tare and Trett," and "Alligation Partial."

From letters which have reached me I find that the general purport of my letter has been misapprehended, since some appear to infer that I question the geometrical power of our University mathematicians. I meant nothing so unreasonable. We have geometricians who rival (and I believe more than rival) in power the best Continental geometricians. But their geometrical strength has not been attained during their University career; and no one who considers carefully the mathematical course at either University, can believe that it tends either to form geometricians or to foster geometrical taste.

I candidly admit that I do not speak of either course from personal experience. All I know of geometry was learned before my Cambridge time, and very nearly all I know of analytical mathematics was learned after that time. But I know quite well the nature of each course, and can sustain my statement that our universities do not encourage the study of geometry. Whether they should do so is a matter on which I have expressed no opinion. RICHD. A. PROCTOR

Brighton, Oct. 7

P.S.—Mr. Todhunter refers to the actual solution of the problem as a "matter of some interest, though of course unconnected with the theoretical solution." As I have had some experience in constructive geometry (having always made it a practice to solve astronomical problems constructively before proceeding to numerical calculation), I may be permitted to make some remarks on this point. First I would add to the compasses and parallel ruler (the only instruments mentioned by Mr. Todhunter) that most useful instrument the square. With this instrument (which would be needed in any case) the following construction would be as convenient as the one founded on the sixth-book solution. The problem, be it remembered, requires that a circle should be described through a given point to touch two given straight lines. Let P be the point, AB and AFCG the lines, AHDK the bisector of BAC (this bisector must be drawn in both methods, so that I leave its construction untouched); with the square draw CPD square to AK, and PE square to CD; with centre D draw circular arc PE; with centre C and distance PE draw half circle FLG; then FH and GK drawn square to AG (with the square) are radii of the two circles fulfilling the conditions.

## Prof. Newcomb and Mr. Stone

IN Mr. Proctor's letter to NATURE of the 23rd ult., he remarks that Prof. Newcomb had stated to him that he was bewildered at having a discussion of the transits of Venus and the parallax of the Sun, deducible from them, prior to that of Mr. Stone, attributed to himself; and Mr. Proctor goes on to state that he was justified in his belief that such a discussion had been made because a writer, signing himself "P. S." had asserted that it had in a letter appearing in the *Astronomical Register* for December 1868. He further gives two reasons for the unhesitating credit which he had given to the assertion of "P. S." The first of them is that there is strong internal evidence that the writer was a distinguished astronomer having those as his initials (or a part of his initials, it would be more correct to say); of this it seems scarcely needful to say more, as the writer in question may prefer not to be unearthed. But of Mr. Proctor's other reason, may I be permitted to say a word? It is that the assertion of "P. S." was "permitted to remain uncorrected."

Had Mr. Proctor turned to the very next number of the *Astronomical Register* (that for January 1869) he would have found a letter signed also with initials "W. T. L." in which "P. S.'s" assertion that Prof. Newcomb had published any discussion of the transit of Venus in 1769, is most emphatically contradicted. "P. S.," it is true, made a rejoinder in the March number of the *Register* (page 65) but in it he neither denies "W. T. L.'s" contradiction, nor refers (as of course he could not) to any original investigation of the transit-of-Venus problem by Prof. Newcomb. He contented himself with the rather unintelligible remark that "W. T. L.'s" answer was not in "the spirit of the age we live in." The latter writer in the following number of the *Register* (page 88) pointed how, in all probability, the mistake of "P. S." had arisen from misunderstanding part of the title of a paper by Prof. Newcomb on the Distance of the Sun, and the matter dropped.

Now, as Prof. Newcomb was as likely to have seen "W. T. L.'s" contradiction as "P. S.'s" assertion, there would certainly seem no necessity for his further disowning himself what "P. S." had claimed for him. W. T. LYNN

Blackheath, Oct. 2

## Note on the Cycloid

I DO not know whether it has been noticed that the cycloid is a projection of the common helix (thread-inclined 45°). I suppose the property must have long since been recognised, but have not seen it mentioned.

The proof is very simple, and may be thus presented:—

Suppose a vertical circle to have its plane east and west (a luminous point, for the nonce), the sun in the meridian and 45° high. Then the shadow of the circle on a horizontal plane will clearly be a circle; and further, if a point move uniformly round the vertical circle, the shadow of the point will move uniformly round the shadow-circle. Now, let the centre of the vertical circle advance horizontally towards the south, while a point moves round its circumference at the same uniform rate. The moving point will describe a right helix with a thread-inclination of 45°. Its shadow will move uniformly round the shadow-circle while the centre of this circle advances uniformly and at the same rate in a straight line. It will therefore describe a cycloid.

It is obvious that all the varieties of curtate and prolate-cycloids may be obtained as projections of helices, by changing the thread-inclination.

Also it is obvious that if the sun (or the point of projection) were in the zenith, the shadow (or projection) of the helix first dealt with would be the curve called "the companion to the cycloid." RICHARD A. PROCTOR

## Is Blue a Primary Colour?

IN recent works on colour blue is called a primary colour. If blue is a primary colour a mixture of yellow and blue transparent pigments could not produce green, but would form an opaque combination. The colour produced by a mixture of yellow and blue pigments—if blue is an elementary colour—will depend on the colour reflected by the coloured layer itself, and not on the light passed through it from the white surface underneath. The brilliancy of the green produced by mixing yellow and blue pigment, is a measure of the transparency, to the green rays, of the blue pigment employed. Or in other words, there is as much green in the blue pigment employed, as there is green in the green produced by mixing that pigment with yellow. Blue must, therefore, be a compound colour, since the blue pigment passes the green rays.

Further. When the light reflected from blue substances is examined with a prism, it is found to be composed of green and violet. Again, when green and violet are combined by means of a rotating disc, blue is produced. By varying the proportions of green and violet any colour from green through blue-green or sea-green, blue, blue violet or indigo to violet, may be obtained. Again, when the solar spectrum is thrown on a blue surface, the green and the violet rays are reflected in the same way as a yellow surface reflects the red and the green rays.

The following is a simple way of showing that blue is not an elementary colour, and that violet is an elementary colour:—Take a piece of red, a piece of green, and a piece of violet glass. Any two of these form an opaque combination—that is to say, the first glass stops all the rays which could pass the second and the second stops all that pass the first. But a green and a blue glass do not form an opaque combination, but pass the green rays. If we place the red, the green, and the violet glasses in a row close to each other, with the green in the centre, place a piece of yellow glass so as to overlap the junction between the red and green glasses, and a piece of blue glass to overlap the junction between the green and violet glasses, and arrange the combination so that white light can pass through it, it will be seen that the yellow glass passes the red and green rays, and the blue glass passes the green and violet rays; and that the only effect of the yellow and blue glasses is to deepen the colours when the light passes through them.

Darrock, Falkirk, Sept. 23 JOHN AITKEN

### Anthropology and M. Comte

PERHAPS you will allow me to state that your report of my paper "On the Anthropology of Auguste Comte," read before the British Association at Edinburgh, is wrong in two essential particulars. First, I did *not* attempt to "expound the views of M. Comte according to the principles laid down by Mr. Darwin." Comte's views on man and his relation to the animal kingdom were published upwards of twenty years ago; Mr. Darwin's recently. Second, I did *not* "maintain that Auguste Comte's worship of humanity would be the great doctrine of the future." I may, and do believe this, but I made no reference to it whatever in the paper which you have so correctly mis-reported. The Positive religion was not the subject of discussion, and I limited myself to what my paper implied. J. KAINES

3, Osborne Road, Stroud Green Lane, N., Sept. 22

### A Plane's ——?

IT is perhaps answering somewhat at cross purposes, but would not the word *aspect* meet Mr. Wilson's requirements? In reference to those important planes of every-day life, garden walls or house fronts, its use is well established; and there would be no violence to either custom or language in applying it to geometry. J. K. LAUGHTON

[Another correspondent suggests the term "slope."—ED.]

### Meteorological Phenomenon

AMONGST some old memoranda I find the following, which I copy verbatim:— JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Sept. 18

"Monkstown, near Dublin, about 3.10 P.M. 25th of July, 1858, saw, about opposite the sun, an appearance like the rainbow, but horizontal, and extending along a few degrees of the horizon. The red was above the sea-horizon, and the green below. I could not make out beyond the green, but this might be because the blue was blended with the colour of the sea. As I did not see it commence, I cannot say how long it lasted. It faded gradually but rapidly, without any other change in the sky that I saw. The day was alternate sunshine and heavy showers; the sun was shining at the time.

"This note has been made within half an hour after its disappearance."

### Lunar Rainbow

A VERY perfect lunar rainbow was seen here last night. I noticed it first at 9.42. At that time the northern portion of it only was visible, but its intensity steadily increased, and by 9.45 the arch was complete. Both at the northern and southern extremities there was a peculiar glare, extending upwards about 20°, the apex of the arch being remarkably clear and well-defined. The rainbow faded away as rapidly as it had been developed, and at about 9.50 had entirely disappeared. At the time of the occurrence the western portion of the heavens was very clear, and the moon about 8° above the horizon. Temperature cold, with a biting wind from W.S.W. R. B.

Hinderton, Neston, Cheshire, Sept. 23

### The Corona

MAY I suggest a method of observation which would possibly be a more delicate test than that which our own sight affords for ascertaining the outer limits of the corona?

It seems probable, from a discussion of former observations, that the polarisation of the sky is altogether changed during totality, and that instead of being radial to the sun as at other times, its plane is perpendicular, or nearly perpendicular, to the horizon. This appears at all events to be the case over a very large area about the eclipsed sun.

In passing along a parellel to the horizon through the sun's centre, we should expect to find, at some little distance from the limb, the pure atmospheric polarisation unaffected by any component due to the corona.

At such a point an observer using a Savart might therefore expect to find the bands disappear at an angle of 45° to the horizon. Having carefully turned out all trace of the bands upon the centre of his field, let him now pass onward towards the sun's limb (directing his attention all the time to the centre of his field only), when he there perceives the first trace of bands. He will know that the plane of polarisation has changed. If, on going backwards, the bands disappear again, while in passing onwards they continue to increase, he will know that that change is due to a component introduced by the corona; and he will be able to estimate the distance from the moon's limb at which such a component first became visible.

I feel disposed to think that by this method he will be able to trace the corona further than he could by the unaided eye; for it will be somewhat equivalent to making the corona shine upon a perfectly black back ground of sky; and much more than equivalent to accomplishing this with a Nicol only, for the Savart will detect less than one-eighth of the polarisation detectable by the Nicol.

The visible outer border of the corona is where our eye first distinguishes a difference between

The light of the sky
and
The light of the sky + the light of the corona,

while by this method the visible outer edge of the corona will be where we first distinguish a difference between an area of no polarisation and polarisation due to the corona.

In using a Savart with a large field, the central portion of the field might well be marked by fixing in the common focus of the telescope a plate of glass with a small circle etched upon it corresponding say to 8′ or 10′ of diameter in the field of view.

A. C. RANYARD

### A Rare Moth

A FINE specimen of the rare moth *Deïopeia pulchella* (crimson speckled footman) was captured by R. Beck on the Moors near Scarborough on the 11th inst. Could any of the readers of NATURE inform me whether it has ever been taken so far north before? W. E. WALLER

Oliver's Mount School, Scarborough, Sept. 22

### Meteorology in America

THE writer of the article on this subject may be interested in hearing that a meteorograph, similar in some respects to that invented by Prof. Hough, was sent to the International Exhibition just closed in London. It was invented and constructed in Sweden, and one similar is said to have been performing satisfactorily for nearly three years. In the Swedish, as in the American instrument, the height of the mercury in the barometer, and the wet and dry thermometers, is felt by steel wires descending the tubes; but in the Swedish instrument the levers to which these wires are attached are acted on by very fine screws, the revolutions of which, translated by a series of wheels into the language of barometers and thermometers, are printed every quarter of an hour on an endless roll of paper. The whole apparatus is set in motion by a galvanic battery, which even winds up the clock which regulates its own action. The barometer is tapped before it is registered, but there is no correction for temperature. The price is 350*l*.

The barometer invented by Prof. Wild seems to bear some resemblance to the barograph invented by Mr. King, and now used at the Liverpool Observatory. W. R.

### Ruined Cities of Central America

IN the summary of the proceedings of the late meeting of the British Association, in the issue of NATURE for August 31, is an abstract of a paper by Captain L. Brine, R.N., *On the Ruined Cities of Central America*. The gallant captain is wrong in stating that the existence of these ruined cities was unknown until within a comparatively recent period. All the early chronicles abound in allusions to them—Remesal. Vasquez, Cogolludo, Villagutierre, Juarrez, and others. Uxmal and Chichen Itza, which Captain Brine speaks of as "discoveries," were undoubtedly occupied places at the time Grijalva touched the shores of Yucatan. Copan, although then a ruin, was visited and minutely described by Dr. Palacios as long ago as 1576. Captain Brine would lead us to infer that these remains have been "discovered" since the expedition of Del Rio to Palenque in 1787.

That these Ruined Cities were built by the progenitors of the various families of the Tzeudal or Maya stock found in Central America at the time of the discovery, and who are still there, and that many of them were then occupied and flourishing does, not admit of doubt—is capable of demonstration.

Big-eyed Wonder should be eliminated from modern speculation!

E. GEO. SQUIER

New York, Sept. 14

### The Dinnington Boulder

I HAVE been favoured with a letter from a geologist residing at Newcastle-upon-Tyne, who kindly informs me that he has inspected the "fossiliferous boulder," and pronounces it to be a block of carboniferous limestone.

This gentleman, from his knowledge of the district, says, that this limestone (underlying the coal measures) crops out about seven or eight miles to the west or north west of Dinnington, from whence probably it came. The question asked of its direction of travel is therefore satisfactorily answered.

J. BROUGH POW

Barbourne, Worcester, Sept. 21

### Mechanical Drawing

IN the opening address of the President of the Mechanical Section of the British Association, descriptive geometry and geometrical projection are both spoken of as subjects of little value to the mechanical draughtsman.

Now, being interested in the matter, I would like to ask the nature of that special kind of mechanical drawing of which the President spoke, and which leads to mensuration and geometry. I suppose from the address, descriptive geometry and geometrical projection will be dispensed with, seeing, as he says, that it is no loss to the mechanical draughtsman to be ignorant of the latter. As an illustration of that *real* mechanical drawing which he advocates, would Prof. Jenkin be kind enough to show the method he would adopt in the construction of a drawing which would show the lines of intersection of the surfaces of a cone and sphere whose axes are not in the same plane?

I can assure Prof. Jenkin that a word of advice from him will always be a great boon to the hardworking student.

DRAUGHTSMAN

Fall River, Mass., Sept. 18

### Ice-Fleas

I SHOULD have thought that the "ice-fleas" described by Prof. Frankland had been almost as familiar to Alpine travellers as their more offensive namesakes of the châlets. They are described by De Saussure (Voyages, § 2249), by Mr. Morell, "Scientific Guide to Switzerland," p. 275; by myself "Alpine Regions," p. 207, where references are given, chiefly to a paper by M. Nicolet in "Neue Denkschriften der Allg. Schweiz. Gesellsch." vol. v. (1841); and by other writers on the Alps.

T. G. BONNEY

St. John's College, Cambridge

### Thermometer Observation

ONE very hot day last summer I exposed to the sun, in the same position, three thermometers; No. 1 was a new one mounted on box wood, No. 2 was similar, but very dirty from exposure to the weather; No. 3 is what is known as a bath thermometer, with a metal scale. In the shade they all agree to about 1°, but in the sun No. 2 rose about 8° above No 1, and No. 3 about the same above No. 2. Here we have a discrepancy of about 16°, caused no doubt by the different heat absorbing and radiating powers of the substances on which the thermometers were mounted. I think this may somewhat account for the various readings we see announced by different observers.

D. J. STUART

## *THE USE AND ABUSE OF TESTS*

THE gradually increasing recognition of the claims of Science by the Government is cause for unmingled satisfaction to every one who is interested in the material and moral progress of the country. And now that the Government has set its hand to the work, it seems disposed to let no timorous counsels or half-measures prevail. The readiness with which the demands of astronomers have been met last year and this, the really admirable practical instruction recently given to science teachers at South Kensington, are evidence of the earnestness of the intentions of those in authority.

In the present attitude of the Government towards Science, however, everything is not yet as it should be. Much of the practical value of this earnestness consists in the manner in which details are carried out, and there is one department of the administration in which a spirit of mischief appears to delight in neutralising all efforts at improvement. The recent movement to compel all candidates for employment under Government to submit themselves to an educational test is in the main a good one; but it may be carried to an excessive, even to a ludicrous, extent. Tests are in themselves valueless, unless they are so contrived as to test the possession by the candidate of those qualifications which will best fit him for the office he aspires to fill.

There are at the present time vacancies in one of our Government scientific establishments for two junior assistants, and the principal of the establishment was desirous of appointing two young men who possessed the needful qualifications of neat and orderly habits, punctuality, and obliging demeanour, and a love of Science for its own sake. The establishment in question has, however, the misfortune to be under the control of the Board of Works; and when the authorities of this department heard of the vacancies, they insisted, notwithstanding the remonstrances of those most interested, in announcing them for public competition. The consequence will be that the posts will, in all probability, be given to those who display the best acquaintance with English History or French, but who have not proved themselves possessed of a single qualification for these particular posts. This Procrustean system of measuring all men by the same standard will not answer. The inevitable result will be to fill all the square holes with round men, and all the round holes with square men. As reasonably might we require all the clerks in the Foreign Office to be acquainted with the properties of the chemical elements, or every assistant in the library of the British Museum to be able to name the bones in the human skeleton; for these are as essential to the liberal education which every gentleman ought to possess, as a knowledge of English History or French. The system pursued in the British Museum, which is fortunately under the control of another department of the administration, would satisfy all reasonable requirements: that the principals of all establishments should have the right to nominate candidates to vacancies, subject to a qualification-test of their general acquirements. It is but fair that in departments where the efficiency of the subordinate officials depends so much on their willingness to co-operate heartily with their superiors, and on the possession of qualities which no examination can possibly test, the principals should have some voice in the appointment of those who may probably succeed to the offices they themselves occupy. An opportunity is thus also given for the encouragement of young scientific aspirants, who may be known as earnest and careful workers, but who would otherwise stand little chance of Government employ.

We make these strictures in no carping spirit, but simply with a desire that the good work now commenced may not be marred by errors of administration. The only object of the system of competitive examinations, and of compelling all candidates for Government posts first to submit themselves to a qualification test, is in order that these offices may not be the refuge for genteel incompetence, but may be bestowed on the most fitting aspirant. We fear the above facts will show that the present system is not calculated in all cases to secure this end.

## THE GIBRALTAR CURRENT

MR. CROLL having stated (NATURE, August 17) that, taking my own data, and having "in regard to the Gibraltar current and Dr. C.'s general oceanic circulation, determined the absolute amount of those effects on which his circulation depends," he has satisfied himself by mathematical investigation "that the work of the resistances greatly exceeds the work of gravity, and that consequently there can be no such circulation as that for which Dr. C. contends,"—I think it well to point out that the question of the existence of such a circulation is not to be disposed of by the calculations of even such an expert computer as Mr. Croll, but must be decided by the collection and comparison of facts ascertained by observation and experiment.

Now, as it happens that an opportunity has been recently afforded me by the Hydrographer to the Admiralty of carrying out, in conjunction with Captain Nares, of H.M.S. *Shearwater*, a series of further researches on the Gibraltar current, which place beyond all doubt the outflow of dense Mediterranean water into the Atlantic, over the "ridge" or "marine watershed" between Capes Trafalgar and Spartel, and beneath the surface-inflow of Atlantic water, I would submit (1) whether there must not be some fundamental fallacy in Mr. Croll's computations in regard to the Gibraltar current, and (2) whether this fallacy should not destroy all confidence in the infallibility with which Mr. Croll credits himself in regard to the general oceanic circulation.

No one can be more ready than myself to admit that this last doctrine is at present only a hypothesis, resting on a very narrow basis of fact. But as this hypothesis has been accepted as probable by such great masters in physical science as Sir John Herschel and Sir William Thomson, and as the means of putting it to the test will be supplied by the Scientific Circumnavigation Expedition, which (I have every reason to expect) will be fitted out by Her Majesty's Government next year, I would venture to suggest whether prudence does not dictate to the opponents of that doctrine, that they should either drop further discussion of it for the present, or that at any rate they should refrain from attempting to demonstrate its impossibility.

The number of NATURE which contained Mr. Croll's letter, having also given an account of the discussion which took place in the Physical Section of the British Association on a communication I made to it with reference to this subject, I may mention that my especial purpose in that communication was to obtain the judgment of the able physicists there assembled, as to a fundamental question at issue between my friend, Prof. Wyville Thomson, and myself, namely, the *cause* of that flow of polar water over the deepest parts of the ocean bottom, bringing down its temperature even under the equator to 35°.5, as to the fact of which we are in entire agreement. By my excellent colleague it is considered* that this flow is due to an indraught of polar water, occasioned by the surface efflux of equatorial water resulting from the action of the Trade Winds. To myself (not professing more than an elementary knowledge of physics) it seemed probable, on the principle of "least action," that the surface-water so removed would be replaced by an inflow from some other part of the oceanic surface, that is, by a horizontal circulation, rather than by an uprising of the whole subjacent mass, so as to draw in polar water at the bottom, and I have pointed out that such a surface-replacement is known to take place in the case of the Gulf Stream, one portion of which directly returns into the equatorial current, completing the shorter circulation, whilst the other has its complement in the Greenland, Labrador, and other polar surface-currents, of which the principal is traceable southwards nearly as far as the exit of the Gulf Stream from the Narrows, thus completing the longer circulation.

* See his Address on "The Distribution of Temperature in the North Atlantic." NATURE, July 27.

The correctness of this "common-sense" judgment was most emphatically affirmed, on the basis of profound physical knowledge, by Sir William Thomson and Prof. Stokes. It was agreed by these high authorities that in the open ocean the action of wind on the surface can never produce any other than a surface movement; the water propelled onwards from one part of the oceanic area being replaced by a surface inflow from other parts. It is, therefore, for my opponents to explain how, otherwise than by gravity, it happens that polar water finds itself at the depth of 2,000 fathoms under the equator. That the bottom-temperature of the equatorial area, if there were no movement of polar water towards the equator, would be at least 20° higher than it is, may be asserted without the least hesitation; the temperature of the Mediterranean, which is cut off from communication with the lower stratum of the Atlantic, being 54° at corresponding depths.

It was agreed by Sir William Thomson and Prof. Stokes, that when a wind blows continuously into a loch or fiord, so as to produce a rise of water at its head to the amount of 6, 8, or 10 feet, such an excess of vertical pressure produces an outward under-current; the evidence of such outflow being afforded by the continuance of the surface in-current at the rate of three or four miles per hour, without any further increase in the rise of water at the head of the loch. This exceptional case was advanced by Sir W. Thomson as strongly confirming my general principle, not as invalidating it; and I would therefore recommend Mr. Croll to test his method of investigation by this ascertained fact, rather than spend his time in demonstrating the impossibility of what he may hereafter have to admit as no less certainly proved.

WILLIAM B. CARPENTER

H.M.S. *Shearwater*, Malta, Sept. 29

## SCIENCE IN ITALY

IN NATURE for June 8, I sketched a short notice of some of the Italian scientific serials, among them the *Annali di Chimica Applicata alla Medicina*, published at Milan. With the commencement of the present year the *Gazetta Chimica Italiana* has been launched at Palermo. The project of this publication originated in Florence with a society of Italian chemists, who met there in October last, and resolved to entrust the first year's "direction" of the magazine to Prof. Stanilaus Cannizzaro of the University of Palermo.

The Italian Chemical Gazette very nearly resembles the *Journal of the Chemical Society of London*. Like this it contains, first, original memoirs; second, translations or abstracts of the most important foreign chemical memoirs; third, a review of technological chemistry, agricultural chemistry, and crystallography; fourth, a summary of the principal chemical journals of Germany, England, and France; fifth, miscellaneous notes that may be interesting to those who cultivate chemical science. It is published monthly.

The most prominent, the longest, and most interesting of the original papers is by Prof. Cannizzaro: "Historical notes and reflections on the Application of the Atomic Theory to Chemistry, and on the Systems of Formulæ for expressing the Constitution of Compounds." This paper is continued in the number for January, April, and May, and is not yet completed. The following extract from the introductory observations will indicate the spirit in which it is written:—"A few are still dissatisfied with the arguments against the dualistic system, and continue to employ the atomic weights of Berzelius, or the equivalents of Gmelin; and among those who have adopted the new system of atomic weights and formulæ, there are many who have done so merely in a spirit of concession, and make a display of scepticism respecting its intrinsic value; others, on the contrary, push their faith to the extent of fanaticism, and give equal value to the essential and accessory parts of the system, or even cling to hypotheses that merely lean against it or have been discarded. They often speak on molecular subjects with as much dogmatic assurance as though they had actually realised the ingenious fiction of Laplace—had constructed a microscope by which they could detect the molecules, and observe the number, form, and arrangement of their constituent atoms, and even determine the direction and intensity of their mutual actions. These things, which have been offered merely as hypotheses more or less probable, and to be taken for what they are worth as simple artifices of the intellect, are valuable, and have done good service in collocating facts and inciting to further careful investigations that one day or other may lead to a true chemical theory; but when perverted by being stated as actual truths, they falsify the intellectual education of students of inductive science, and bring reproach upon the modern progress of chemical science."

We learned a great deal from Italy in the Middle Ages, and may yet learn more. I earnestly commend the above lesson to some of our laboratory aspirants, who are occupying themselves in ringing the changes upon organic compounds, and who afterwards describe their atomic achievements as glibly, mechanically, and confidently, as though they had been laying bricks or piling shot.

An interesting paper (a note it is modestly called) on "The Absorbent Power of Red Phosphorus" is contributed to the May number, by Fausto Sestini, from the Laboratory of the Royal Technical Institute of Udine. (Udine is a small town, smaller than Croydon, and situated about 70 miles N.E. of Venice. How many of such towns in England have Royal Technical Institutes with laboratories for original research?) The author finds that red phosphorus absorbs many substances without combining with them, after the manner of porous charcoal. Thus it may be made to take up 3·369 per cent. of iodine, a considerable quantity of sulphur, rosaniline, &c. This power of "chemical adhesion" may be easily and strikingly shown by shaking powdered red phosphorus in a test tube containing a coloured solution of iodine in bisulphide of carbon. When a sufficient quantity of phosphorus is used, the whole of the iodine is taken up and the solvent rendered colourless. Rosaline is similarly removed from an etherial solution, and a portion of it may be again recovered unaltered from the phosphorus by washing with alcohol.

The July number contains some further contributions by Sestini from the same laboratory, on the proportions of bisulphide of carbon, its solubility in water, and the compounds formed by its contact with aqueous solutions of the oxides of the metals of the alkaline earths. Also some interesting communications from the laboratory of the University of Siena by Prof. G. Campani, among which is one showing that the absorption bands of an ammoniacal solution of carmine so closely coincide with those of blood, as to be undistinguishable in a spectroscope with a scale of twenty degrees. Mr. Sorby will probably be able to tell us whether any difference is distinguishable by more minute examination.

Lieben and Rossi contribute a series of rather important papers on some of the alcohols, and besides these there are some of the ordinary miscellaneous contributions to organic chemistry. W. MATTIEU WILLIAMS

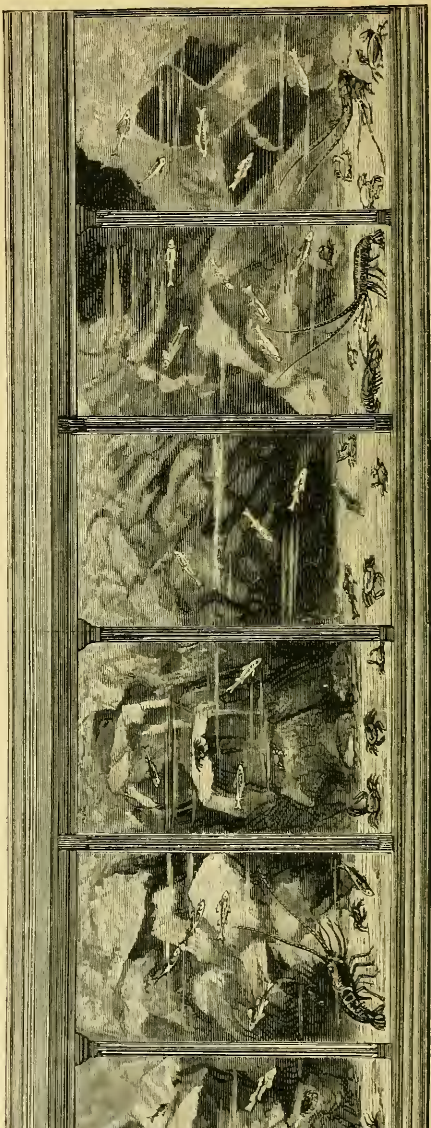
## THE CRYSTAL PALACE AQUARIUM

IN NATURE of April 20 last appeared a short paragraph, stating that this "enterprise, of which great scientific use can certainly be made," was taking form, and that when some of the marine animals were introduced, and the thing was in working order, a description of it would be given.

The building undertaken by the Crystal Palace Aquarium Company was commenced in July 1870, much too late therefore to be opened when at first contemplated, April 1, 1871, though at Easter last half a dozen of the marine tanks were temporarily converted into freshwater ones, and some pike, tench, carp, eels, &c., were shown therein for three days; when the place was closed, and the progress of the works continued, and then the establishment was finally opened on August 22, 1871. It would have been well if the sea-water had been in good condition in the early part of the summer, so that advantage might have been taken of the then exceptionally cool weather to transport some of the great abundance of animals at that time on the coasts of England; but that was not possible, and then, when the water *was* fit, the weather became very hot, and the sending of many animals was thereby prevented. Such creatures as could be got, however, were obtained, and the opportunity is now being taken of the present increasingly colder season to add other animals constantly, so that in a short time most of the tanks will be populated.

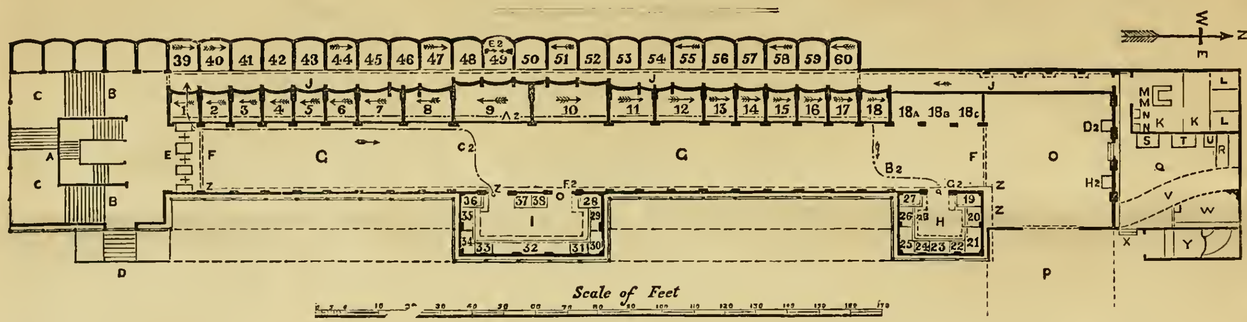
The accompanying plan, on page 471, drawn to a scale of about 50 ft. to 1 in., shows the ground occupied by the Aquarium and its adjuncts to be nearly 400ft. long and 70ft. broad, and it is situated at the northern end of the Palace, on a portion of the site of it burnt in 1865. It is of one story high, and, therefore, this ground plan shows everything, except the sea-water reservoir beneath the Saloon G G, extending under its whole width, and running below Tanks 9 and 10, and going lengthwise from E to H2. This reservoir contains 80,000 gallons of sea-water, and the tanks above contain 20,000 gallons, in all 100,000, gallons weighing a million pounds; and the fact of the aggregate contents of the tanks being only one-fourth of the contents of the reservoir, is extremely serviceable in keeping the water clear, as, supposing the water in, say tank 10 (holding 4,000 gallons), became turbid from any cause, it can be emptied by syphons in less than an hour into the reservoir, where so comparatively small a quantity of fluid would not appreciably disturb the purity of so great a mass, from which, in less than half an hour, No. 10 can again be filled, and thus all the tanks where animals exist, are, by being constantly pumped into, day and night, from the large, clear, and cool reservoir below, where there are no creatures, kept ever in good order. The main aëration which is thus depended on for the health of the creatures, is by these means produced by mechanical agitation, and the quantity of sea-weed necessary to decompose the poisonous carbonic acid gas evolved from the animals, which could not be effected by mechanical agitation, is grown upon the rocks of the aquarium by the action of light on the spores of algæ existing invisibly in the water. As the motion of the water needs to be incessant, all the machinery is in duplicate, there being two boilers, each of four horse power, two steam-engines, each of three horse power, and two of Forbes's patent pumps, and one of each is kept ever in action, the other being in reserve in case of accident. The sea-water issuing from the pumps at the rate (indicated by a counter, while a tell-tale clock furnishes evidence of the attention of three enginemen, each working for eight consecutive hours) of from 5,000 to 7,000 gallons an hour, passes in the first place into the two highest tanks, 9 and 10, half into each, and from thence it runs, diverging north and south, as far as tanks 18 and 1. From 18 it flows into 60, and from 1 into 39, in each case passing

FRONT VIEW OF TANK NO. 10 (18 FEET LONG), CRYSTAL PALACE AQUARIUM.



FRONT VIEW OF TANK NO. 9 (18 FEET LONG), CRYSTAL PALACE AQUARIUM.

## PLAN OF CRYSTAL PALACE AQUARIUM.




## KEY TO PLAN.

**A** Staircase from Palace.
**BB** Staircase to Palace.
**CC** Storerooms below Staircases.
**D** Communication with Palace Grounds (public).
**E** Turnstiles.
**FF** Screens at north and south ends of Saloon.
**GG** Saloon, containing Marine Tanks 1 to 18, and the three projected Fresh-water Tanks, 18A, 18B, 18C.
**H** North Room, containing Marine Tanks 19 to 27.
**I** South Room, containing Marine Tanks 28 to 38.
**JJJ** Attendants' Gallery, containing reserved Marine Tanks 39 to 60. (Private.)

**KK** Room, containing as follows:—
**LL** Two Steam Boilers, and
**MM** Two Steam Engines, and
**NN** Two Steam Pumps.
**O** Junction with Conservatory.
**P** Part of Conservatory (upper end).
**Q** Workroom.
**R** Slab for preparing Food for Animals.
**S** Store Cupboard.
**T** Slab.
**U** Sink.
**V** Flue.

**W** Office.
**X** Communication with Palace Grounds (private).
**Y** Heating Apparatus room.
**ZZ** Heating Pipes.
**A2** Sea-water Pipes supplying Tanks 1 to 18.
**B2** Sea-water Pipe supplying Tanks 19 to 27.
**C2** Sea-water Pipe supplying Tanks 28 to 38.
**D2** Point of issue of Sea-water from Reservoir to circulating system.
**E2 F2 G2** Three points of entrance of Sea-water from circulating system to Reservoir.
**H2** Float showing height of Sea-water in Reservoir.

*The direction of flow of Sea-water in the Tanks is shown by arrows, which for want of space are omitted in Tanks* 19 *to* 38, 41 *to* 43, 45, 46, 48, 50, 52 *to* 54, 56, 57, *and* 59.

below the floor of the gallery J J J, and then the two currents converge centrally and fall together into the reservoir at tank 49. A portion of the water, however, is arrested above tank 9, and is by separate pipes conveyed into the rooms H and I, where, after circulating in the tanks 19 to 38, it finds its way to the reservoir below at F 2 and G 2. Independently, however, of the simple fall of water from one tank to another in steps of from 3 to 6 inches in height in the series 1 to 18 (tanks 9 and 10 being 6 feet high, while 1 and 18 are 3 feet high—all internally), other streams of water, mixed with great quantities of air in minute bubbles, are driven from the main pipe into all the tanks with force. through jets, so that myriads of such bubbles, controlled by stopcocks, are forced in a state of fine division (resembling falling sand, or steam) nearly or quite down to the bottom of each tank, and thus the fluid is charged with as much atmospheric air as it will take up in open vessels. The amount of aëration (which also depends mucb on the amount of water entering) varies much, according to the dimensions of the tanks. Thus, tank 10 holds 4,000 gallons, and tank 1 holds 400 gallons, and, as tank 10 has a stream equal to its own bulk running through it once an hour, it necessarily follows that as the same current flows through tank 1 (of only one-tenth the capacity of tank 10) then tank 1 has a stream equal to its bulk, ten times as often as tank 10, that is to say, once every six minutes, and as these grades of aëration vary in all the tanks, they can be chosen according to the varying requirements of different kinds of animals. There is no intention to change the sea-water, but only to add from time to time a requisite quantity of distilled water to compensate for evaporation, and also to add whatever constituents the animals may deprive the sea-water of. For example, lobsters, crawfish, crabs, oysters, annelides with calcareous tubes, and many other animals, are constantly making new shells or adding to their old ones, and the matter is derived from the sea-water, and must be re-supplied.*

The material used for the pumps, stop-cocks, and jets, and for nearly all the pipes (the exceptions being the stoneware pipes connecting tanks 39 to 60) is vulcanite, or hard india-rubber. This was recommended by Prof. Faraday for the purpose in 1857.

In tanks 1 to 18 the creatures can be viewed only through the plate-glasses forming the fronts of the tanks; but in the twenty tanks of the rooms H and I (Nos. 19 to 38), which are made to contain small specimens, the view is through the surface of the water, as well as through the glasses of the fronts, as in the table-cases of a museum. The shallowness of these tanks, varying in water-capacity from 40 to 270 gallons each, much increases both their aëration and the accessibility of the objects they contain, and the much-shaded position of some of them, *e.g.*, Nos. 19, 21, 25, 27, 28, 30, 34, and 36, affords means of maintaining some organisms, both animal and vegetable, needing an unusual amount of darkness. For example, no green algæ (*Chlorosperms*) will grow in the gloom of these tanks, while they are admirably suited for the *Rhodosperms* (or red algæ) which always flourish best in much obscurity. So, too, no direct sunlight can enter tank 1, and as it contains only sea-anemones, it may be expected that this intentional arrangement will somewhat retard the usual fading of some of the colours of these animals when in aquaria. Tanks 1 to 18 are lighted from a source not seen by spectators in front of the glasses.

In tanks 39 to 60 the view is only through the surface of the water. These twenty-two receptacles, each holding about 300 gallons of sea-water, contain, or are intended to contain, creatures which are at intervals drafted into the show tanks (1 to 38) and, acting as reserves and not for public inspection, they enable large numbers of animals to be purchased when they are to be cheaply and easily got, and thus these store-places in part remove the uncertainty of supply, which hitherto has attended inland marine aquaria. They are also used to keep living food, as mussels and shrimps, for the other animals.

For the general supply of the aquarium, the company possesses a large marine pond, in communication with the sea at every tide, and serving as a store, with a resident agent (Mr. C. Rogers), at Plymouth. This pond is capacious enough to furnish many animals, otherwise hard to be got, to all the public aquaria in Europe. The company has another agent (Mr. John Thompson) and store-place, at Southend, Essex; and supplies are obtained also from Weymouth, from Mr. R. T. Smith; from Menai in North Wales, from Mr. E. Edwards; from Tenby, in South Wales, from Mr. W. Jenkins, together with other contributions from North and South Devonshire and the Channel Islands. Notwithstanding all these facilities, however, the difficulty of procuring animals in good health, and of sufficient variety, and of right size, is very great—so great, indeed, on account of periods of excessive heat or cold, or rough weather, that there are probably not more than a dozen or fifteen weeks of any average year (with seldom a couple of weeks consecutively) in which animals can be most advantageously got, and this applies especially to fishes.

The animals at present in the aquarium are the following*:—Sea-anemones, fourteen species; tube and other worms, six species; star-fishes, three species; sea-urchins, lobsters, crawfish, edible-crabs, spider-crabs, swimming crabs, and various other crabs; prawns, two species; barnacles, oysters, mussels, cockles, and scallops; whelks, periwinkles, dogwinkles, and tops; cuttles, two species; and many fishes, as skate, angel-fish, launce, pipe-fish, lump-fish, and sucking-fish; sole, plaice, cod, whiting-pout, whiting, and rockling; wrasse, four species; goby, three species; blenny, three species; dragonet, gunnel, grey-mullet, sea-bream, sea-scorpion, two species; pogge, gurnard, weever, and basse. All of these have to be fed constantly, many of them hourly, throughout the day, except on Sundays; and as for the sea-anemones, of which there are already in the aquarium over 3,000 individuals, everyone of them has a morsel of food proportioned to its size given it at frequent intervals with a pair of wooden forceps, by an attendant whose sole occupation this is, as these flower-like creatures being so very non-locomotive as to be almost absolutely fixed, cannot pursue their food, or in an aquarium obtain it in any other manner, they being deprived of the actual ocean, every wave of which, when the animals are in a state of nature, bringing them nutriment which is arrested by their outspread and waving tentacles.

* The sea-water was supplied in casks by Mr. W. Hudson, of Brighton.

* *Marine Animals in the Crystal Palace Aquarium, from August* 20 *to October* 10, 1871.—1. Actinoloba dianthus. 2. Sagartia bellis. 3. Sagartia miniata. 4. Sagartia rosea. 5. Sagartia venusta. 6. Sagartia nivea. 7. Sagartia troglodytes. 8. Sagartia viduata. 9. Sagartia parasitica. 10. Anthea cereus. 11. Actinia mesembryanthemum. 12. Bunodes Ballii. 13. Tealia crassicornis. 14. Cerianthus Lloydii. 15. Uraster rubens. 16. Cribella oculata. 17. Solaster papposa. 18. Sipunculus Bernhardus. 19. Nemertes Borlasii. 20. Terebella conchilega. 21. Sabella reniformis. 22. Sabella unispira. 23. Terebella penicillus. 24. Sabella tubularia. 25. Serpula contortuplicata. 26. Serpula triquetra. 27. Spirorbis communis. 28. Gammarus locusta. 29 Palæmon serratus. 30. Palæmon squilla. 31. Crangon vulgaris. 32. Homarus marinus. 33. Palinurus quadricornis. 34. Pagurus Bernhardus. 35. Galathea strigosa. 36. Galathea squamifera. 37. Porcellana platycheles. 38. Pinnotheres pisum. 39. Portunus puber. 40. Portunus depurator. 41. Carcinus Mænas. 42. Pilumnus hirtellus. 43. Xanthis florida. 44. Xanthis rivulosa. 45. Cancer pagurus. 46. Maia Squinado. 47. Hyas araneus. 48. Inachus Dorsettensis. 49. Stenorhynchus phalangium. 50. Balanus balanoides. 51. Lepas anatifera. 52. Ascidia mentula 53. Cardium echinatum. 54. Mytilus edulis. 55. Anomia ephippium. 56. Eolis coronata. 57. Aplysia punctata. 58. Purpura lapillus. 59. Buccinum undatum. 60. Nassa reticulata. 61. Murex erinaceus. 62. Sepiola Rondeletii. 63. Octopus vulgaris. 64. Raia batis. 65. Squatina angelus. 66. Scyllium canicula. 67. Hippocampus brevirostris. 68. Syngnathus acus. 69. Ammodytes lancea. 70. Anguilla acutirostris. 71. Cyclopterus lumpus. 72. Liparis vulgaris. 73. Solea vulgaris. 74. Platessa vulgaris. 75. Motella vulgaris. 76. Merlangus vulgaris. 77. Morrhua vulgaris. 78. Labrus maculatus. 79. Labrus mixtus. 80. Crenilabrus melops. 81. Crenilabrus rupestris. 82. Callionymus lyra. 83. Gobius niger. 84. Gobius unipunctatus. 85. Gobius Ruthensparri. 86. Blennius pholis. 87. Blennius gattorugine. 88. Muraenoides guttata. 89. Zoorces viviparus. 90. Mugil capito. 91. Pagellus centrodontus. 92. Tottus bubalis 93. Cottus scorpius. 94. Asphidophorus cataphractus. 95. Criglia hirundo. 96. Labrax lupus.

The food consumed by a very few of the animals now present in the aquarium is vegetable, consisting of green seaweeds (*Ulva*, *Porphyra*, *Enteromorpha*, &c.), but by far the greater number have animal food given them. This consists of shrimps (alive or dead), crabs, mussels, oysters, and fish, but "butcher's meat" they never get. The large amount of organic matter thus continually (from 8 A.M. till 6 P.M. on six days a week) placed in the water, and the correspondingly great quantity of excrementitious matter resulting from it, is nearly all rendered harmless by being decomposed chemically by the oxygenation of the streams of water, and by the growing vegetation, without the use of any filter, and without the water being made turbid. In fact, the circulating system of the water in this aquarium is similar to, and avowedly made on the general model of, the circulating system of the blood of many of the animals which the aquarium itself maintains in life and health. Thus, the steam engine represents a heart, the coals consumed by the boilers are the food, the pipes are the veins and arteries, and the wide spreading air-charged streams of water discharged at the jets are the lungs.

Very few deaths occur, and the condition of the creatures will be further improved when the vegetation will have grown more. There are, however, reasons for supposing that not in any aquarium yet devised can any pelagic animals be permanently kept, and that therefore the bulk of specimens must be littoral creatures. But there are many marine animals and plants, both of the deep sea and the shore, which at present cannot be kept in captivity at all. The reason of this is in some cases known, but with others there is not the smallest clue as to the means to be adopted for their successful maintenance.

In front of tanks 1 to 18 are placed obliquely, and over tanks 19 to 38 are suspended vertically, glazed frames to contain drawings of the animals. These pictures will be numbered to correspond with the numbered descriptive paragraphs of a guide-book now being prepared for the aquarium, so that any animal can be readily found. Although tank No. 1 contains exclusively sea-anemones, and thus properly commences with the lower animals, yet the classification of the creatures throughout the building is not made with reference to any acknowledged system founded on organisation, but the creatures are, so far as the limits of the place permit, arranged with reference to *habits* rather than *structure*, and in such manner that, as much as possible, one animal shall not interfere injuriously with another.

The building is very cool in summer. Thus, during the hottest part of the season just passed through, when the true temperature of the general atmosphere in the shade was 88° F., that of the air in the aquarium was only 68° F., and the sea-water never rose higher than 63° F. For winter, hot-water pipes are arranged to maintain the temperature of the air from 60° F. to 65° F.

The ventilation everywhere is remarkably good, and there is no tank in any of the entire series of sixty, which cannot be brought into free contact, when needed, with the open air.

The amount of daylight can also be very exactly regulated; and as, for the exhibition of the aquarium on winter evenings, it will be necessary to use powerful artificial illumination, some experiments are now being made on the best mode of lighting it, but it is not precisely known what will be the behaviour, in artificial light, of animals a great number of which are more or less nocturnal. Indeed, in an aquarium the difficulty ever is to show animals which endeavour to avoid being seen.

The architect, Mr. C. H. Driver, of Victoria Street (the builders being Messrs. Jackson and Son),* has shown much ingenuity in turning to good account every part of the space placed at his disposal, and in his simplicity of design he has not disobeyed any law of service in construction, in any case. Everything is done with a meaning, and with a definite and obvious purpose. Thus, as animals cannot exist with comfort without rock-work in the tanks, it has been plentifully introduced; but whatever picturesqueness of form it possesses, is merely a consequence of its being in the first place useful, and so strictly and severely is this principle carried out, that such rock-work does not project anywhere even an inch above the water's level, instead of being employed, as in most Continental aquaria, that of Berlin in particular, in the spectator's part of the building, where it is not wanted, and where, being perfectly useless, it is therefore ugly, and is merely an expensive excrescence. Everything in the Crystal Palace Aquarium is made to look like what it is, and not like something else, and not to pretend to be some other and more expensive material. Thus, if deal wood for its preservation is necessary to be painted, it is not also grained to look like oak or walnut-wood. Nor is cement squared withi mitation masonry joints, or otherwise treated so as to look like stone. Nor is there any use of sham marble. It was certainly deemed advisable to make the building externally to correspond in general appearance with the arched and other iron framings which compose the Crystal Palace adjoining, and in which the glass of that edifice is set, but even then, this framing on the outside of the aquarium walls is employed usefully to strengthen those walls, which are purposely made insufficiently strong if such framing were absent. And wherever, either outside or inside the place, a little enrichment has been indulged in, it properly consists only in *the decoration of construction, and not in the construction of decoration.* Systematic economy in this Aquarium is in fact throughout observed in such manner that the largest number and variety of animals may be preserved in the best condition in the smallest space.

The two woodcuts on page 470, each on a scale of half an inch to one foot, represent the pair of largest tanks, Nos. 9 and 10, inhabited by crawfish and other crustaceans, and by wrasse, grey mullet, and other fishes. The front of each tank is composed of three pieces of glass, divided and supported at equal distances from either end by two large vertical mullions of slate and iron, and subdivided by three other and smaller vertical mullions of iron only. These six glasses, each measuring six feet square and one inch thick, are among the heaviest polished plates made in this country, by Messrs. Goslett and Co., and the water pressure on their aggregate surfaces amounts to 46,656 lbs., or nearly twenty-one tons.

W. A. LLOYD

* "Buildings for scientific purposes should be plain and useful above all things, *in appearance as in fact*."—PROF. RUSKIN.

## THE BIRDS OF THE LESSER ANTILLES*

THE Lesser West Indian Islands, although mostly belonging to Great Britain and inhabited by a large number of intelligent colonists, and moreover easily accessible from our shores by a regular fortnightly line of packets, have hitherto been strangely neglected as regards their zoology. Of their botany we have an excellent account by Dr. Grisebach, published under the energetic superintendence of the authorities of the Herbarium at Kew. I am anxious to call the attention of the students of NATURE to what an interesting field here lies available for investigation,—particularly as regards the ornithology of these islands.

The West Indian Islands seem to me to constitute a distinct subdivision of the neo-tropical region, which may be called the *Sub-regio Antillensis.* This sub-region is divisible into two portions, which correspond to the two usually recognised divisions of the islands into the Greater and Lesser Antilles. The former of these is characterised by the presence of the remarkable mammal-forms *Sole-*

* Principally extracted from a paper read before the Zoological S on March 21, 1871.

*nodon*, *Capromys*, and *Plagiodon*; and by several peculiar types of ornithic life, such as *Spindalis*, *Sporadinus*, *Todus*, and *Saurothera*, which run on as far as Porto Rico, but do not cross into the Lesser Antilles. The latter, if we put the Chiroptera aside, present but few traces of mammal-life, except one or two species of Agouti (*Dasyprocta*) and Mouse (*Hesperomys*), but are tenanted by certain characteristic forms of birds, such as *Ramphocinclus*, *Cinclocerthia*, *Orthorhynchus*, and *Eulampis*, which are not found in the Greater Antilles.

The ornithology of the Greater Antilles is now tolerably well known to us, although specimens from most of the islands are rare in collections and difficult to obtain. The Lesser Antilles, on the other hand, are still very imperfectly investigated as regards their birds, many of them being, so far as I know, still unvisited by any naturalist or collector. There can be no doubt, however, that every one of them is well worthy of being worked at, and that the results to be obtained from a thorough examination of the whole group would be of great importance towards a more complete knowledge of the laws of distribution. To show how slight our acquaintance is with this subject and how much remains to be done, I will mention the principal islands or island-groups in order, and specify what knowledge we have of their ornithology.

1. *The Virgin Islands.*—Out of these islands we may, I think, assume that we have a fair acquaintance with the birds of St. Thomas, the most frequently visited of the group, and the halting-place of the West Indian mail-steamers. Mr. Riise, who was long resident here, collected and forwarded to Europe many specimens, some of which were described by myself,* and others are spoken of by Prof. Newton, in a letter published in the *Ibis* for 1860, p. 307. Mr. Riise's series of skins is now, I believe, at Copenhagen. Frequent allusions to the birds of St. Thomas are also made by Messrs. Newton in their memoir of the birds of St. Croix, mentioned below. In the "Proceedings of the Academy of Natural Sciences of Philadelphia," for 1860, p. 374, Mr. Cassin has given an account of a collection of birds made in St. Thomas by Mr. Robert Swift, and presented to the Academy: twenty-seven species are enumerated.

Quite at the extreme end of the Virgin Islands, and lying between them and the St. Bartholomew group, is the little islet of Sombrero, "a naked rock about seven-eighths of a mile long, twenty to forty feet above the level of the sea, and from a few rods to about one-third of a mile in width." Although "there is no vegetation whatever in the island over two feet high," and it would seem to be a most unlikely place for birds, Mr. A. A. Julien, a correspondent of Mr. Lawrence, of New York, succeeded in collecting on it specimens of no less than thirty-five species, the names of which, together, with Mr. Julien's notes thereupon, are recorded by Mr. Lawrence in the eighth volume of the "Annals of the Lyceum of Natural History of New York" (p. 92). The remaining islands of the Virgin group are, I believe, most strictly entitled to their name, so far as ornithology is concerned, for no collector on record has ever polluted their virgin soil. Prof. Newton (*Ibis*, 1860, p. 307) just alludes to some birds from St. John, in the possession of Mr. Riise.

2. *St. Croix.*—On the birds of this island we have an excellent article by Messrs. A. and E. Newton, published in the first volume of the *Ibis.*† This memoir being founded on the collections and personal observations of the distinguished authors themselves, and having been worked up after a careful examination of their specimens in England, and with minute attention to preceding authorities, forms by far the most complete account we possess of the ornithology of any one of the Lesser Antilles. It, however, of course requires to be supplemented by additional observations, many points having been necessarily left undetermined, and it is much to be regretted that no one seems to have since paid the slightest attention to the subject.

3. *Anguilla, St. Martin, and St. Bartholomew.*—Of this group of Islands St. Bartholomew alone has, as far as I know, been explored ornithologically, and that within a very recent period. In the Royal Swedish Academy's "Proceedings" for 1869 will be found an excellent article by the veteran ornithologist, Prof. Sundevall, on the birds of this island, founded on a collection made by Dr. A. von Goës. The species enumerated are forty-seven in number, amongst which the most interesting perhaps is the *Euphonia flavifrons*, originally obtained, along with one or two other species, in the latter part of the last century, and figured by Sparrman in his "Museum Carlsonianum," along with several other species from the same island.

4. *Barbuda.*—Of this British island I believe I am correct in saying that nothing whatever is known of its ornithology, or of any other branch of its natural history.

5. *St. Christopher* and *Nevis*, to which may be added the adjacent smaller islands *St. Eustathius* and *Saba.*—Of these islands also our ornithological knowledge is of the most fragmentary description. Mr. T. J. Cottle was, I believe, formerly resident in Nevis, and sent a few birds thence to the British Museum in 1839. Amongst these were the specimens of the Humming-birds of that island, which are mentioned by Mr. Gould in his well-known work. Of the remainder of this group of islands we know absolutely nothing.

6. *Antigua.*—Of this fine British island I regret to say nothing whatever is known as regards its ornithology, Amongst the many thousands of American birds that have come under my notice during the past twenty years, I have never seen a single skin from Antigua.

7. *Montserrat.*—Exactly the same as the foregoing is the case with the British island of Montserrat.

8. *Guadaloupe*, *Deseadea*, and *Marie-galante.*—An excellent French naturalist, Dr. l'Herminier, was for many years resident as physician in the Island of Guadaloupe. Unfortunately, however, he never carried into execution the plan which I believe he contemplated of publishing an account of the birds of that island. He sent a certain number of specimens to Paris and to the late Baron de la Fresnaye, to whom we are indebted for the only article ever published on the birds of Guadaloupe,* or of the adjacent islands.

9. *Dominica.*—Dominica is one of the few of the Caribbean Islands that has had the advantage of a visit from an active English ornithologist. Although Mr. E. C. Taylor only passed a fortnight in this island in 1863, and had many other matters to attend to, he nevertheless contrived to preserve specimens of many birds of very great interest, of which he has given us an account in one of his articles on the birds of the West Indies, published in the *Ibis* for 1864 (p. 157). It cannot be supposed, however, that the birds of this wild and beautiful island can have been exhausted in so short a space of time, even by the energetic efforts of our well-known fellow-labourer.

10. *Martinique.*—This island is one of the few belonging to the Lesser Antilles in which bird-skins are occasionally collected by the residents, and find their way into the hands of the Parisian dealers. There are also a certain number of specimens from Martinique in the Musée d'Histoire Naturelle in the Jardin des Plantes, which I have had an opportunity of examining; but beyond the vague notices given by Vieillot in his "Oiseaux de l'Amérique du Nord," I am not aware of any publication relating specially to the ornithology of this island. Mr. E. C. Taylor passed a fortnight in it in 1863, and has recorded his notes upon the species of birds which he met with in the excellent article which I have mentioned

* Ann. N. H. ser. 3, vol. iv. p. 225; and P. Z. S. 1860, p. 314.
† Ibis, 1859, pp. 59, 138, 252, and 365.

* *Rev. Zool.* 1844, p. 167.

above, but these were only few in number. The International Exhibition in 1862 contained, in the department devoted to the products of the French colonies, a small series of the birds of Martinique, exhibited by M. Bélanger, Director of the Botanical Garden of St. Pierre, in that island.* This is all the published information I have been able to find concerning the birds of Martinique.†

12. *St. Lucia.*—Of this island I believe there is no published ornithological information whatever. The little knowledge of its avifauna which I possess is derived from two sources: first, a few specimens in the Paris Museum obtained by Bonnecourt, a French collector, who visited the island in 1850 and 1851 on his way to Central America; and, secondly, a small series of unpublished coloured drawings in the library of the Zoological Society by Lieut. Tyler, who formerly contributed to the "Proceedings" some notes on the reptiles of that island.‡ These drawings although rough and unfinished, are characteristic and mostly recognisable.

13. *St. Vincent.*—St. Vincent was formerly the residence of an energetic and most observant naturalist, the Rev. Lansdown Guilding, F.L.S., who, however, unfortunately died at an early age in this island without having carried out his plans for a fauna of the West Indies.§

Mr. Guilding paid most attention to the invertebrate animals, but his collection contained a certain number of birds, amongst which was a new parrot, described after his decease by Mr. Vigors as *Psittacus Guildingii*, and probably a native of St. Vincent.

14. *Grenada and the Grenadines.*—Of the special ornithology of this group nothing is known.

15. *Barbados.*—The sole authority upon the birds of Barbados is Sir R. Schomburgk's well-known work on that island.|| This contains (p. 681) a list of the birds met with, accompanied by some few remarks. It does not, however, appear that birds attracted much of the author's attention, and more copious notes would be highly desirable.

Although Tobago and Trinidad are geographically reckoned in the Windward division of the Lesser Antilles, they have zoologically, I believe, nothing whatever to do with them. Both have been peopled with life from the adjacent mainland; or, if in the case of Tobago this was not originally the case, it has been overrun with continental species, and, as well as Trinidad, now presents few, if any, traces of Antillean forms. Of the ornithology of both of these islands we have excellent accounts; of that of Tobago by Sir William Jardine,** from the collections of Mr. Kirk; and of that of Trinidad more recently from the pens of Dr. Léotaud †† and Dr. Finsch.‡‡

P. L. SCLATER

## REMARKS ON THE CLASSIFICATION OF FRUITS

TEACHERS and students alike must feel grateful to Dr. Dickson for his "Suggestions on Fruit Classification." The number of names applied to varieties of fruit renders the study most laborious; and as many of the varieties are closely related, the useless names ought at once to be got rid of. One thing strikes me as being a defect in Prof. Dickson's classification, and that is the employment of certain of the terms in two different ways. For example he uses the terms Achæne, Berry, and Drupe, in a broad and in a restricted sense. In a broad sense as the name of the *genus*, if one may so speak, and again uses the same word as a trivial name—a *species* as it were of the genus. The same is also true of his group of capsules, only he thinks a new name might be given to the fruits generally called capsules. It is unfortunate that four out of his five groups should be open to such an objection, and every teacher will at once be able to appreciate the difficulty which the student must have when the same word is used both in a broad and in a restricted sense. The term Schizocarp seems to be a very admirable one, and I do not think the terminology of fruits would be in any way burdened if a few more resembling it were used. It is not without a very great deal of hesitation that I venture to suggest that new terms should be applied to Dr. Dickson's four groups, Capsule, Achæne, Berry, and Drupe. I think that it is much less objectionable to introduce a few more terms, if distinctive and apposite, than resort to the difficult, and at all times confusing, expedient of using these words in a double sense. Taking the word Schizocarp as a type, I venture to suggest the term Achænocarp for the group of Achænes as used by Dr. Dickson, thus avoiding all confusion, and allowing the term Achæne to remain in its restricted sense. Regmacarp I would apply to the group of capsules, using the term capsule for one division of the group. Pyrenocarp seems applicable to the drupes, and Coccocarp to the berries. The derivation of these terms at once explains their application. Achænocarp, from *a*, privative; *chaino*, I open; and *karpos*, fruit. Regmacarp from *regma*, a rupture, in allusion to the dehiscence. Pyrenocarp from *pyren*, the stone of the fruit; and Coccocarp from *kokkos*, a berry. In using these terms I would employ them in the following manner:—

I. Dry Indehiscent Fruits.
- 1. Achænocarps. Carpels one or few-seeded.
  - A. Glans. Pericarp hard and thick.
  - B. Achæne. Pericarp thin. Including the varieties Caryopsis and Cypsela.
- 2. Schizocarps. Carpels breaking up into indehiscent portions.
  - A. Carcerulus. Breaking longitudinally, no forked carpophore.
  - B. Cremocarp. Breaking longitudinally, a forked carpophore.
  - C. Lomentum. Breaking transversely.
  - D. Dischisma. Breaking longitudinally, and then transversely. Fruit of Platystemon. The term, which is new, is derived from *dis* and *schisma*, a division.

II. Dry Dehiscent Fruits.
- 3. Regmacarps.
  - A. Follicle. Simple, dehisces by one suture.
  - B. Legume. Simple, dehisces by both sutures.
  - C. Capsule. Compound, dehisces longitudinally, transversely, or by pores.
  - D. Regma. Compound, dehisces by rupture along inner angle of lobes.

III. Succulent Indehiscent Fruits.
- 4. Pyrenocarps. Endocarp indurated.
  - A. Drupe. One stone, simple or plurilocular.
  - B. Pome. Two or more-stoned, ovary superior or inferior.
- 5. Coccocarps. Seeds in a pulp.
  - A. Uva. Ovary superior, thick or thin skinned.
  - B. Bacca. Ovary inferior, thick or thin skinned.

IV. Succulent Dehiscent Fruits.
- A. Succulent Capsule. *e.g.*, Æsculus, Balsamina.
- B. Dehiscent Drupe. *e.g.*, Walnut.
- C. Dehiscent Berry. *e.g.*, Nutmeg, Squirting Cucumber, *Nuphar advena*.

W. R. MCNAB

* See article on Ornithology in the International Exhibition, *Ibis*, 1862, p. 288.
† On animals formerly living in Martinique, but now extinct, see Guyon, Compt. Rend. lxiii., p. 589 (1866)
‡ See P. Z. S. 1849 and 1850.
§ See his sketch of his plans, "Zool. Journ.," ii. p. 437. He died in 1832.
|| "History of Barbados." London, 1847.
** Annals of Nat. Hist., vols. xviii., xix., xx. (1846-47).
†† Oiseaux de l'île de la Trinidad, Port of Spain, 1866.
‡‡ See Proc. Zool. Soc., 1870, p. 552.

## NOTES

We greatly regret to have to announce that the state of the venerable Prof. Sedgwick's health is such that he will be unable to deliver his usual course of lectures during the ensuing academical year. His place will be filled *pro tem.* by Mr. John Morris, Professor of Geology at University College, London. Though we cannot but regret the cause which has taken Prof. Morris to Cambridge, his nomination by Prof. Sedgwick to serve as his deputy is a cause of congratulation to the University.

In Sir John F. Burgoyne, F.R.S., who died on Saturday last, in the 90th year of his age, the English army has lost the most eminent man of science among her officers. In both civil and military capacities, as chairman of the Board of Public Works in Ireland from 1830 to 1845, and at the Siege of Sebastopol, he evinced engineering talents of no ordinary kind. Sir John Burgoyne's only son perished in the *Captain*, being in command of that ill-fated vessel.

Dr. Henry S. Wilson has been appointed Demonstrator of Anatomy at the University of Cambridge. Dr. Wilson formerly held a similar office in the University of Edinburgh.

The Professor of Chemistry at Cambridge, Mr. Liveing, will give instruction in practical chemistry on Tuesdays, Thursdays, and Saturdays at 1 P.M. The instruction will be given at the University Laboratory. The Laboratory will be open for students daily from ten A.M. until six P.M. The Demonstrator (Mr. Hicks, B.A.) will attend to give instruction on mornings and afternoons alternately. The Professor of Chemistry will deliver a course of lectures in Spectrum Analysis and some other special branches of chemistry on Tuesdays, Thursdays, and Saturdays at noon, commencing on October 26, in the Chemical Lecture-room, next Downing Street. No fee will be required of those who do not wish for a certificate.

The Oxford School of Science and Art, in connection with the Science and Art Department of the Council on Education, South Kensington, has been granted the use of the New University Museum at Oxford, where lectures will be given this month on Mathematics (Elementary), Magnetism and Electricity, Animal Physiology, and Inorganic Chemistry. We regard this act of the University as one of very good omen.

The combined examination held by Magdalen and Merton Colleges for scholarships in mathematics and natural science terminated on Saturday, when the following elections were declared :—Magdalen College : Demyship in Mathematics—Mr. R. R. Corkling, Manchester Grammar School. Demyships in Natural Science—Mr. E. Steel, Manchester Grammar School ; Mr. G. R. Christie, Magdalen College School. *Proxime accesserunt* for natural science demyship—Mr. Hamsworth, Mr. Hopwood, Manchester Grammar School. Merton College : Mathematical postmastership—Mr. F. G. Stokes, Cowbridge. Natural Science postmastership—Mr. Lane, Cheltenham Grammar School. There were fourteen candidates for the mathematical and sixteen candidates for the natural science foundations.

At the Oldham School of Science and Art, three Queen's medals have been awarded by the Department to the artisan students of this school, the silver medal for mathematics to John Armitage ; a bronze medal for machine construction and drawing to John Robertson ; a bronze medal for applied mechanics to Thomas Marsden. Mr. Armitage has also gained a Whitworth Scholarship this year.

We have received the examination papers for the Scholarship and Exhibition in Natural Science recently award by St. Mary's Hospital Medical School. The questions appear to have been very carefully framed to show the attainments of the candidates in chemistry, physics, zoology, and botany, and we congratulate this young school on setting so admirable an example to its older sisters in encouraging a real knowledge of science among its students.

Earl Granville has shown his interest in scientific instruction by offering prizes in chemistry, mechanics, and mathematics to the examinees at the Margate centre of the Oxford Local Examinations.

To Sir John Lubbock, who has recently been twitted on his predilections for prehistoric man, we commend a letter which has recently appeared in the *Times* to the effect that a large section of the old Temple of Avebury has just been parcelled off into building allotments, and that the remainder is likely to be similarly dealt with before long. It may be that from a utilitarian point of view this can no longer matter, inasmuch as this celebrated remnant of the "Stone Age" has been so thoroughly wrecked that scarce anything now remains of it. According to Dr. Stukely, the Temple was nearly perfect in the time of Charles II., who visited it, and had plans and drawings made, copies of which are reproduced in Dr. Stukely's works. There were then standing between 200 and 300 stones, and it was, in his opinion, as superior to Stonehenge as a cathedral would be to a parish church. All that now remains of this wonderful monument, and of the two avenues, each of nearly a mile in length, by which it was approached, is about two-thirds of the great circular earthen mound by which it was enclosed and about twenty of the stones. The rest have been utilised by the inhabitants of the village to build their cottages, erect their parish church, make bridges, stone fences, and mend the road. It is said that a beershop was built out of a single stone. This is encouraging !

The death is announced of Mr. Thomas Pilgrim, engineer, who died on the 6th inst., at the age of seventy-one years, at his son's residence at Plumstead. For the last thirty-five years Mr. Pilgrim was intimately associated with Mr. Francis Pettit Smith, and with the introduction of the screw-propeller. He acted as chief engineer of the *Archimedes*, the first ship ever sent to sea propelled by the screw.

The annual Exhibition of Fungi was held at the Royal Horticultural Gardens, on Wednesday the 4th inst., and was decidedly better than any of its predecessors. Nearly all the British edible and poisonous fungi were shown in a living state, including several rare species. The visitors showed the greatest possible interest in the plants exhibited, and the Fungus exhibition was one of the best attended of the year. The prizes for the best collections of edible and poisonous species, offered by Mr. W. W. Saunders, were in the first place awarded to Mr. English and Mr. W. G. Smith ; but, through some informality on the part of these exhibitors, the first prize was ultimately conferred on Messrs. Hoyle and Austin, of Reading.

Marlborough College has taken an honourable lead among our public schools in the cultivation of science, and we therefore turned over with more than ordinary interest the leaves of the Report of its Natural History Society for the half-year ending Midsummer 1871, just received. We do not look to these reports for papers of original research that materially advance our scientific knowledge ; rather, for such as will in the first place show an accurate and careful observation of the phenomena of nature on the part of the writer ; and, secondly, that will promote the study of natural history among his hearers. We are disposed, therefore, to agree with the secretaries that the production at the meetings of these societies of papers which show a very limited amount of knowledge, if only such knowledge as is shown be the result of honest work, is better than having no papers at all, and to endorse their remark in the preface, that "failure is the indispensable ingredient of success ;

and that if the Society is worth anything at all, it is strong enough to outlive many unsuccessful papers." Judged by this standard, the Report before us is decidedly satisfactory. A few only of the papers read during the half are printed at length; but they contain evidence of much careful work in the various departments of natural science. Only one field day was held during the half; and the further publication of the new edition of the "Marlborough Flora" is postponed till the next number. The committee reports that the Botanical Garden, which was started last half in the corner of the Wilderness, has fully realised the hopes of its originators. We confidently expect from the Marlborough College Natural History Society a long career of usefulness, and no small share in moulding the scientific tastes of the rising generation.

IN our last week's number we referred to the Burmese hairy woman. A correspondent of the *Times* has supplied some additional information. He writes: "When I was at Mandalay in 1859, I saw the same woman and three of her children. The eldest and youngest were hairy like their mother, while the second, like his father, presented no such peculiarity. The husband was a man who report said had been induced to wed this woman to become possessed of the marriage portion which the King of Burmah had promised to bestow upon her on her bridal day. The bridegroom was a plucky individual at any rate, though his motives may have been somewhat mercenary. The hairy woman, whose name I now forget, had a pleasant and intelligent face—there was nothing whatever repulsive in it. The hair on the face and breast was several inches long; on the forehead it was parted in the middle, and blended with that of her head. Of a light brown colour on her cheeks, it paled gradually towards the bridge of her nose, and the centre of her lips, chin, and neck. Those of your readers who have a copy of Colonel Yule's narrative of the embassy to Ava will see a good likeness of the woman and a description of herself and family."

THE *Pall Mall* promises a novelty in literary publications. An English periodical is to be printed in Berlin, bearing the title of *The German Quarterly Magazine*. Its object is to make the treasures of German learning accessible to the English speaking public. Two of the most eminent literary men of Germany, Profs. Virchow and Von Holtzendorff, have undertaken its joint management, conducting the editing alternately, so as to offer in one number articles chiefly on Natural Science under the great physician's direction, and in the following essays on historical and political subjects published under M. Von Holtzendorff's supervision.

MESSRS. ASHER AND CO. announce for November, "Man in the Past, Present, and Future: a Popular Account of the Results of Recent Scientific Research, as regards the Origin, Position, and Prospects of the Human Race;" translated from the German of Dr. L. Büchner, by Mr. W. S. Dallas, F.LS.

THE first two parts are published of a new edition of Griffith and Henfrey's "Micrographic Dictionary." The names of the editors, Dr. J. W. Griffith, the Rev. M. J. Berkeley, and Prof. T. Rupert Jones, are a guarantee that the treatment of the various subjects will be carried down to the present state of scientific knowledge; and that the book will be indispensable to every biologist and student of the microscope.

MR. ROBERT GRAY, late Secretary to the Natural History Society of Glasgow, has issued a prospectus of his work shortly to be published, "The Birds of the West of Scotland, including the Outer Hebrides, with Occasional Records of the Rarer Species throughout Scotland generally." Since the publication of the works of Sir William Jardine, Prof. Macgillivray, and Mr. Selby, nothing in a collected form on the Birds of Scotland has been brought under the notice of ornithologists, and the book seems likely to fill a useful place in ornithological literature.

A NEW description of lamp for street lighting has recently been experimented on in London, the principle of which is the application of reflectors, in order to bend down and utilise the amount of light which is at present wasted by upward radiation. It is manifest that the rays of light from a street lamp which now strike the eye of a spectator placed on the ground are only a small portion of those actually emitted by the flames. The rays which pass through the upper portions of the sides of the lantern, or through its sloping roof, are entirely dissipated, or at best, if partially and imperfectly reflected by clouds or atmospheric particles, become visible only in the form of the red glow which overhangs a distant town. Mr. Skelton, the inventor, calculates that about two-thirds of the light given by the gas flame are in this way lost, and he has arranged strips of looking-glass in such a way that the loss will be effectually prevented. The upper half of each side of the lamp, and the whole of each side of the sloping roof, are occupied by a frame, in which the strips are placed with their reflecting surfaces downwards, in a manner somewhat analagous to the laths of a Venetian blind. The precise character of the effect produced will depend upon the distance of the strips apart, upon their width, and upon their angle of inclination; but the general result is, subject to small variations, that the street receives three times as much light as would fall upon it through lanterns of the ordinary kind. The frames holding the strips are glazed on both sides, and made dust-proof, so that the mirrors will not themselves become soiled or tarnished, and the reflector as a whole can be cleaned in the ordinary way, by simply wiping the glass. The plan is equally applicable to every form of lamp, and the patent includes the application of prismatic reflectors, which would present advantages in certain cases

WE learn from the *Photographic News* that a correspondent, writing from Florence, says:—"The Ruballino Society have lent their steamer *Sardinia* to Mr. Josellis for his marine explorations. Mr. Josellis has invented a marine photographic apparatus connected with a diving bell, by which photographs of the 'world below the sea' can be taken. This diving-bell can be made use of in many ways, but one can understand how useful to natural science a series of negatives (to be afterwards enlarged) of the myriads of zoophytes found in the subaqueous world would be." Good news this for the managers of the approaching four years' dredging expedition. We should like, however, to hear something more of the principles of the apparatus.

THE editor of the *Scottish Naturalist* proposes to do for the Lepidoptera of Scotland what has been so well done for British plants in the *Cybele Britannica*. He solicits the assistance of all persons acquainted with the subject in ascertaining the distribution of the species throughout the country, which for that purpose is divided into thirteen natural districts. In addition to the district distribution, information is solicited on the following points:—The vertical range of each species; the relation between the range of a species and that of its food-plant; the relation between the range of a species and the geological formation of the district; the influence of the proximity of the sea, for some insects (as is the case with certain plants) appear to occur at a higher north latitude on the sea coast than inland; and local races or varieties. The list will be illustrated by a map of Scotland. It will appear primarily in the *Naturalist*, but a limited number of copies will be printed in a separate form.

THE ratio of suicides has been established by M. Decaisne recently before the French Academy of Sciences. It is in London only one in 175 deaths; in New York, one in 172; in Vienna, one in 160; but in Paris, it has reached one in 72. The number of suicides from drunkenness, which in 1848 was 141 for all France, reached 401 in 1866. We doubt the accuracy of all these figures.

## η ARGUS AND ITS SURROUNDING NEBULA, &c.*

IN the last paper I had the honour of bringing before the Society, I referred to a correspondence which was then pending on the star η, and the attached nebula, in the constellation Argo-Navis. It will be fresh in the minds of many of the members of this Society that authorities, previously quoted, have confirmed the alterations that have been recorded in this object. Mr. E. B. Powell, of Madras, writing to the Royal Astronomical Society some observations on the binary star α Centauri, has a concluding note thus:—"I have to observe that to Mr. Abbott must be ascribed the first publication of the fact that η is no longer in the dense portion of the nebula, where it was seen by Sir John Herschel."—(*Vide* Monthly Notices R.A.S. vol. 24, p. 172.)

It was in March 1865, that I first pointed out the fluctuations in this object, through the Melbourne equatorial, to Mr. Ellery at the Observatory, when the star η was out of the nebula, and the altered figure of the dark space was filled with 12th magnitude stars, richly coloured as described in Monthly Notices R.A.S., vol. 25, p. 192.

Notwithstanding this in connection with all other evidence, strong opposing influences have been brought to bear against the movements which have been observed, although it is well known to every astronomer that there is nothing stationary in the universe. The distance of such objects as the nebula about η Argus is in all cases so immensely great, their position in the sky often unfavourable, and convenient times for observing so far apart, that any alteration or physical changes may for centuries remain unknown.

The late Sir William Herschel writes, and is followed by Sir John, thus:—"Gravitation still further condensing and so absorbing the nebulous matter, each in its immediate neighbourhood might ultimately become stars, and the whole nebula finally take on the state of a cluster of stars," &c.—(*Vide* "Outlines of Astronomy," 5th edition, p. 640.) Mr. Proctor considers that an increased or decreased distance in space may account for the fluctuations.

The present object was observed and faithfully recorded by Sir John Herschel, when stationed at the Cape of Good Hope in the year 1837. It is quite impossible to say what, if any, alterations may have taken place in the nebula before that time; but it is certain that changes have taken place both in the star and in the nebula since 1854, and these fluctuations have been so great and unusual as to raise a doubt in the mind of Sir John Herschel as to their reality. This opinion, coming from such an authority, has influenced many others, who, notwithstanding all evidence, and without a single observation of their own, have refused to credit these recorded facts. Some also, who have but lately commenced observing, contrary to all scientific rule, ignore all previous observations made by others, in order to make an opening for their own.

To decide certain points of difference which are said to exist between the drawings made by Sir John Herschel, Lieut. Herschel, and myself respectively, referees have been appointed by the Council of the R.A.S. The present paper has relation to the observations made for, and the reply sent to, the referees, in answer to their queries on the points alluded to

In carefully looking over the drawings taken at Bangalore by Lieut. Herschel, with the object η Argus, 15° above the horizon, and also the reversed copy of Sir J. Herschel's, and on consideration of the discussion given with the drawings, I do not think that Lieut. Herschel's observations tend to disprove any one of the alterations which I have previously communicated to the Society. The present drawing, and the answers given to the referees, will, I think, render this clear.

The present observations have been made with the same instrument as the former ones, the object in the same position—approximately .o° above the horizon. The measures were taken with a bar micrometer by Cook and Sons, the bars being carefully traced in pencil on the drawing paper, in such a manner as exactly to fill the field of the telescope. All the stars visible were dotted down, the distances from η of the 6th, 7th, and 8th magnitude stars were lettered, measured, and catalogued from a scale of equal parts, after which the micrometer pencil lines were rubbed out, and the nebula inserted.

The first question put by the referees relates to a comparison of the positions of the principal stars and smaller groups as shown in my two drawings, which are said to have a sufficient general agreement with each other, considered as eye drafts, while they are irreconcilable with both Sir John's and Lieut. Herschel's configurations. A simple inspection of my drawing of 1870 with the reversed drawing of Sir John Herschel (A.A., plate 4 in the Monthly Notices R.A.S.) will show that the following principal stars hold a relative position considered as eye drafts, but not with the Cape Monograph as expressed in the letter D.D., C.C., (β), (κ), B.C., (E.), 522, 558, 640, 337, 383, 415, (γ), (λ), &c., &c. There are many other stars in my copy of 1870 that are not laid down in plate 4, pricked off from Lieutenant Herschel's drawing.

The other question of note refers to my "having placed within 11½′ (on the scale of my drawing of η) five stars of magnitude at least equal to η, that is, the 7th magnitude, while in Sir J. Herschel's monograph only one star of that magnitude (marked C.) occurs within that distance;" and continues, "can you give any elucidation of the cause of the discrepancy? also if you would furnish some instrumental determination of the difference of R.A., and P.D., between η and other stars of equal magnitudes."

In my acknowledgment of this letter to Mr. William Huggins, F.R.S., &c., I mentioned that it was not my intention or desire to dispute either Sir John's or Lieutenant Herschel's configurations, but to call the attention of the astronomical world to the altered features of both the star and the nebula, with a view of obtaining a solution of the changes seen in this most remarkable object. I further stated that the above question was of a physical nature, and could only be answered as such.

On reference to my former papers, it will be seen that mention is made, more than once, of the fact that the increase of stars of the same magnitude as η renders it difficult to know that star from others, but by its position, and a marked difference in the *light*.

It is to this cause I have so frequently referred the increase of light, which I think is now clearly confirmed by a comparison of Lieutenant Herschel's description with that of Sir John's. At one of the monthly meetings of the Society, Sir John Herschel considered the increase of light in the object, as recorded, very strange, and remarked, "when I was at the Cape the nebula could not be seen at all with the naked eye." Lieutenant Herschel, when at Bangalore, compared the increased light, when the object was only 15° above the horizon, to that of Pleiades in Taurus.

Mr. Le Sueur, in his report on the Melbourne reflector, says "the nebula around η Argus has changed largely in shape since Sir J. Herschel was at the Cape. The star shines with the light of burning hydrogen," and in his opinion "has consumed the nebula."

At the monthly meeting of the Royal Society of Victoria, held on the 13th of March, 1871, Mr. Fairlie McGeorge, who has now charge of the reflecting telescope at the Melbourne Observatory, read a paper in which he referred to some observations made with that instrument on the star η Argus, and the nebula; and stated "that the object had evidently undergone great changes since Mr. Le Sueur made his sketches of it. It was now beyond a doubt that enormous physical changes were still taking place."

The catalogue accompanying my present drawing, made for the referees, and laid on the table, will show that there are now in the same field two stars of the 6th, two 6½, three 7th, four 7½, four 8th, and nine of the 8½ magnitude, and it is literally crowded with others of from the 8½ to the 12th magnitude. Those lying outside the field and occupying an area of about 1½°, have their magnitudes attached. The small cluster I take to be Sir J. Herschel's 3276, described as "a fine, bright, rich, not very large cluster," if so it is now a beautiful cluster of richly-coloured stars, quite equal to κ Crucis.

It is almost impossible to define the boundary of the nebula, as it appears to be gradually fading away, and is not so distinct in outline as formerly.

The finest nights have always been selected for observing, and no delineation of the object has ever been given, but what was an accurate representation of its appearance through the telescope.

The following is an extract from a letter addressed by Mr. Severn, of Melbourne, to the Astronomer Royal, and printed in the Monthly Notices, Royal Astronomical Society, for April, 1870:—"I may say that I cannot confirm the new position given to η Argus in respect to the nebula. I have watched it for fourteen years, and it is just where it was; of course much less brilliant."

A letter dated 21st June in the same year which I received

* Read at a meeting of the Royal Society of Tasmania, 9th May, 1871.

from Mr. Severn contains the following passage:—"My present motive is to draw your attention to the injustice done you in the η Argus business; I have of course read all your letters in the Monthly Notices of the R.A.S. on the subject. You must not allow the *Spectator*, or Mr. Le Sueur, or any other man to deprive you of your discovery; you have at least done, and that years ago, what the 4ft. Cassegranians and Mr. Le Sueur are claiming as their discovery. I can't stand this, and therefore if you don't defend yourself, by writing to our papers, I must. I send you a *Leader* with my paper in it, also another *re* η."

On reading these two extracts, which are dated about the same time, it will appear that the writer must have very suddenly changed his mind.

In June 1869 I visited Melbourne for the purpose of seeing the new large reflecting telescope, and must confess to being much surprised on seeing the object η Argus in such a small field with so large an instrument. Mr. Le Sueur thought at the time that he saw a faint shadow of a lemniscate; and what I saw was a dark path across the nebula, not unlike that portion of Eridanus, occupied by 188 and 198 l. C. and not far from the star Achernar. The object was only seen between passing clouds, and although the best speculum was in the instrument at the time, the definition was not good.

In June 1862 I brought before this Society a copy of the drawing made from observations on that beautiful cluster of coloured stars known as κ Crucis, the original drawing, &c., of which was at the time remitted to the Royal Astronomical Society, with notes on the variation of both colour and position when compared as eye drafts, with Sir John Herschel's observations made at the Cape of Good Hope. (Vide Monthly Notices, R.A.S., Vol. 23, p. 32.)

As the instrument used at the Cape was in every respect different from the one used in Hobart Town, and the effect of colour varying, as it does, so much in different persons, I discontinued observing to allow time for other changes to become known, and have now waited nearly nine years, in order to compare the object with the previous drawing by the same optical means. Sir John Herschel estimated this cluster to be formed of from 50 to 100 stars; in the drawing of 1862, a copy of which now lies on the table, there were laid down 75 stars to which the colour of each was given. It is now known that certain alterations have taken place since 1862, but a series of cloudy nights has prevented the possibility of preparing a sequent to the former drawing in time for the present meeting.

F. Abbott

## *SCIENTIFIC SERIALS*

*Transactions of the Manchester Geological Society.* Vol. ix., Parts 1, 2, and 3; Vol. x., Part 1. We have in these first three parts the President's Address and the papers read before the society during the session 1869-70. The papers are twelve in number, and embrace a variety of topics. Mr. Boyd Dawkins gives an account of some explorations in the Denbighshire caves. In one of these a large quantity of human bones was found intermingled with remains of horse, goat, hare, rabbit, badger, large birds, wolves, wild cats, foxes, and Celtic shorthorns, roe and red deer. He is of opinion that this cave has been used as a burial place at different times in the pre-Roman era. The skulls found belong to that type which Professor Huxley terms the "river bed skull," and the tibiæ indicated the platycnemic character or the bandy-leggedness of the people to whom they belonged. There are other three papers on palæontological subjects—"On a Specimen of Homalonotus Delphino-cephalus," by Mr. Edward Holber; "On some Starfishes from the Rhenish Devonian Strata," by Mr. J. Eccles, and "On two Species of Productus," by the same author. To these may be added another by the president, Mr. J. Aitken, "On the Pholas-boring Controversy," in which the author concludes, against the notion upheld by Mr. Macintosh, that the holes found in the faces of certain limestone rocks at many different levels, even as high as 1,435 feet above the sea, have been bored by pholades during a period of submergence. He inclines to the belief that the holes have been formed by land molluscs, as originally suggested by Dr. Buckland. There are several papers on physical geology, which will repay perusal. The longest of these is one by Mr. Spencer, "On the Millstone-Grit Rocks" of Halifax, which will be of use as a guide to that locality. The author distinguishes four beds of grit separated by intervening thick shales. Lists of fossils are given, and these are not so meagre as one might have expected. Mr. J. Curry has a paper "On the Throw of the Pennine Fault," which he thinks is not so great as is commonly believed. Some interesting "Observations on the Temperatures at the Pendleton Colliery," by Mr. J. Knowles, are sure to be frequently referred to. "On some of the Causes of the Different Modes of Working and Ventilating Coal Mines," by Mr. Warburton, contain some wholesome criticism. He maintains "that the systems of working coal, as at present practised, do not depend upon the nature or condition of either the coal or the roof, but upon the mining education of those who have the management." Difficulties in the way of ventilation arise from ignorance and from the modes of working often interfering with well-known natural laws. Other papers in Vol. ix. are "On the Use of Gunpowder in Mines," by Mr. Greenwell; "On two Dykes in North Lancashire," by Mr. Eccles; and "Observations on some Specimens of Silver Ore from United States," by Mr. Fletcher. Part 1. of Vol. x. is occupied for the most part with the President's address, inaugurating the session 1870-71. Mr. Aitken treats of our coal supply in its various aspects, and a number of other, chiefly palæontological, topics. The other communications in this part do not call for any special remark. They are three in number, viz., "The Spirorbis Limestone in the Forest of Wyre Coal Field," by D. Jones; "On Faults in Drift," by J. Aitken; and "On the Underground Conveyance of Coals," by G. C. Greenwell. We are glad to see from the report of the Council that the Society is flourishing, and that the number of contributors to the Transactions is increasing.

*Verhandlungen der k. k. geologischen Reichsanstalt zu Wien.* Nos. 8 and 9 (1871). No. 8 contains the usual short summaries of papers and reports, among which may be mentioned one on the last earthquake and the hotsprings and solfataras at Milo; and another on the Tertiary Land-fauna of Central Italy, by E. Suess. The other papers are more of local interest, but a number of useful analyses of minerals is given. Among the notices of contemporary publications is one of a work by Dr. Prestel, on the Climatal and other Changes which the Coasts of the North Sea have undergone since Glacial Times. In No. 9 will be found a short account of a Coast Survey of the Adriatic Sea. The survey when completed will, it is expected, make the bed of this sea as well known as that of any other which has been explored. The bottom of the south basin of the Adriatic is covered throughout, it would seem, with a yellow sludge or slime, which is brought down by the large rivers of Albania. In this same area a remarkable rocky plateau rises up from the slimy sea-bed, at a depth of from 325 to 370 fathoms to within 100 fathoms of the surface. Some details of other parts of the sea bottom are given. The number contains several other reports, among which we find some account of the Library of the Institute, which would appear to be in a flourishing condition. The usual literary notices and lists of books received conclude the number.

## *SOCIETIES AND ACADEMIES*

### London

**Royal Microscopical Society**, October 4.—Mr. W. Kitchen Parker, F.R.S., President in the chair. The first meeting of the session was held on Wednesday evening. Mr. Parker contributed a valuable paper "On the Development of the Facial Arches of the Embryo Salmon," at the conclusion of which he expressed his opinion that the development of the brain case of the osseous fishes demonstrates that group to be much closer allied to the Sauropsida, or Birds and Reptiles, than it is to that of the Batrachia, or Frog tribe. Mr. Parker highly eulogised the use of chromic acid as a medium for hardening without distorting the substance of the brain when required for sections.—Dr. Spencer Cobbold handed in a report on some preparations of Entozoa with accompanying notes, forwarded to the Society by Mr. Morris, of Sydney, and made observations on some of the most interesting forms. Of the five species collected by Mr Morris, Dr. Cobbold stated that by far the greatest amount of importance was to be attached to the discovery in Australia of *Stephanurus dentatus*. This Entozoan was introduced to the scientific world as early as the year 1834 by Natterer, who found it in large quantities infesting the adipose tissues of a breed of Chinese pigs, on the Rio Negro in Brazil. Up to the year 1870 nothing further was heard of this parasite, when Dr. Cobbold received a communication from Prof. Fletcher, of New

York, stating that it was committing great destruction among the pork-raising districts of the United States, thousands of pigs in some localities falling victims to its ravages. In aspect and structure *Stephanurus* bears a close resemblance to *Trichina*, but is of much larger size, the cysts of the former frequently measuring an inch or an inch and a half in length; its greater magnitude is the principal safeguard against its introduction into the human subject. Dr. Cobbold supplemented his remarks with some observations on the question of sewage irrigation connected with the propagation of entozoic diseases. In his opinion it played a very important part, and he did not feel his position in the slightest degree destroyed from the fact of Mr. Hope's ox brought up for nearly two years on the produce of the "Breton" irrigated farm being entirely free from internal parasites of any kind. This animal had never been allowed to graze, but had had all its food cut and carried to it; its water was all brought to it, and altogether the animal had been so carefully guarded and nurtured that the Entozoa were shut out from any chance of obtaining a foothold. The soil, again, on Mr. Hope's estate was of such a porous nature that the matter containing the undeveloped germs was at once absorbed, while on swampy ground, as instanced about Croydon and other low-lying districts, where this mode of irrigation was practised, the roots of the grasses were constantly immersed in it. The prevalence of tape-worm and other entozoic diseases in those parts of India where sewage irrigation is carried out, is enormous, and thousands of cattle are destroyed as being unfit for human food. This wanton destruction of all carcases containing traces of *Cysticercus*, or other Entozoa, Dr. Cobbold severely censured, as the meat, on being thoroughly cooked, even though infested with parasites, is wholesome, free from any abnormal taste, and its consumption is unattended by deleterious results.

PARIS

Academie des Sciences, September 25.—M. Faye in the chair. M. Dumas, the perpetual secretary, gave many interesting details of a report written by a committee of which he is a member, describing the *Phylloxera vastatrix*, the pest of the vine. A prize of 400*l*. was offered for its destruction by the French Government, and will be awarded in 1873. But two candidates have invented means which appear to be good. M. Faucon has suggested putting the whole vine garden under water for two days, which is sufficient to suffocate the insects with injuring the plant itself. When it is impossible to inundate, M. Blanthou suggested to water with a liquid composed of 1,000 parts water and one of impure phenic acid.—M. Fonssagrives has discovered that the mouldiness of Roquefort cheese, which is eaten by French *gourmets* only in a state of putrefaction, when placed on a piece of bread, developes the *Oidium aurantiacum*, which may account for the abundant appearance of this pest last summer.—M. Dumas reported upon the results obtained by microscopical selection, as suggested by M. Pasteur and practised by many French silkworm breeders for curing the silkworm plague known as *pébrine*. The results are magnificent, and the plague may now be considered as almost entirely suppressed. Last year one-tenth of the French silkworm breeders used the method invented by M. Pasteur, and the use of it will be almost universal in the course of a few years.—M. Grimaud of Caux, one of the veteran members of the Parisian scientific press, read a memoir "On the Smoke of Locomotives in the Mont Cenis Tunnel." M. Grimaud finds it to be a great objection, and to require much caution. But such is not the advice of people who are fresh from the tunnel.—M. Philips read a long paper, by a gentleman who does not belong to the Institute, "On the Integration of some Special Differential Linear Equations." He commented largely upon the paper, which he finds worthy of much consideration. New communications "On the Spectrum Analogies of Simple Bodies" were also read.

PHILADELPHIA

Academy of Natural Sciences, January 3.—Mr. W. M. O. Vaux, vice-president, in the chair. Professor O. C. Marsh, of Yale College, exhibited a tooth of a new species of *Lophiodon*, from the Miocene of New Jersey, which was the first indication yet discovered of remains of the Tapiridæ on the Atlantic coast, or of the genus *Lophiodon* in this country, east of the Rocky Mountain region. The tooth, which was in a perfect state of preservation, was the first true molar of the left upper jaw. It measured across the crown seven lines in antero-posterior diameter, and eight and one-quarter lines in transverse diameter. This would indicate an animal intermediate in size between *L. occidentalis* and *L. modestus* of Dr. Leidy. From the latter species it may readily be distinguished by the enamel of the crown, which is smooth and not wrinkled. As this species is evidently distinct from any described, Prof. Marsh proposed for it the name *Lophiodon validus*. The specimen was found in the miocene marl of Cumberland County, New Jersey, and apparently at about the same horizon as the *Elotherium Leidyanum*, and *Rhinoceros matutinus* Marsh, from Monmouth County.

January 24.—Mr. Vaux, Vice-President, in the chair.—Mr. Thomas Meehan presented a fruit of a pear, which presented the external appearance of an apple, gathered from a Tyson pear tree growing in the garden of Dr. Lawrence, of Paris, Canada. Dr. Lawrence had a Rhode Island greening apple near the pear tree, and some of the latter interlaced with it. The pear tree was full of blossoms last spring, but only those interlacing bore fruit. They had all the appearance of apples, so much so, that many who had seen them supposed there must have been some mistake as to Dr. Lawrence gathering them. Dr. L., had, however, when he first saw them, obtained Mrs. Lawrence's aid in separating the branches, so that there should be no mistake. The specimens had been sent to Mr. Meehan, who regarded them as apples; but on cutting them open, found the seed to be of the pear. The granular matter characteristic of the pulp of the pear also existed in the carpels, but none in the pulp, which was wholly fibrous, as in the apple; the insertion of the stalk, also, was that of the pear. Instead of the cavity being funnel-shaped, as in the apple, it was campanulate, as if the stem had been pushed in, carrying the epidermis and pulp with it. He had no doubt that the fruit had the pedicel, carpellary walls, and seeds of the pear, with the granular pear-pulp wanting; but with the fibrous pulp and epiderm of the apple. As to the law of its production, he disliked speculation, but it would seem that there were two ways in which it might be produced—either by a natural evolution of form, independent of sexual influence, which plants at times exhibited, or by cross-fertilisation with the apple. In the latter case, if found true, it would have an important bearing on the question often mooted, whether cross-fertilisation effected change immediately in the fruit impregnated, or whether change only appeared after the germination of the impregnated seeds. In the case of varieties of Indian corn, we know the change is immediate; and it was generally believed some cucurbitaceous plants furnished similar facts; but he thought it had not been known in other plants, especially in the case of species as distinct as the apple and pear.

## BOOKS RECEIVED

ENGLISH.—The Micrographic Dictionary: Griffith and Henfrey; New Edition, Parts 1 and 2 (London: Van Voorst).—Homo *versus* Darwin (London: Hamilton and Co.).—Notes on Comparative Anatomy: W. M. Ord (London: Churchill).

FOREIGN.—(Through Williams and Norgate).—Neues Handwörterbuch der Chemie: Dr. Hermann v. Fehling: Erster Band, 1te u. 2te Lieferung.

## CONTENTS

THURSDAY, OCTOBER 19, 1871

## *HELMHOLTZ ON THE AXIOMS OF GEOMETRY*

THE *Academy* journal of the 12th of February, 1870 (vol. i. p. 128), contained a paper by Prof. Helmholtz upon the Axioms of Geometry in a philosophical point of view. The opinions set forth by him were based upon the latest speculations of German geometers, so that a new light seemed to be thrown upon a subject which has long been a cause of ceaseless controversy. While one party of philosophers, especially Kant and the great German school, have pointed to the certainty of geometrical axioms as a proof that these truths must be derived from the conditions of the thinking mind, another party hold that they are empirical, and derived, like other laws of nature, from observation and induction. Helmholtz comes to the aid of the latter party by showing that our Axioms of Geometry will not always be necessarily true; that perhaps they are not exactly true even in this world, and that in other conceivable worlds they would be entirely superseded by a new set of geometrical conditions.

There is no truth, for instance, more characteristic of our geometry than that between two points there can be only one shortest line. But we may imagine the existence of creatures whose bodies should have no thickness, and who should live in the mere superficies of an empty globe. Their geometry would apparently differ from ours; the axiom in question would be found in some cases to fail, because between two points of a sphere diametrically opposite, an infinite number of shortest lines can be drawn. With us, again, the three angles of a rectilineal triangle are exactly equal to two right angles. With them the angles of a triangle would always, more or less, exceed two right angles. In other imaginary worlds the geometrical conditions of existence might be still more strange. We can carry an object from place to place without necessarily observing any change in its shape, but in a spheroidal universe nothing could be carried about without undergoing a gradual distortion, one result of which would be that no two adjoining objects could have a similar form. Creatures living in a pseudo-spherical world would find all our notions about parallel lines incorrect, if indeed they could form a notion of what parallelism means. Nor is Helmholtz contented with sketching what might happen in purely imaginary circumstances. He seems to accept Reimann's startling speculation that perhaps things are not as square and right in this world as we suppose. What should we say if in drawing straight lines to the most distant fixed stars (by means not easy to describe), we found that they would not go exactly straight, so that two lines, when fitted together like rulers, would never coincide, and lines apparently parallel would ultimately intersect? Should we not say that Euclid's axioms cannot hold true? It may be that our space has a certain twist in a fourth dimension unknown to us, which is inappreciable within the bounds of the planetary system, but becomes apparent in stellar distances.

Though Helmholtz gives most of these speculations as due to other writers, he seems, so far as I can gather from his words, to stamp them with the authority of his own high name. It requires a little courage, therefore, to maintain that all these geometrical exercises have no bearing whatever upon the philosophical questions in dispute. Euclid's elements would be neither more nor less true in one such world than another; they would be only more or less applicable. Even in a world where the figures of plane geometry could not exist, the principles of plane geometry might have been developed by intellects such as some men have possessed. And if, in the course of time, the curvature of our space should be detected, it will not falsify our geometry, but merely necessitate the extension of our books upon the subject.

Helmholtz himself gives the clue to the failure in his reasoning. He says: "It is evident that the beings on the spherical surface would not be able to form the notion of geometrical similarity, because they would not know geometrical figures of the same form but different magnitude, except such as were of infinitely small dimensions.' But the exception here suggested is a fatal one. Let us put this question: "Could the dwellers on a spherical world appreciate the truth of the 32nd proposition of Euclid's first book?" I feel sure that, if in possession of human powers of intellect, they could. In large triangles that proposition would altogether fail to be verified, but they could hardly help perceiving that, as smaller and smaller triangles were examined, the spherical excess of the angles decreased, so that the nature of a rectilineal triangle would present itself to them under the form of a limit. The whole of plane geometry would be as true to them as to us, except that it would only be exactly true of infinitely small figures. The principles of the subject would certainly be no more difficult than those of the Differential Calculus, so that if a Euclid could not, at least an Archimedes, a Newton, or a Leibnitz of the spherical world would certainly have composed the books of Euclid, much as we have them. Nay, provided that their figures were drawn sufficiently small, they could verify all truths concerning straight lines just as closely as we can.

I will go a step further, and assert that we are in exactly the same difficulty as the inhabitants of a spherical world. There is not one of the propositions of Euclid which we can verify empirically in this universe. The most perfect mathematical instruments are not two moments of the same form. We are practically unacquainted with straight lines or rectilineal motions or uniform forces The whole science of mechanics rests upon the notion of a uniform force, but where can we find such a force in operation? Gravity, doubtless, presents the nearest approximation to it; but if we let a body fall through a single foot, we know that the force varies even in that small space, and a strictly correct notion of a uniform force is only got by receding to infinitesimals. I do not think that the geometers of the spherical world would be under any greater difficulties than our mathematicians are in developing a science of mechanics, which is generally true only of infinitesimals. Similarly in all the other supposed universes plane geometry would be approximately true in fact, and exactly true in theory, which is all we can say of this universe. Where parallel lines could not exist of finite magnitude, they would be conceived as of infinitesimal magnitude, and the conception is no more abstruse than that of the direction of a continuous curve, which is never the same for any finite distance. The spheroidal creatures would find the distortion of their own bodies rapidly vanishing as the distance of the motion is less, which only amounts to the truth, that a small portion of an ellipse is ultimately undistinguishable from a circle. The truth of the Axioms of Geometry never really comes into question at all, and Helmholtz has merely pointed out circumstances in which the figures treated in plain geometry could not always be practically drawn.

It is a second question whether the dwellers in a spherical world could acquire a notion of three dimensions of space. We must remember that such beings could bear no analogy to us, who have solid bones and flesh, and live upon a solid globe, into which we can penetrate a considerable distance. These beings have no thickness at all, and live in a surface infinitely thinner than the film of a soap bubble, in fact, not thin or thick at all, but devoid of all pretensions to thickness.

There would be nothing at first sight to suggest the threefold dimensions of space, and yet I believe that they could ultimately develop all the truths of solid geometry. They could not fail to be struck with the fact that their geometry of finite figures differed from that of infinitesimals, and an analysis of this mysterious difference would certainly lead them to all the properties of tridimensional space. Indeed, if Riemann, prior to all experience, is able to point out the exact mode in which a curvature of our space would present itself to us, and can furnish us with analytical formulæ upon the subject, why might not the Riemann of the spherical world perform a similar service, and show how the existence of a third dimension was to be detected? It might well be that the inhabitants of the sphere had in the infancy of science never suspected the curvature of the world, and, like our ancestors, had considered the world to be a great plain. In the absence of any experience to that effect, it is certain that the notion of thickness could not be framed any more than we can imagine what a fourth dimension of our space would be like. We have some idea what a world of one dimension would be, because as regards *time* we are in a world of that kind. The characteristic of time is that all intervals beginning and ending at the same moments are equal. But suppose that some people discovered a mysterious way of living which enabled them to live a longer time between the same moments than other people; this could only be accounted for by supposing that they had diverged from the ordinary course of time, like travellers taking a roundabout road. Though in one sense such an occurrence is utterly inconceivable, yet in another sense we can probably anticipate the character of the phenomenon, and the 47th proposition of Euclid's first book would doubtless give the most important truth concerning times thus differing in direction.

With all due deference to so eminent a man as Helmholtz, I must hold that his article includes an *ignoratio elenchi*. He has pointed out the very interesting fact that we can conceive worlds where the Axioms of our Geometry would not apply, and he appears to confuse this conclusion with the falsity of the axioms. Wherever lines are parallel the axiom concerning parallel lines will be true, but if there be no parallel lines in existence, there is nothing of which the truth or falsity of the axiom can come in question. I will not attempt to say by what process of mind we reach the certain truths of geometry, but I am convinced that all attempts to attribute geometrical truth to experience and induction, in the ordinary sense of those words, are transparent failures. Mr. Mill is another philosopher whose views led him to make a bold attempt of the kind. But for real experience and induction he soon substituted an extraordinary process of *mental experimentation*, a handling of ideas instead of things, against which he had inveighed in other parts of his "System of Logic." And the careful reader of Mr. Mill's chapter on the subject (Book II. chapter 5) will find that it involves at the same time the assertion and the denial of the existence of perfectly straight lines. Whatever other doctrines may be true, this doctrine of the purely empirical origin of geometrical truth is certainly false.

W. STANLEY JEVONS

## LEIGHTON'S LICHEN-FLORA OF GREAT BRITAIN

*The Lichen-Flora of Great Britain, Ireland, and the Channel Islands.* By the Rev. W. A. Leighton, F.L.S. (Published for the Author. Shrewsbury, 1871.)

IT falls so rarely to the botanical reviewer in this country to notice works on Lichenology, that we gladly avail ourselves of the present opportunity of introducing to our readers a little unpretentious volume which has the excellent object primarily—"of elevating the knowledge of our insular lichens to a level with that of other branches of our country's flora," and which, moreover, completely vindicates the title of Britain's lichens to at least equal study with the other families of her cryptogamia. Since the publication of Mudd's excellent "Manual" in 1861, the additions made to the lichen-flora of Great Britain and Ireland have been both so numerous and important, that lichenological students have felt the want of some systematic work containing a complete list of the British lichens up to the present date, along with specific diagnoses and other aids to their identification. It was generally felt, moreover, that no fitter authority could undertake so intricate a labour than Mr. Leighton, whose name is identified with lichenological progress in this country by the publication of many important papers of a monographic character, and who is justly regarded, both by home and foreign botanists, as the representative and father of lichenology and lichenologists in Britain. The present work, which we are glad to find is to be followed, in due time, by another which is even more urgently required —a Conspectus of all known lichens throughout the world—is a convenient 12mo volume of about 470 pages, which confines itself mainly to a systematic enumeration, with specific diagnoses, of all the lichens at present known to occur in "Great Britain, Ireland, and the Channel Islands." The nomenclature and classification followed are those of Dr. Nylander, of Paris, who is described as "the *facile princeps* of modern microscopic lichenologists." Succeeding the specific diagnoses, the author cites the leading synonyms; gives references to published plates and fasciculi of dried specimens; narrates the general geographical distribution of species throughout the world, on the one hand, and throughout the three kingdoms on the other; specifies the particular localities of growth in each of these latter kingdoms; and gives, so far as possible, the date of original discovery in Britain, with the name of the discoverer.

Besides the fruits of laborious compilation, the work obviously contains a large amount of original research. There are no less than seventy-five species, varieties, or orms, described for the first time (though not necessarily in this volume) by Mr. Leighton himself; many of these referring, however (as in the case of the *Graphideæ*), to varieties or forms that do not apparently require separate description and nomenclature. He has also given great attention to the action of certain chemical substances on the thallus and apothecia, and has to a considerable extent employed the said reaction in his minor classification. Only those who have attempted similar works can understand the immense labour involved in their preparation; and British botanists ought to feel, and doubtless do feel, themselves under great obligations to Mr. Leighton for undertaking and successfully executing so difficult a task. The present work has been published at Shrewsbury for and by the author himself—a procedure which enables a writer to escape the irksome and mischievous fetters sometimes imposed by publishers. But this circumstance—of local publication—is apt to be attended with certain countervailing disadvantages; so that in the present instance it does not surprise us that the typography, paper, and binding—the general up-get of the volume—do scant justice to all the author's labours in its compilation.

It is always an ungracious task to expose faults in a work that is, on the whole, excellent; that has been a labour of love; that embodies the fruit of much research; and that could have been fitly undertaken by very few individuals. But Mr. Leighton himself apparently invites co-operation, if not criticism, in order to the preparation of a fuller and more accurate second edition; and his present work contains defects of a character that seriously mar its usefulness to the student, and that no honest reviewer, if he is to be critical at all, would be warranted in passing without notice. It is then a very serious defect of the book that it contains no Index of Species and Varieties, alphabetically arranged after the manner of that in Mr. Crombie's Enumeration. For small genera, containing not more than half a dozen species, it may be comparatively easy to find *varia* or *communis*, or any other type; but in large genera such as *Lecanora*. *Verrucaria*, and *Lecidea*, each containing from 73 to 233 species, the student must carefully read that number of names, spread over 53 to 110 closely printed pages in each case, before he finds perhaps the species of which he is in search. Only the most ardent lichenologist, who has abundant leisure as well as patience at command, will care to take this amount and kind of trouble. The omission referred to is of such importance that we counsel Mr. Leighton to lose no time in issuing a full and legible Index of species and varieties as a supplement to the present work; and to avail himself of the opportunity, which we trust its rapid sale and extensive circulation will give him, of inserting such an Index in its proper place in a second edition. The form of the said Index should be that adopted by Crombie in his Catalogue of the British Lichens (1870), and not that of Mudd, in his Manual (1861), which is infinitely less easy to use.

In his present work, Mr. Leighton assumes too high a previous standard of technical knowledge on the part of the student. How many beginners in lichenology are likely to know—without being informed—what our author means by a "glypholecine" epithecium, or "bacilliform" spores? In fact, there ought to be a Glossary, to explain the meaning of the technical terms employed throughout the work; and this is the more necessary, seeing that, unlike Mudd in his "Manual," Mr. Leighton gives no Introduction explanatory of the general structure and morphology of lichens. Further, the student cannot be expected to know by intuition the meaning of the abbreviations used by the author, such as B.; Bohl.; Zw.; M. and N.; Arn.; Fellm; Th.M. Fr.; Flk. D.L.,; Nyl. Syn., Scand. or Pyr.; Hepp sporen; and so forth. There ought certainly to have been prefixed a full explanation of all these, and similar, contractions; which explanation would necessarily include a comparatively complete and most useful Lichen Bibliography. Again, there is no standard of form, size, or colour. We are told that certain spores are large, moderate, small, minute, or very minute; and certain spermatia long, shortish, or shortly cylindrical. But in no case are measurements given; and the student has to form his own opinion as to the signification of these unscientific, vague, relative terms. He is left, moreover, to conjecture as to what constitute the "positive" and "negative" reactions of hydrate of potash and hypochlorite of lime; and as to what is a "vinous" reaction of the hymenial gelatine with iodine!

The work professes to give a "*full* diagnosis" of each species. But that surely cannot be considered a *full* diagnosis, which systematically omits almost all reference to the important Secondary Reproductive Organs? In not a single species, so far as we have been able to discover, is there a full description of the *Spermogones! Pycnides* are not once mentioned in the volume! No doubt in one or two species the character of the *spermatia* is sketched by a single term, or by other inadequate means. Thus in *Opegrapha amphotera* the spermatia are said to be "different from *O. vulgata*;" but we are not told what is their character in *O. vulgata*. There are certain large and important genera in which the spermogones are not at all mentioned even in the diagnosis of the genus (*e.g.*, *Verrucaria*, *Cladonia*, *Collema*, *Leptogium*, *Opegrapha*, and *Graphis*); while in others such a description as "Spermatia various" (*e.g.* in the *Ramalinei*) conveys little or no real information! In a very few exceptional instances, among the higher Lichens, are spermogones or their contents described. Where the attempt is made, the result is singularly bald and unsatisfactory, and is obviously not the fruit of original investigation. And, further, the beginner will scarcely understand what is meant by crenated, oblong cylindrical, straight, curved, or slender spermatia, without plates, which are wholly wanting in the present volume. A student cannot be said to have acquired a "knowledge" of Lichens, who is ignorant of the characters of their *Spermogones* and *Pycnides*. To the biologist or physiologist, therefore—to him whose object is to study the whole Natural History of a given Lichen-species—such omissions in a systematic work on a national Lichen-flora is one of primary importance. The author tells us that he aims at descriptions, which will "*facilitate the student* (*sic.* the italics being ours) in the ready and accurate determination of his *specimens*;" that is to say, the naming or ticketing of them, which is something very different from imparting a knowledge of all their natural characters! The truth is that such works as the present are calculated not to create Biologists, but to perpetuate a race of mere collectors and labellers—men whose highest aim is to gather "new" or "rare" species; who spend their holidays in accumulating *specimens*, sending those that are unfamiliar across the Channel for identification or naming. One of the results of the latter procedure is that the present work contains no less than 200 British species or varieties bearing Nylander's name as the author of their first description!

While, however, meagre attention has been thus bestowed upon the secondary reproductive organs, undue prominence is given to the action of potash and lime on the thallus and apothecia, and the reaction of iodine with the hymenial gelatine; phenomena that are so uncertain and inconstant that they vary even in the same individual under different circumstances. We would not *exclude* chemical or *any* natural characters from the definition of species; but the present work seems to us to furnish ample illustration of the danger of making use of secondary, trivial, inconstant characters as a basis for classification (*e.g.* the genus *Cladonia*.)

The localities of growth are satisfactory so far as they go; but they are utterly inadequate as representing the distribution of species in either of the three kingdoms. In order to specify, with at all adequate fulness, the diversity of locality occupied in England, Scotland, and Ireland respectively by the species enumerated, Mr. Leighton must have examined for himself the contents of all the Lichen-Herbaria in these kingdoms; and, though the said herbaria are neither numerous nor large, compared with those of flowering plants, such a labour is obviously impossible for any *one* man of average leisure and opportunities.

There is no Tabular Summary showing the numerical richness of the British Lichen-Flora; an omission, it may be, of minor importance, but still of importance, inasmuch as it is always interesting to "take stock" occasionally of the rate of progress of the additions that are being made to a national Sub-Flora. Basing our calculations on the data supplied by the present work, we find a total of 73 genera and no less than 781 species; whereas only last year in his enumeration, Crombie (p. 124), gave the whole number of British Lichens then known as 658, the difference apparently representing, or consisting of, so-called *new* species. Of the host of these *new* species added of late years to our Lichen-Flora, perhaps not above one-fifth will survive in that "struggle for existence," to which they will sooner or later have to submit at the hands of the philosophic botanist. A large proportion will doubtless be found to consist of *mere forms of common, protean, widely distributed species*—forms that neither require nor deserve separate nomenclature and rank.

We have not exhausted the list of blemishes in the book before us. But to notice *all* the errors in matters of detail; all those points on which other lichenologists are likely to take grave exception to his views; all the faults in typography or otherwise, would extend and expand this review into a Treatise on the Classification of Lichens; for it would necessarily deal with certain features of that Nylanderian system, which Mr. Leighton follows in his present work.

With all the aids the author gives the student, it will, we fear, be impossible for the latter to identify the majority of the less common and familiar species without reference to authentic specimens named by Mr. Leighton himself. The work is so elaborate and complex, the principles and practice of classification adopted in it are so puzzling, that we candidly confess our own general impression to be one of increasing bewilderment, and of growing indisposition to attempt the identification or nomenclature of Lichens at all! We hesitate not to avow our own preference for studies on the Biology of the common economical species, such as those which *at present* are called *Cladonia rangiferina*, *Usnea barbata*, *Ramalina calicaris*, *Parmelia saxatilis*, *Roccella tinctoria*, or *Lecanora tartarea*.

On the whole, however, the "Lichen-Flora of Great Britain" is a work that should find a place in every public botanical library in the three kingdoms, as well as in the private libraries of all students of the extremely puzzling cryptogamic family of which it treats.

W. LAUDER LINDSAY

## OUR BOOK SHELF

*A Complete Course of Problems in Practical and Plane Geometry, adapted for the Use of Students preparing for the Examinations, &c.* By John William Palliser, Second Master and Lecturer of the Leeds School of Art and Science. (London: Simpkin and Marshall.)

A NEW class-book on Practical Geometry commends itself to our attention. Mr. John Palliser, of the Leeds School of Art and Science, has produced one of those educational works which a demand created by Government examinations has recently brought to our aid. Reserving our opinion as to the final tendency of an epidemic for what are called practical results, we must, in justice, say that this class-book of Mr. Palliser's is the very thing for cheapness, conciseness, comprehensiveness, to rapidly possess the student with a ready-handed ability to answer all demands of the examiner. The work is not encumbered with demonstration, for this, in view of the proposed end, would be out of place; it is a laboratory of experimental formulæ. We have a recipe for constructing all conceivable polygons within the compass of a single circle, for drawing lines to invisible points, and for trisecting the most obdurate angles by the magic of a slip of paper. Faith is all that is demanded of the student, faith in the formulæ before him, and industry to get them by heart. Not troubled with the Why, he has only to remember the How; but he must be careful, exact, and neat-handed; and this, if not mental training, is next of kin to it. The arrangement of the book is generally good, the style concise in the extreme, the letter-press wonderful at the price, and the diagrams, with their faint, dark, or dotted lines, are highly effective and intelligible, not less so from the fact of the *lettering* being (what we very seldom find it) correct.

To examine in detail the 220 problems of Mr. Palliser's book is more than we can just now undertake; but so far as we have dipt into them there is little to complain of, considering that the work is merely practical. The style, we have said, is concise; but (if we might venture a criticism on a point where most geometers are more guilty than Mr. Palliser) it would lose nothing in intelligibility if the nominative case were less frequently preceded by a multitude of perplexing conditions which really have to be neglected by the learner till the said nominative is reached, and then returned to lastly in that natural order of thought which geometers have a fancy for inverting. Whilst taking these minor exceptions, we must not omit to call the author's serious attention to Problem 13, which, whether we consult the diagram or the letter-press, is wholly fallacious. Such a construction will not effect the object of the problem, the bisection of the angle, though the line H K will converge in common with the two given lines. We must further enter protest against the *unqualified* proposal "to draw a straight line equal to the true length of the circumference of a circle" (Prob. 184) as misleading to the learner. But, any such defects notwithstanding, here is a most wonderful eighteenpenny book.

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### Geometry at Oxford

In the last number of Nature Mr. Proctor remarks that "no one who considers carefully the mathematical course at either University, can believe that it tends either to form geometricians or to foster geometrical taste."

With regard to Oxford, I think it is only fair that some qualification should be offered to this conclusion. In Cambridge, candidates for mathematical honours have to run their race in a course clearly marked out for them, and loss of place is naturally the result of individual vagaries. But in Oxford the order of merit is not carried further than distribution into classes, and I do not believe there is anything to prevent a skilful geometrician finding himself in the first class with those who put their trust most in analytical methods.

I cannot pretend to much geometrical capacity, but I know something of Oxford mathematical teaching. Speaking for myself, the fascinating lectures of the present Savilian Professor of Geometry will never cease to hold perhaps the most prominent place in my recollections of university work. It is quite true that I remember conversing with a college tutor who was rather doubtful about modern geometrical methods, and seemed disposed to look upon these lectures as "dangerous." He was a great stickler, however, for "legitimacy," thinking it wrong, for example, to import differential notation into analytical geometry; but I do not think he had a large following amongst younger Oxford men. I certainly did not find, in reading with some of them, that geometry was at all in disfavour. I have often had neat geometrical solutions pointed out to me of problems where other methods proved cumbrous or uninteresting; and conversely I have found geometrical short cuts were far from objected to. On the whole, the characteristic feature of the Oxford examination system (most marked in the Natural Science School, but making itself felt in all the others) being to encourage a student after reaching a certain point in general reading to make himself strong in some particular branch of his subject, I believe special attention to geometrical methods would pay very well.

Oct. 13 W. T. Thiselton Dyer

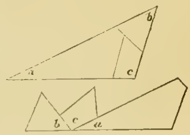
### Elementary Geometry

Your correspondent, "A Father," has in view a very desirable object—to teach a young child geometry—but I fear that he is likely to miss altogether the path by which it may be reached. His principle, that "a child must of necessity commit to memory much that he does not comprehend," appears to me to be totally erroneous, and not entitled to be called a fact. To this time-hallowed principle it is due that a large proportion of all who go to school learn nothing at all, while those more successful learn with little improvement of their faculties. It is a convenient principle which allows the title of teacher to be assumed by those who only hear lessons. Children labour under this difficulty that they learn only through language, which is to them a misty medium, particularly when the matter set before them is in any degree novel or abstruse, and no pains are taken to clear up the obscurity of new expressions. Children know nothing of abstraction, and learn to generalise from experience, not from words. Committing to memory what is not understood is a disagreeable task; begetting a hatred of learning, and causing many to believe that they want the special faculty required for the task set before them. The art of teaching the young ought to be the art of enabling them to comprehend, and memory ought to be strengthened not by drudgery but by being founded on understanding and by the rational connection of ideas.

Now geometry is the science of figure; it theorises reality, and the truth of every proposition in it may be made apparent to the senses. Double a piece of paper and cut out a triangle in duplicate. The two equal triangles thus formed, A and B, may be put together so as to form a parallelogram in three different ways. The child who makes this experiment will learn at once

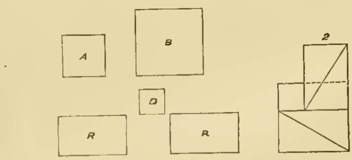

what is meant by a parallelogram, and he will perceive its properties, viz., that its opposite sides and angles are equal; that it is bisected by the diagonal, &c. But if he learns all this by rote, he acquires only a cloud of words, on which his mind never dwells. Propositions touching abstractions and generalisations can never be understood by the young without abundant illustration. When a geometrical truth is made apparent to the senses, when seen as a fact and fully understood, the language in which it is expressed having no longer a dim and flickering light, is easily learned and remembered, and the learner listens with pleasure to the discussion of the why and wherefore.

It is not enough for a child to learn by rote the definition of an angle. He ought to be shown how it is measured by a circle; and by circles of different sizes. In short, he ought to be taught what words alone will not teach him, that an angle is only the divergence of two lines. Let us now come to the important theorem that the three angles of any triangle are equal to two right angles (Euclid i. 32). Cut a paper triangle, mark the angles, then separate them by dividing the triangle and place the three angles together. They will lie together, filling one side of a right line, nd thus be equal to two right angles. Let the learner test the theorem with triangles of every possible shape to convince himself of its generality, and then, fully understanding what it means, he will also understand the language in which it is proved.




It is a mistake to decry the use of symbols. They enable us to get rid of the wilderness of words, which form a great impediment in mathematical reasoning. Ordinary language can never group complex relations for comparison so compactly as to bring them within the grasp of the understanding. When we would compare objects, we place them close together, side by side. But the features and lineaments of objects described in language are too widely scattered to be kept steadily in view. It is easier to learn the use of symbols than to commit to memory what is not understood. Those who would learn mathematics without symbols can advance but a little way.

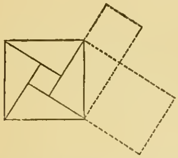
Neither is there any good reason for rejecting the second book of Euclid, though it certainly may be much abridged. The relations of whole and parts, sum and difference are easily exhibited, and an acquaintance with them is of great value to arithmeticians. Let us take for example the following propositions: "The squares (A and B) of any two lines (or numbers) are equal to double the rectangle under those lines (R and R, or the product in case of numbers) and the square of their difference D."

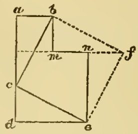



Now these figures being constructed, it will be found that when the two squares are placed together as in Fig. 2, the rectangles cover exactly the parts marked with diagonals, and the square of the difference the remainder.

| | | | |
|---|---|---|---|
| In numbers, the square of | 5 | = | 25 |
| " | 3 | = | 9 |
| | | | 34 |
| Double product of 5 and 3 | | | 30 |
| Square of diff. | | | 4 |
| | | | 34 |

Here is a perfect demonstration evident to the senses. But let us go one step further. The rectangles in the preceding theorem may be bisected by diagonals and set round the square of the difference in such a manner as to form the square of the hypothenuse of the right-angled triangle, the sides of which are also those of the assumed squares. The squares of the sides of a right-angled triangle, therefore, are together equal to the square of the hypothenuse, since the former may be changed into the latter.






The same conclusion may be arrived at by a still shorter and simpler course. Let any two squares be joined together as in the annexed figure, or, rather, let them be cut in paper in one piece. Then take *a c* equal to the side of the greater square, and join *b c* and *c e*. Cut off the two equal triangles *b a c* and *c d e*, and place them in the positions of *b m f* and *f n e*, and the two squares will be thus transformed into the square of the hypothenuse of the right-angled triangle, of which they form the sides.

Thus we have at once a demonstration of the famous Pythagorean theorem (Euclid i. 47), and have attained with three or four steps the same height climbed by Euclid with forty-seven. The words of his demonstration, committed to memory by a child, remain there mere words and nothing more. Words serve to mark and denote ideas, but cannot create them, where the material of ideas does not already exist. But the child who with paper or card amuses himself in going over the visible demonstration suggested in this letter, in various forms and repeatedly—for neither old nor young can be said to learn a truth merely by its transient recognition—will assuredly awaken to an agreeable consciousness of the reasoning faculty, and feel no difficulty in future geometrical studies.

In 1860 there was published for me, by Messrs. Williams and Norgate, a little volume entitled, "The Elements of Geometry Simplified and Explained," adapted to the system of empirical proof, and of exhibiting the truth of theorems by means of figures cut in paper. It contained in thirty-five theorems the quintessence of Euclid's first six books, together with a supplement of thirty-three not in Euclid. There was no gap in the sequence or chain of reasoning, yet the 32nd and 47th provisions of Euclid were respectively the 3rd and 17th of my series. This book proved a failure, for which several reasons might be given, but it will be sufficient here to state but one, namely, that it came forth ten years before its time. What became of it I know not. But of this I am convinced, that though I failed, success will attend those who follow in my footsteps. W. D. COOLEY

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THE discussion in your last part on methods of teaching elementary geometry reminds me that at a period when I was teaching the subject to a considerable number of pupils, I frequently overcame the difficulties of very young or inapt students by commencing with the study of a *solid*, such as a cube, encouraging the pupils to frame definitions for the parts of the object. The ideas existing in the child's mind of a solid, a plane, a line, and a point, were thus put into words in an order the reverse of that in which they would have been had Euclid been used. The chief properties of parallelograms and triangles followed, and were easily *discovered* by the use of a pair of compasses, scissors, and paper, and that at an age when Euclid was a sealed book. I believe children can be most easily taught to solve problems in plane geometry when they occur in connection with early instruction in practical solid geometry. Most children try to draw, and if they were encouraged to represent simple objects by "plans" and "elevations," the necessity of obtaining a knowledge of how to describe the forms presented to them would frequently carry the pupils through a large number of the principal problems of plane geometry with a pleasure they could not experience if the "problems" were put before them, without any reason for their solution but the teacher's command. The powers of truthful representation gained by such teaching, would be of the utmost value to thousands who would never attempt to learn "Euclid;" whilst, so far as I am able to judge, it is more likely to prepare the boy to read formal works on geometry with pleasure than to create a distaste for the study.

THOMAS JONES

Woolwich, October 9

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## The Coming Eclipse

I HAVE been very much interested in Mr. Lockyer's lecture at the Royal Institution on the late eclipse. I am especially glad that he is at length able to acknowledge the existence of comparatively cool hydrogen, because in my Eclipse Report of 1868 (vol. xxxvii. Part 1, R.A.S. Memoirs), I stated that I believed from the evidence of the photographs that hydrogen was dispersed from the prominences in visible streams in some cases, and in others invisibly.

But while Mr. Lockyer admits this, he seems to me very unnecessarily to avoid everywhere the use of our familiar term "atmosphere" to include the whole gaseous envelope of the sun. This seems to me to be the sense in which Kirchhoff used the word when he said it was extensive.* It certainly was the sense in which I used it, and, I believe, that in which all who spoke of an extensive atmosphere did so use it. In this sense there can be no doubt that the sun *has* an extensive atmosphere, the outer portion of which is comparatively cold and capable of reflecting light if the polarisation now not doubted be due to reflection.

There is one consideration which, however, does not seem to have occurred to Mr. Lockyer. If the cold atmosphere, as I will venture still to call it, reflect the prominence light, it will also reflect the solar light. Its *reflected* light then should be such as reaches us at ordinary times, and not so exclusively chromospheric. Adding to this the light which is due to cool hydrogen, we should have, I anticipate, a faint continuous spectrum with the bright line F, and also a solar spectrum with, perhaps, some of the chromospheric lines reversed. That is not what has been found, and I do not at present see any way of reconciling the facts with the theory that the undoubted polarisation is due to reflection.

Before going to another subject, I would wish to direct attention to my friend Captain (now Major) Branfill's observations in 1868† on the polarisation of the corona. Mr. Proctor, indeed, in his book on the Sun, says that the Astronomer Royal did not consider them conclusive, but I have his official statement that he did so consider them, and an inquiry as to Mr. Proctor's authority leads me to think that Mr. Airy's meaning was mistaken. I think any one who reads the account in the original will feel that the plane of polarisation was satisfactorily determined. An observer in 1870 has said that he found the bands of Savart persistent. I have not now time to look up the reference, but he used, it seemed to me, the centre of the moon as the centre of rotation. Captain Branfill was careful not to do this, as his figures prove (page 25 of Report).

Now to the future. I have received from Government an inquiry as to recommendations to observers coming out. I am now suggesting, in addition to my own station at Dodabetta, that observers should be stationed at Kotagherry in the Nilgherries, at Manantoddy among the coffee districts to the west, and at Tirupur, close to Avenashy Road Station of the Madras Railway. Of these Manantoddy is the least accessible, but the whole will give a range of stations from 8,600 feet high down to the ordinary level of the plain country. More observers could be accommodated on the Nilgherries, where the weather, I am assured, is likely to be excellent. Of Ceylon I have not satisfactory accounts, nor of the west coast.

If these stations be adopted, I would suggest that, if possible, there should be a conference of observers. The possibility will depend on our leisure, which, probably, none of us can now foresee.

I should say that I have made these suggestions without reference to Mr. Pogson, because I know nothing of his plans, having received no answer to inquiries; it is possible those may modify projects, but any visitors should bear in mind that it is almost necessary that some European residences should be close to their stations. J. T. TENNANT

H.M. Mint, Calcutta, Sept. 11

* Mr. Lockyer has long ago shown that the Sun's atmosphere lies partly above and partly below the superior limit of the photosphere. The word Atmosphere was used by Kirchhoff in the manner indicated, because he believed the photosphere to be liquid.—ED.

† American observers seem never to have seen the Report.

### British Mosses

NOT having noticed in the last number of NATURE, Oct. 12, any correction made by either the Rev. Mr. Berkeley or Dr. Dickie, of a statement made by the former gentleman in the previous number, Oct. 5, which, as it reads, is calculated to lead to error, if left unnoticed, I send you this note.

In the short paragraph at p. 446, "Notaris on Mosses," Mr. Berkeley, in correcting a previous omission having reference to the genus Habrodon, states that *Conomitrium julianum* had been sent to Dr. Dickie by Mr. Wilson from his district," Warrington. This being only one side of the truth, I take the liberty of supplying the other side. Any person reading the paragraph as it stands would certainly suppose that this very elegant, and very remarkable moss was a native of the Warrington district, which it is not, nor of any other part of the British Isles that I am aware of. No doubt Dr. Dickie received fresh specimens of the moss from Mr. Wilson at Warrington, as I also did, but they were of foreign origin, and only cultivated by Mr. Wilson in his little conservatory at Warrington, where he had them placed in a large-mouthed jar filled with water, in which condition I saw the plants during the month of October, 1870, on the occasion of the last visit I paid to my now departed friend.

I may further remark that I had been led to suppose it was Dr. Schimper, of Strasburg, who first made known the genus Habrodon to be British. In the summer of 1865 he and the late Mr. Wilson paid me a visit at Dublin, and after leaving Ireland, Dr. Schimper accompanied a party to the Highlands of Scotland, on which excursion the Habrodon was discovered growing on trees near Killin, whence I have specimens from the party, which were collected on that occasion.

Glasniven, Oct. 16 D. MOORE

### Corrections

A PARENTHETICAL passage in my "note on the Cycloid" has been transposed. Instead of "(a luminous point for the nonce) the sun in the meridian," &c., it should have been "the sun (a luminous point for the nonce) in the meridian," &c.

In Mr. Abbott's paper on $\eta$ Argus and its surrounding nebula there occurs the statement that I consider "an increased or decreased distance in space may account for the fluctuations of the nebula." I have never suggested such an explanation. What I have said is that the fluctuations, if real, would seem to suggest that the nebula has not those inconceivably vast dimensions which would correspond to the vast distance once assigned to it. My opinion was (and is), *not* that the nebula is nearer than it was formerly, but that it is nearer than it was formerly supposed to be. RICHD. A. PROCTOR

### A Universal Atmosphere

WILL you permit me to ask Mr. Mattieu Williams how, on his hypothesis, "that the atmosphere is universal, and that each planet attracts to itself an atmosphere in proportion to its mass," he accounts for the well-known fact that the moon shows no signs of an atmosphere sufficient to produce any indication of refraction during the occultations of a star?

I think Mr. Williams's book deserves far more attention than it has received, so I trust I shall be acquitted of any wish to indulge in carping criticism. JOHN BROWNING

111, Minories, October 10

### The Temperature of the Sun

HAVING been absent from home I have but just seen Mr. Ericsson's article on the "Temperature of the Sun" in NATURE, (No. 101, p. 449. All who feel an interest in the subject must be indebted to Mr. Ericsson for the experimental evidence which he has contributed to the investigation, and for such further light as his ingenuity will doubtless enable him to throw upon it; but few, I think, will be inclined to admit that the reasoning advanced in his recent article justifies in any degree the inferences which he has there drawn.

At the outset of the inquiry it does not seem very likely that we shall gain much correct knowledge of the condition of the solar atmosphere by inquiring what that condition would be if it were replaced by a medium similar to the terrestrial atmosphere, and containing the same quantity of matter for corresponding areas of the spherical surface. If the case were otherwise it would be necessary to point out that Mr. Ericsson's numerical results are vitiated by his omission to consider that the volume of a sphere varies as the cube of the radius, and therefore that on the data assumed by him the earth's atmosphere raised to the temperature of the solar surface, instead of attaining a height of 279,006 miles, would barely reach to one-twelfth of that limit.

But I may further remark that the assumptions on which Mr. Ericsson's calculations are founded are open to many objections. It is far from certain that the direct proportion between the increase of volume of gases at constant pressure and the increase of temperature, holds good for an enormously high temperature such as prevails in the solar atmosphere, and it is certain that the resistance offered by that medium to the passage of radiant heat depends not solely or mainly on its temperature, but on its chemical—*i.e.* its molecular—constitution.

It may further be noted that Mr. Ericsson's experiments on the diminution of heat emanating from a disc of incandescent iron, according to the angle at which its face is inclined to a fixed thermometer, do not justify similar conclusions with regard to heat emanating from a mass of incandescent gases or vapours. At the same time it may be regretted that Mr. Ericsson has not given fuller details respecting the experiments in question, which may give valuable results irrespective of the conclusions to which he has applied them. JOHN BALL

### Flight of Butterflies

CAN you tell us where the yellow butterflies are going?

About ten days since, while chatting with several gentlemen at the Jackson Sulphur Well about caterpillars, one of them remarked that the worm was about, for, says he, the yellow butterflies are all going east.

We thought at first he was telling us a "fish story", but soon became convinced that he knew whereof he spoke, for while we sat there a great number of bright-coloured, medium-sized butterflies came by us, all winging their way towards the rising sun.

Now, we do not think that this fly is related to the caterpillar, for the moth that lays the egg of that destructive worm is a very different fly; nevertheless it is a singular fact that they are all going east.

I have been at several different points since leaving Jackson, and at every place they fly the same way. Can you tell us whither they go? Perhaps if you will ask the question in your widely-circulated journal, some naturalist, or somebody over to the eastward, may tell us where they rest. ALA

Mobile, Sept. 6

[A similar fact will be found recorded in our "Notes" respecting the *Urania leilus.*—ED.]

### Velocity of Sound in Coal

THIS is a very interesting subject, at least to those who have anything to do with coal mines. And yet I have not met with anything that points to it, nor any formula whereby it might be calculated. But perhaps this is a subject to which the attention of physicists has not been drawn. I have been told that blasting has been heard at the distance of 150 yards underground, and I have heard the signals of the colliers, *i.e.*, by hitting the surface of the coal with one of their tools, at the distance of fifty or sixty yards, and have also heard the shouts of the men at the distance of fifteen yards; but I have never met any person who could give the velocity, nor seen any book on physics in which there is anything concerning it. But perhaps it is a very hard subject to deal with from the difference of the specific gravity of the coals, and also the different temperatures that we meet there. And if from these different causes it would be hard to find the real velocity, yet by calculating a velocity that might be rather theoretical at first, we might by degrees come nearer the truth.

D. JOSEPH

Ty Draw, Pontyfridd, Oct. 5

### Prof. Newcomb and Mr. Stone

I AM obliged to Mr. Lynn for pointing out that the statement by "P. S." was contradicted. I had not been aware of this. It never occurred to me to doubt either the authorship or the authenticity of the statement. I cannot tell how it chanced that "W. T. L 's" response escaped my attention. Perhaps I never saw the January number of the *Astronomical Register;* or, perhaps, a variety of other reasons which would not interest your readers.

The only point of the least interest in the matter (if the matter has any interest at all) is the fact that Prof. Newcomb did not discuss the observations of 1769, as I had believed. I have already admitted this, and withdrawn those expressions of commendation which I had founded on the strongly-worded letter of Prof. Smyth, so that I am rather at a loss to know what purpose Mr. Lynn had specially in view when he wrote his letter. I thank him, however, as warmly as though I knew what he meant.

RICHD. A. PROCTOR

## *SCIENCE AT THE UNIVERSITIES*

THE following courses of lectures will be delivered at the University of Oxford in Natural and Physical Science during the ensuing term:—The Sedleian Professor of Natural Philosophy, the Rev. Bartholomew Price, M.A., will deliver a course of Lectures on Light, on Tuesdays, Thursdays, and Saturdays, at one o'clock, commencing October 19th, at the Lecture Room, Museum, Upper Corridor South. The Savilian Professor of Astronomy, the Rev. C. Pritchard, M.A., proposes to give two courses of lectures during the present term; the one on Astronomical Instruments, the other on the Lunar Theory. The Professor of Experimental Philosophy, R. B. Clifton, M.A., will give a course of Lectures on Experimental Optics, on Wednesdays and Fridays, at twelve o'clock, commencing October 20, at the Physical Laboratory, University Museum. The Physical Laboratory of the University will be open daily for instruction in Practical Physics, from ten to four o'clock, on and after Thursday, October 19. The Linacre Professor of Anatomy and Physiology, G. Rolleston, D.M., will lecture on Circulation and Respiration, on Tuesdays, Fridays, and Saturdays, at one o'clock, commencing October 20, at the Museum. The Professor proposes to form classes for Practical Instruction, as in former terms. Persons who join these classes will come to the lectures on Saturdays at one o'clock, and will also come to the Museum on three mornings in the week for study and demonstration, under the superintendence of Mr. Charles Robertson, the Demonstrator of Anatomy, and Mr. C. S. Taylor, of Merton College. The Hope Professor of Zoology, J. O. Westwood, M.A., will not lecture during the present term, being engaged in the classification of the Hope, Burchell, Bell, and other collections, at the New University Museum, where he will be happy to see gentlemen desirous of studying the Articulated Animals, daily, between 1 and 5 P.M. A course of lectures will be given on behalf of the Professor of Chemistry, by A. Vernon Harcourt, M.A., in continuation of the Professor's course, on Tuesdays and Saturdays, at eleven o'clock, commencing October 21, at the Museum. There will also be an Explanatory and Catechetical Lecture on Thursdays, at eleven o'clock, to commence on Thursday, October 26. The Laboratory of the University will be open daily for instruction in Practical Chemistry from 9 A.M. to 3 P.M., on and after Monday, October 16. The ordinary course of instruction in the laboratory includes those methods of Qualitative Analysis, a knowledge of which is required of candidates for honours in the School of Natural Science who make Chemistry their special subject. In addition to this two courses of instruction will be given in the Laboratory, the one on the Methods of Qualitative Analysis, the other a course of elementary practical instruction in Chemical Manipulation, intended for those commencing the study of Chemistry.

At Cambridge the following lectures in Natural Science will be delivered during Michaelmas Term in connection with Trinity, St. John's, and Sidney Sussex Colleges:—On Electricity and Magnetism (for the Natural Sciences Tripos), by Mr. Trotter, Trinity College, on Mondays, Wednesdays, and Fridays, at 10, commencing Wednesday, October 18. On General Physics, Sound, and Light (for the Natural Sciences Tripos 1872, and following years), by Mr. Trotter, Trinity College, on Tuesdays, Thursdays, and Saturdays, commencing Thursday, October 19. On Chemistry, by Mr. Main, St. John's College, on Mondays, Wednesdays, and Fridays, at 12, in St. John's College Laboratory, commencing Wednesday, October 18. Attendance on these lectures is recognised by the University for the certificate required by medical students previous to admission for the first examination for the degree of M.B. Instruction in Practical Chemistry will also be given. On Palæontology (the Protozoa and Cœlenterata), by Mr. Bonney, St. John's College, on Mondays, Wednesdays, and Fridays, at 9, commencing Wednesday, October 18. On Geology (for the Natural Sciences Tripos, preliminary matter and Petrology), by Mr. Bonney, St. John's College, on Tuesdays and Thursdays, at 9, commencing Thursday, October 19. A course on Physical Geology will be given in the Lent Term, and on Stratigraphical Geology in the Easter Term. Papers will be given to questionists every Saturday at 11. On Botany, for the Natural Sciences Tripos, by Mr. Hicks, Sidney College, Tuesdays, Thursdays, and Saturdays, at 11, beginning on Tuesday, October 31. The lectures during this term will be on Vegetable Morphology. Mr. Hicks will also give examination papers in Botany to candidates for the next Natural Sciences Tripos on Mondays, at 1 P.M., beginning October 30. These examinations will be free to those who have attended the botanical lectures of the last term. On the Elements of Physiology, by the Trinity Prælector in Physiology (Dr. M. Foster), Mondays, Tuesdays, and Wednesdays, at 11 A.M., commencing Monday, October 23. A course of Elementary Practical Physiology, on Wednesdays and Thursdays, commencing Wednesday, October 25, at 2 P.M.

## *AN EXPLOSION (?) ON THE SUN**

ON the 7th of September, between half-past 12 and 2 P.M., there occurred an outburst of solar energy remarkable for its suddenness and violence. Just at noon the writer had been examining with the telespectroscope an enormous protuberance or hydrogen cloud on the eastern limb of the sun.

It had remained, with very little change since the preceding noon, a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, andfloated above the chromosphere, with its lower surface at a height of some 15,000 miles, but was connected to it, as is usually the case, by three or four vertical columns brighter and more active than the rest. Lockyer compares such masses to a banyan grove. In length it measured 3′ 45″, and in elevation about 2′ to its upper surface, that is, since at the sun's distance, 1″ equals 450 miles nearly, it was about 100,000 miles long by 54,000 high.

At 12.30, when I was called away for a few minutes, there was no indication of what was about to happen, except that one of the connecting stems at the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another at the northern end a little brilliant lump had developed itself, shaped much like a summer thunderhead.

What was my surprise, then, on returning in less than half an hour (at 12.55), to find that in the meantime the whole thing had been literally blown to shreds by some inconceivable uprush from beneath. In place of the quiet cloud I had left, the air, if I may use the expression, was filled with flying *débris*—a mass of detached vertical fusiform filaments, each from 10″ to 30″ long by 2″ or 3″ wide

* From the *Boston Journal of Chemistry*, communicated by the author.

brighter and closer together where the pillars had formerly stood, and rapidly ascending.

When I first looked, some of them had already reached a height of nearly 4′ (100,000 miles), and while I watched them they rose with a motion almost perceptible to the eye, until in ten minutes (1.5) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurement; the mean of three closely accordant determinations gave 7′ 49″ as the extreme altitude attained, and I am particular in the statement because, so far as I know, chromospheric matter (red hydrogen in this case) has never before been observed at an altitude exceeding 5′. The velocity of ascent also, 166 miles per second, is considerably greater than anything hitherto recorded.

As the filaments rose they gradually faded away like a dissolving cloud, and at 1.15 only a few filmy wisps, with some brighter streamers low down near the chromosphere, remained to mark the place.

But in the meanwhile the little "thunder head," before alluded to, had grown and developed wonderfully into a mass of rolling and ever-changing flame, to speak according to appearances. First it was crowded down, as it were, along the solar surface; later it rose almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads which were most curiously rolled backwards and downwards, like the volutes of an Ionic capital: and finally it faded away, and by 2.30 had vanished like the other.

The whole phenomenon suggested most forcibly the idea of an *explosion* under the great prominence, acting mainly upwards, but also in all directions outwards, and then after an interval followed by a corresponding inrush: and it seems far from impossible that the mysterious coronal streamers, if they turn out to be truly solar, as now seems likely, may find their origin and explanation in such events.

The same afternoon a portion of the chromosphere on the opposite (western) limb of the sun was for several hours in a state of unusual brilliancy and excitement, and showed in the spectrum more than 120 bright lines whose position was determined and catalogued—all that I had ever seen before, and some fifteen or twenty besides.

Whether the fine aurora borealis which succeeded in the evening was really the earth's response to this magnificent outburst of the sun is perhaps uncertain, but the coincidence is at least suggestive, and may easily become something more, if, as I somewhat confidently expect to learn, the Greenwich magnetic record indicates a disturbance precisely simultaneous with the solar explosion.

C. A. YOUNG

Dartmouth College, September 1871

## THE KEA—PROGRESS OF DEVELOPMENT

A NOTICE of the development of a striking change in the habits of a bird may be considered by naturalists interesting enough to justify a brief record in your journal. The Kea (*Nestor notabilis*) may be seen and heard in certain localities amidst the wild scenery of the Southern Alps in the middle island of New Zealand, for it is not so rare as has been described. This fine bird belongs to one of our indigenous genera, an examination of its structure proves that it shares with the *Kaka* a claim to a position amongst the *Trichoglossinæ* or Brush-tongued Parrots; the under side of its thick tongue near the tip is fringed with papillæ, enabling it to collect the sweets of its favourite blossoms. Through how many years has this species been content to range over shrub-covered heights and rock-bound gullies, gathering its subsistence from the nectar of hardy flowers, from the drupes and berries of the dwarfed shrubs that contend with a rigorous climate, and press upwards almost to the snow line of our Alpine giants? To these food-resources may be added insects found in the crevices of rocks, beneath the bark of trees, and its aliment not wholly vegetarian, yet such as called forth no display of boldness in order to procure a sufficient supply. This peaceful demeanour was observed under the ascendency of Moaic conservatism. The European has been the means of corrupting the simplicity of its ancient habits; the meat-gallows of the back-country squatters attracted the attention of our mountain-parrots in the winter season. To them they became points of interest in their wanderings, and furnished many a hearty meal torn from the dangling carcass as it swung in the frosty air; neither were the drying sheepskins, stretched on the rails of the stockyard, neglected. The Paneka has been destined to supply the enterprising Kea with a dainty only equalled perhaps by that which the epicurean African cuts warm from his bovine victim—our *educated* bird now tears his food from the back of the living sheep. From a local paper one learns that, for the last three years the sheep belonging to a settler "in the Wanaka district, (Otago) appeared afflicted with what was thought to be a new kind of disease; neighbours and shepherds were equally at a loss to account for it, having never seen anything of the kind before. The first appearance of this supposed disease is a patch of raw flesh on the loin of the sheep, about the size of a man's hand; from this matter continually runs down the side, taking the wool completely off the part it touches, and in many cases death is the result. At last a shepherd noticed one of the mountain parrots sticking to a sheep and pecking at a sore, and that the animal seemed unable to get rid of its tormentor. The runholder gave directions to his shepherds to keep watch on the parrots when mustering on the high ground; the result has been that during the present season when mustering high upon the ranges near the snow line, they saw several of the birds surrounding a sheep which was freshly bleeding from a small wound in the loin; on other sheep were noticed places where the Kea had begun to attack them, small pieces of wool having been picked out."

From the recent settlement of the country, it would be quite possible to date each step in the development of the destructiveness of the Kea, the gradual yet rapid change from the mild gentleness of a honey-eater, luxuriating amidst fragrant blossoms when the season was lapped in sunshine, or picking the berried fruits in the more sheltered gullies when winter had sternly crushed and hidden the vegetation of its summer haunts. Led, perhaps, to relish animal food from its partly insectivorous habits, its visits to the out-stations show something like the bold thievery of some of the Corvidae, whilst its attacks on sheep feeding on high ranges exhibit an amount of daring akin to the savage fierceness of a raptorial. Is the position of Nestor in our avifauna an anomalous one? A sucker of honey, devourer of fruit, destroyer of insects, render and tearer of flesh—will the difficulty be met by classing our mountain bird as omnivorous, or is it to be considered as only one other instance in which system puzzles and hampers the field naturalist?

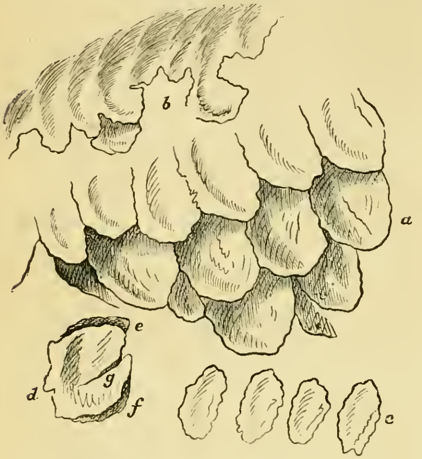
THOS. H. POTTS

## ON A NEW FORM OF CLOUD*

THE accompanying figure on p. 490, represents a form of cloud which I have seen but twice in my life;* the first time about the commencement of June 1871, at five o'clock in the evening, at Washington, U.S.; the second at Beloit, Wisconsin, U.S., during the same year, and at the same hour. The state of the atmosphere presented similar meteorological conditions at both times. The appearances coincided with

* See my new classification of clouds with sixteen engravings in the *Rural New Yorker*, January 29, February 26, April 9, May 21, June 4 and 11. It will be reprinted in the Report of the Smithsonian Institution for 1870, with an historical introduction, in print now for the next number of the *Annales Hydrographiques* of Paris.

a north-west storm passing slowly north of the city without bursting, and disappearing in the south-east. Great branched masses of cloud appeared suspended from a sheet of Pallio-Cirrus. Some resembled bunches of grapes (*a*), others stalactites (*b*) in a striking manner, and still others formed round balls (*c*) separated by the azure of the sky. These balls seemed to be formed of snow flakes, and approached the form of Cirro-Cumulus; one might say of masses of snow rolled upon themselves by the effect of electric currents developed during the storm. This was accompanied by thunder and lightning at Washington, and by lightning only at Beloit. *d* represents one of these balls detached, with two sorts of penumbra, darker in *e* and *f*, and a streak at *g*, the rest whitish. Somebody at Beloit told me he had seen this form of cloud two or three times. A slightly brilliant aurora borealis was seen at Beloit the same evening. The night of its appearance at Washington no aurora was visible, but I do not know whether there may not have been one in other parts of




the United States. The same evening and the next day at Beloit the temperature fell several degrees. It is a general belief that the aurora borealis is followed by a decrease of temperature. We know that in higher strata of the air vapour of water floats constantly in the form of frozen needles, especially in the polar regions. It is not impossible that these ice needles may be drifted by the electric current which engenders the aurora borealis* into lower latitudes, and thence towards lower strata of the atmosphere by the winds and storms. Hence the cooling of the air which is said to attend the aurora.

ANDRÉ POËY

## *EXOGENOUS STRUCTURES AMONGST THE STEMS OF THE COAL MEASURES*

THE perusal of Dr. M'Nab's reply to my short article on the existence of an exogenous process of growth amongst the cryptogamic stems of the coal measures, confirms my previous conviction that the discussion of *the details* of my proposition can lead to no beneficial results until the publication of my large store of new facts has been completed. Dr. M'Nab's article convinces me, as indeed is necessarily the case, that he has no conception either of the nature or of the extent of those facts. Were it otherwise, he would see at a glance how far his explanations are from accounting for them. He has given an exposition of a common process of exogenous growth, which is true as far as it goes; but I can assure him that the modifications of that process, so far as we can infer from peculiarities of structure, have been much more varied in past geological ages than he is aware of. He is pleased to affirm two things which require proof: (1) that I have "been led away by the mere superficial resemblance of the parts;" and (2) that I have "never tried to understand the homologies of these stems." To the first of these charges I plead not guilty; to the second I reply that I was *trying* to understand these things when he was a child at school. Whether or not I have succeeded remains to be seen, but as yet he has told me nothing new to me.

In studying the relations of the several parts of a plant, we have to consider three things, of which Dr. M'Nab has mainly dwelt upon one. These are—

1. The relative positions of the tissues.
2. The mode of their development.
3. The functions they have to perform.

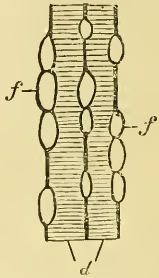



FIG. 1

The first point where I shall differ from Dr. M'Nab is in supposing that a correspondence on the first of these clauses invariably pre-supposes a similar correspondence on the second. I shall have to show on a future occasion that Nature has attained the same end in more ways than one; and that she refuses to be shut up to that dichotomous arrangement pre-supposed by Dr. M'Nab; but for the present I will limit my illustration to the particular mode of growth upon which he rests his case.

If we take a perfect Stigmaria, we find its centre (*a*, Fig. 2, p. 491) to be occupied by an axis of ordinary cellular parenchyma unmixed with any vascular tissue. This is surrounded by a ligneous or vascular cylinder (*b*) which, in its turn, is invested by a thick bark (*c*) consisting of a mixture of parenchyma and prosenchyma arranged in definite positions. The central axis differs in no respect whatever from the cellular piths of ordinary exogenous stems. The woody cylinder consists of vessels which, in the transverse section, are arranged in radiating lines (*d*) running from the pith to the bark; these lines are separated by intervening cellular tracts (*e*), which I, in common with Brongniart and Dr. Hooker, designate medullary rays. The radiating lines of vessels exhibit proofs of distinct interruptions to the process of growth, and afford clear evidence that the cylinder began as a thin ring of vessels surrounding the pith, and which grew, by successive concentric additions of vessels, to its peripheral surface where the cambium layer is found in ordinary exogens. We have here no trace of the limiting tissues of which Dr. M'Nab speaks; the growth has been free and prac-

* See my Memoir on the Development of Electricity during the Aurora Borealis in the "Annuaire de la Société Météorologique de France," 1861, vol. ix. p. 42.

tically continuous, in an outward direction, by the addition of layer after layer. The materials for the new vessels have obviously been furnished by some protoplasmic element which, whether we call it cambium, or choose to give it some other name, was located at the line of junction between the wood and the bark. The additions effected by its agency have gone on through successive ages until the thin vascular cylinder became a large hard-wooded stem capable of upholding a gigantic forest tree.

If we turn to the medullary rays, we find that they consist of vertical laminæ of cells. In the tangential section they appear as vertical lines of cells (*f*, Fig. 1, p. 490), undistinguishable from those seen in the corresponding sections of most conifers. In radial sections made in the plane of the medullary rays, we find that the latter proceed continuously from the pith to the investing bark, with each of which tissues they become intimately blended at their corresponding extremities. The component cells further exhibit, in this radial section, the mural arrangement so characteristic of ordinary medullary rays. As the vascular cylinder increased in diameter by additions to its exterior, so these medullary rays became lengthened by the similar addition of new cells to their outer extremities, such cells being supplied from the same source (cambium) as the corresponding new vessels.

Now, in all these processes of growth, I re-affirm that we have nothing which can, in any plain sense of the word, be termed *Acrogenous*. I can discern no material difference between what I have just described and what occurs in a *Cycad* or in a *Conifer*. In all these cases the additions are equally made to the exterior of a gradually enlarging cylinder, new cells being added to the outer extremities of the medullary rays, and vessels to the intermediate lines of vascular tissues; the raw material for both having been furnished, as in exogens, by some protoplasmic layer located between the vascular cylinder and the bark. I do not very clearly understand what Dr. M'Nab means when he speaks of a "pseudo-exogenous" growth, or of an "increase which takes place in the wood cells of the primitive tissues, not, as in Dicotyledons, by additions to the wood-cells of the fibro-vascular bundles." I detect no such difference as he seems to imply in the example which I have given.

If I rightly understand his meaning, Dr. M'Nab considers me to affirm that in all these cryptogamic plants of the coal-measures, there has been exactly the same process of growth, corresponding in each minute detail, as takes place during the growth of an oak tree. I have never affirmed this. On the contrary, I shall have to show that, amongst these coal plants, there are indications of many remarkable combinations and varied modifications of the process of growth.

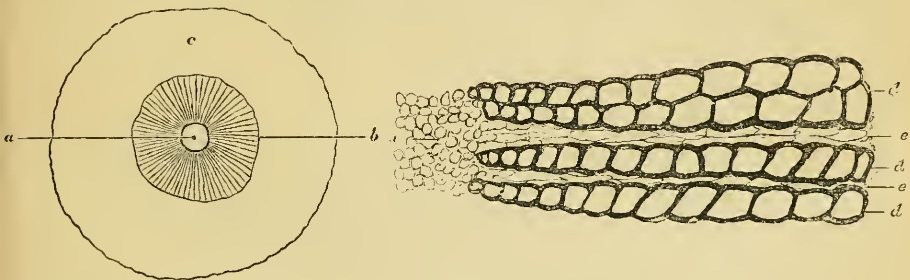



FIG. 2

Whether we do or do not accept the doctrine of evolution, we should expect to find such generalised combination amongst these primæval forms of vegetable life. I once more repeat, however, that these matters are scarcely capable of further discussion until my series of detailed memoirs has been published. When this takes place, I think Dr. M'Nab will see that I have not made the two "fatal errors" which he imagines I have done, and that there is more in my proposed classification than he, at present, has any idea of. At the same time I may remind him that the recognition of an exogenous process of growth amongst cryptogams is not now propounded for the first time. Dr. Hofmeister has given us most detailed accounts of such a process in his history of the development of Isoetes—itself a Lycopodiaceous plant. I merely propose to show that a mode of increment which now lingers in this one dwarfed genus amongst living Lycopodiaceæ, was once widely diffused, not only throughout this group of plants, but equally presented itself amongst the Calamitaceæ.

Prof. Dyer's temperate and intelligent reply to my article on the above subject resolves itself into two parts, the first of which deals with facts and the second with opinions. As to his facts he is in the same position as Dr. M'Nab. He is not acquainted with the materials for forming an opinion which I have in my hands, and upon which my views are based, consequently he has taken one extreme type of Lycopodiaceous stem, and made its supposed characters representative of the entire group. No. 129 of the Proceedings of the Royal Society which contains an abstract of my last memoir on the subject, would have shown him that I do materially differ from Mr. Carruthers in my interpretation of *Lepidodendron selaginoides*, the plant to which he refers, which difference of opinion I also expressed at the Edinburgh meeting of the British Association. I there showed that the central axis does not, as Prof. Dyer affirms, "consist wholly of scalariform vessels," but that these vessels are largely intermingled with true scalariform cells. But this is not all. The plant in question is but one of a large variety of forms. It occupies one end of a linear series of types—the opposite extremity of which series exhibits a very different aspect. The medullary vessels, which, in *Lepidodendron selaginoides*, are thus intimately commingled with the medullary cellular tissue, in the other types gradually recede from the centre to the periphery of the central cellular axis; the latter thus assuming the condition of a purely cellular parenchymatous pith, the cells of which are not even scalariform. The medullary vessels, thus driven to the periphery, now assume the position of the medullary sheath of the higher exogens. The vascular tissues for which I claim an exogenous origin are superadded to the exterior of this vascular medullary cylinder. We thus see that the central axis of these plants, instead of consisting of two parts, as Prof. Dyer affirms, really consists of three,* viz, a central cel-

* Stigmaria is an exception. In it the medullary vessels are altogether absent, as stated in my reply to Dr. M'Nab.

lular pith, an inner ring of vessels belonging to the medullary portion of the axis, and an external vascular cylinder, which grows by additions to its exterior, and which no more belongs to the central medulla than do the ordinary wood layers of an exogenous phanerogam. It has unquestionably been the product, as Prof. Dyer admits to be probable, of a cambium layer.

Speaking of the Lycopodiaceous stems of the coal measures, Prof. Dyer says, "I am inclined to think, with Prof. Williamson, that the stem increased in thickness." This point is not one to be thought about as if it was uncertain. We have in our museum accurate casts of the Dixonfold trees, and the base of the stem of the largest of these, above the point whence the huge roots are given off, is twelve feet in circumference! Higher up it is eight feet. There is surely no room for questioning an increase of thickness here, and this instance is but one example of what is sufficiently common in the coal measures. When we turn to the interior of these large trees, we find, as I have abundant evidence to prove, that they were enabled to sustain their huge bulk by an exogenous development of their outer cylinder of vessels, which were not mere modifications of the medullary vessels, but something superadded. This woody structure was amply provided with medullary rays, and each of the several layers of the thick bark increased *pari passu* with an increase of the ligneous zones, whilst a large cellular pith occupied the centre of the stem. So much for the facts, which are very different from those recognised by either of my two opponents. Now as to opinions, Prof. Dyer says he thinks that this increase was "nothing more than an incident in the life-history of a particular race of plants, nothing more than an adjustment to an arborescent habit dropped when the arborescent habit was lost." I am not sure that I understand all that Prof. Dyer means in this passage. He appears, however, to imply that these exogenous conditions were merely adventitious growths assumed for a season, and thrown off at the earliest opportunity; that they had no true affinity with the plants in which they were found. I confess I see no grounds for so remarkable a conclusion, especially remembering that, at least, these conditions lasted throughout the vast duration of the Devonian and Carboniferous ages. That one object of the exogenous growth was to enable these trees to sustain a huge superstructure, is doubtless true, though we find that growth in myriads of plants that have no such ponderous superstructures; but must we not say the same thing of the oak and the beech, as well as of the Lepidodendra? I see no difference between the cases. We have no more reason for regarding these conditions as merely an incident in the life-history of a particular race in the one instance than in the other.

I will not now discuss the value of the terms exogen and endogen, since the question has little importance in reference to the present object. I will only say that the mode of growth of a plant appears to me to have equal value with the mode of reproduction. There is a fashion in these matters—and in some circles there is now a tendency to elevate the reproductive at the expense of the vegetative, with which I do not agree, but I repeat this is not a question essentially important at present. My two great objects have been, first to demonstrate the existence of the exogenous structure in the trees in question; and second, to show the absurdity of applying the term acrogen to trees so constructed.

The value of my proposed classification is an independent question. I attach but a limited importance to the artificial boundary-lines introduced by systematisers, and do not wish to assign more to my own than to those of others; nevertheless, such divisions are useful so far as they indicate affinities, and it is because I find such affinities in the plants before us, *unrecognised by existing classifications*, that I have suggested a new one. Whatever value different minds may attach to the fact, there exists the great vegetative difference upon which I have dwelt between the Lycopodiaceæ and the Calamites on the one hand, and the Ferns on the other. There is certainly something more involved in this fact than "the old division of plants into trees and shrubs," with which Prof. Dyer compares it. Such a division is merely one of size and duration, not of organisation. Herbs, if they belong to the exogenous group, are as truly exogenous in their type as the most gigantic trees of the same class. Size has nothing to do with the matter. The same uniformity of type, apart from size, exists amongst my fossil cryptogams. True, the exogenous growth attains the fullest development amongst the large trees—but all the rudiments of this growth are equally to be found in the small ones, as my forthcoming memoirs will demonstrate.

The outer exogenous growth must be distinguished from the primitive vessels of the central medullary axis. I have yet to publish a remarkable series of facts illustrating this point. I have stated in a previous article that, in one sense, the exogenous vessels are a development of the vascular bundles of the living Lycopods. This is teleologically true rather than morphologically. Viewed in the latter aspect the two groups of vessels are independent of each other. The medullary vessels may be, and often are, primitive tissues formed at the first growth of the plant or of its young branches. The exogenous ones are something added, furnished by a cambium layer. The two groups retain their independent positions permanently, just as in living exogens the medullary sheath remains distinct from the woody cylinder which encloses it.

W. C. WILLIAMSON

## NOTES

WE believe that the arrangements of the Eclipse Expedition are nearly all made, and that the numbers are now complete. The Expedition sails on Thursday next in the *Mirzapore*, arriving at Point de Galle on the 27th November, if all goes well. M. Janssen, we believe, is already *en voyage*. Prof. Respighi, of Rome, will accompany the English Expedition.

BOTH Mr. Hind and M. Stephan at Marseilles have obtained observations of Encke's comet. Mr. Hind thus writes: "It is a large, faint, and very diffused nebulosity—a different-looking object from what I remember it in one or two former returns, when it has been drawing just within reach of the telescope. The last observation on the 12th of October gives the following place:—At $9^h\ 16^m\ 18^s$ mean time at Twickenham, right ascension, $1^h\ 7^m\ 37^s.8$; north declination, 36° 47′ 38″. The ephemeris for this appearance, published in *Mélanges Mathématiques*, of the Academy of Sciences of St. Petersburg, and calculated by Herr von Glasenapp, of the Russian National Observatory at Pulkowa, required, according to the above observation, corrections of 36 seconds in right ascension and ten minutes in declination, subtractive in both elements. The comet's positions for the next few days will be nearly as follows:—

*For Midnight at Greenwich.*

| | R.A. h. | R.A. m. | Decl. N. Deg. | Decl. N. Min. |
|---|---|---|---|---|
| October 19 | 0 | 30·7 | 38 | 27 |
| ,, 21 | 0 | 17·8 | 38 | 45 |
| ,, 23 | 0 | 3·6 | 38 | 56 |
| ,, 25 | 23 | 48·3 | 38 | 59 |

THE Expedition to Moab, which has been organised by Dr. Ginsburg, and goes out under the auspices of the British Association, will leave England in January. Its object is to explore the geography, antiquities, and natural history of the region. Canon Tristram will accompany Dr. Ginsburg.

*Bulletin Astronomique de l'Observatoire de Paris* is the title of an official circular, containing meridional observations of the sun, moon, and planets, made at the National Observatory, and news as to comets, minor planets, and the like. It promises to be very useful, and it is to be wished that other observatories will follow M. Delaunay's example.

THE contract for the new telescope which Congress has authorised the National Observatory at Washington to procure has been given to Mr. Alvan Clarke, of Boston, the well-known manufacturer of astronomical apparatus. It is to be of twenty-six inches aperture, and to be completed, according to contract, in about two years. It is understood that Mr. Clarke will again visit Europe for the purpose of carefully examining the principal telescopes there before completing the one in question. He has already minutely examined Mr. Newall's 25-inch, the *chef d'œuvre* of our English opticians, Messrs. Cooke and Sons, of York.

THE College of Physical Science at Newcastle-on-Tyne is now fairly at work. Already nearly fifty students are enrolled, and more are expected. Professors Aldis, Page, Herschel, and Marreco delivered their introductory lectures to large and appreciative audiences. Each of the professors, while touching especially on his own particular branch of science, dilated on the advantages accruing from the study of physical science, not only to the student who desires a special technical education, but to the community at large. Prof. Aldis, while expressing a hope that the advantages of the College would be thrown open to women as well as to men, made the following admirable remarks on the study of mathematics by women:—"A mathematical training, by which I do not mean learning Euclid by heart, will be a good preparation for the study of political economy and for the study of nature; I think not a bad preparation for the proper management of a house, and the mother's duties towards her children. I am sure that the time spent in receiving such a training, even if by getting it a lad or a lass be obliged to commence active duties a year or two later, will be time well spent, and will give an impetus which will carry them both through life with an ease which scarcely anything else will afford." We understand that Professors Herschel and Marreco intend that physical and physico chemical measurements shall be practised by the students, although there is yet no physical laboratory. At the time of going to press the question of admitting ladies had not been decided. A fair start seems to have been made, and we can only wish the new college as prosperous a future.

TRINITY College, Cambridge, has, it appears, the power of electing to its Fellowships men of scientific or literary distinction, and we are extremely glad to learn that Dr. Michael Foster has been thus elected. Dr. Foster was recently appointed to the newly-created post of Prælector in Physiology at the College, and this election to a further share of the emoluments and administration of the College proves that the members of the foundation are determined to carry out their intentions of promoting the study of Physiology in Cambridge. A temporary laboratory has been fitted up in the New Museums of the University, in which Dr. Foster gives lectures, and conducts the practical teaching. At the same time Mr. Hopkinson, Senior Wrangler of 1871, was elected a Fellow of Trinity College. These elections are the first-fruits of the act of last session admitting Nonconformists to a full share of the benefits of the University.

MR. WALTER WILLIAM FISHER, B.A., was on Saturday elected to an open Natural Science Fellowship at Corpus Christi College, Oxford, the examiners for which, Dr. Odling and Mr. A. Vernon Harcourt, made honourable mention of Mr. Christopher Childs, Scholar of Merton College. Mr. Fisher entered at Worcester College, from whence be gained a Natural Science Postmastership at Merton College, and was placed in the first class in the Natural Science Schools in Trinity Term 1870.

MR. MOSCARDI, from the Somersetshire College, Bath, has been elected to a Mathematical Scholarship at Worcester College, Oxford, on the Finney Foundation, open *pro hac vice;* and Mr. White, from the Liverpool Institute, has been also elected an Exhibitioner.

IT is gratifying to learn that Her Majesty has conferred the honour of Civil Companion of the Bath on Mr. J. H. Parker, the distinguished antiquarian. It is not often that we find either Science or Art so highly recognised in England; but is the Companionship of the Bath the fittest reward we have to bestow on scientific merit?

THE forty-fourth annual meeting of the Association of German Naturalists and Physicians has lately been held in Rostock. It has entered on the fiftieth year of its existence, having been founded in 1822 by Oken, who brought together twenty-one naturalists in Leipzig. Since that time a meeting has been held each year, with five exceptions. In 1831 and 1832 the meetings were suspended on account of the prevalence of cholera; in 1848, on account of political disturbances; and in 1867 and 1870 on account of war. The *British Medical Journal* states that the recent meeting was not so numerously attended as usual, many of the members having probably been detained at their homes through a fear of their professional services being required on account of the occurrence of cholera. One of the principal features of the meeting was an eloquent address by Prof. Virchow, on the position and prospects of natural science in the new national life of Germany.

SEVERAL friends of the Saturday half-holiday movement in London have offered the sum of thirty guineas for competition to London field-naturalists and microscopists for the encouragement of Saturday afternoon field excursions for botanical, geological, and microscopical purposes. The Duchess of Sutherland offers ten guineas to botanists in three prizes for the best collection of mosses, including the Hepaticæ, obtained within twenty miles of London; the Countess of Ducie ten guineas to microscopists in three prizes for the best lists of the ponds and other aquatic resorts within fifteen miles of London, and the Microzoa found in them; and the Marquis of Westminster ten guineas to geologists in two prizes for the best list of open geological sections and exposures of the strata of the London district, giving the fossil species found in each section, and the characteristic species of each formation exposed, and for the best notes on the connection of the landscape scenery of the London district with its geology. This movement is an admirable one, and altogether to be commended. Professional collectors and dealers are wisely excluded from the competition, the prizes being intended exclusively for those with whom natural history pursuits are solely the recreation of their leisure after-business hours.

THE *Athenæum* states that Prof. Owen has written to the Mayor of Brighton, "on the subject of a survey of the Sussex Wealden deposits, the district made famous by the discoveries of Mantell." Any efforts made by Brighton to get together between the present date and August 1872, the date of the meeting of the British Association, a collection illustrative of the Iguanodon and other extinct animals, would be esteemed a favour, and would be appreciated by members and visitors. Prof. Owen recommends Mr. E. Charlesworth as peculiarly qualified for carrying out the scheme of the authorities, and benefiting permanently the Brighton Museum. After a recent meeting of the Town Reception Committee, Mr. Charlesworth addressed a few of the members of the Committee on the Weald deposits; but the town authorities have no power under existing Acts of Parliament to levy rates for palæontological researches.

THE Coventry Institute has arranged for a complete course of Science Classes in connection with the Department of Science and Art through the approaching winter, in inorganic chemistry, animal physiology, magnetism and electricity, physical geography, and mathematics. We are particularly glad to see that they are arranged for young persons and adults of both sexes.

IN reference to the threatened destruction of what still remains of the Druidical Temple at Avebury, a correspondent of the *Times* states that negotiations are in progress for the purchase of the land intended to have been sold for building allotments, so that the remains of this fine old temple shall remain in their present state.

PROF. PHILLIPS's so much looked-for work on the Geology of the Thames Valley is announced for publication. The Professor proposes to make it his text-book for a course of lectures on Oxford Geology, to be delivered this term at Oxford.

A SEVERE earthquake shock was felt at Callao and other places on the coast on August 21. The direction of the undulations was from N.W. to S.E., and the shocks lasted for fifteen seconds. Cero Azul and Pisco also suffered from the same shock.

ON Sunday the 8th of this month, a violent earthquake shock was felt at Pera and Constantinople. The motion lasted for about five seconds. No great amount of damage was done.

THE terrible fire at Chicago, which raged during the early part of last week, and of which the ravages far exceed those of the Great Fire of London, affords us an additional example from which to judge of the truth of the so-much-disputed assertion, that extensive fires are almost invariably followed by heavy downpours of rain, which have been caused by them. In this case the latest telegrams assure us that the fire was chiefly checked on the third and fourth days by the heavy and continuous downpour of rain, which it is conjecture was partly due to the great atmospheric disturbances which such an extensive fire would cause, especially when we are told that the season just previous to the outbreak of the fire had been particularly dry.

THE Association formed in California for the purpose of introducing Eastern fish into the waters of that State has received a first instalment in 15,000 young shad, hatched in the Hudson River just a week before, and brought in large tin cans filled to the shoulder with fresh water. They proved to be in excellent condition on their arrival on the Sacramento, and were taken thence higher up the river to Tehama, where it was proposed to plant them. The expenses of this enterprise are borne from an appropriation on the part of the State of 5,000 dollars for this special purpose.

A VERY remarkable collection of medicinal and other drugs has been brought together in the Exhibition of Natural Industry of the United States of Columbia or New Granada in the City of Bogota. Among febrifuges it includes the yellow quina of Zaragoza and the Sarpolata, which is considered more effective even than quina of dye plants. It is observed that Mr. P. M. Gonsalez has produced three shades of green from plants discovered by him in Antioquia. The Achivilla of that province produces golden yellow, the Bruja a splendid red, the Ojo Venado an intense black, and the plant of the Sagus a blue equal to indigo.

THERE is in the Museum at Cassel a curious collection illustrating European and other trees. It is in the form of a library, in which the back of each volume is furnished by the bark of some particular tree, the sides are made of perfect wood, the top of young wood, and the bottom of old. When opened the book is found to be a box, containing either wax models or actual specimens of the flower, fruit, and leaves of the tree.

THE *New York Times* states that a solid section cut from one of the original "big trees" of California is in New York on its way to a European Museum. Five men were employed twenty-five days in felling this huge tree; its height is 302 ft., and its largest diameter 32 ft. The specimen was cut at a distance of 20 ft. from the base. The stump is covered in, and is now used as a ball-room! It has been ascertained from counting the annular rings that the tree is more than 2,500 years old.

A CORRESPONDENT of the *Stationer* announces a new fibrous plant for paper-making purposes, the *Cineraria maritima*, or sea rag-wort. Several very satisfactory results have been received from various paper-makers as to its great utility for trade purposes, and there is every reason to believe, if proper attention is paid to its cultivation, it will in time become a staple article of commerce amongst manufacturers. The seed, at present, is imported from France and the south of Europe, but preparations are being made for growing it on a large scale in this country. The same journal, in an article on "Iron-paper-making," gives a history of the manufacture of the thinnest sheet of iron ever rolled, manufactured by Messrs. W. Hallam and Co., of the Upper Forest Tin Works, near Swansea. The sheet in question is 10in. by 5½in., or 55in. in surface, and weighs but 20 grains, which being brought to the standard of 8in. by 5½in., or 44 surface inches, is but 16 grains, or 30 per cent. less than any previous effort, and requires at least 4,800 to make 1in. in thickness.

IT is stated that tobacco in any form may be used with great advantage against snakes of all kinds. By pouring a decoction of it in suspected places, they are driven away, and this fact is known to both the natives of Hindostan and to those of North and South America. If it can be administered to them it is certain death.

IN his "Contributions towards the Materia Medica and Natural History of China," Mr. Frederick P. Smith records the following facts respecting the use of Fungi as food in the Celestial Empire:—Large quantities of Fungi are eaten by the Chinese of every province under the name of *Hiang-kw'an*, and have some medicinal or dietetic properties assigned to them. The Polypori, or Boleti, are generally preferred to the Agarics, so largely eaten in Europe. *Kwei-k'ai*, or *Ti-k'ai*, are edible Agarics, or Helvellæ, and perhaps include poisonous sorts. They are burnt and applied to swellings and sores. *Ti-rh* is probably an Agaric, said to be tonic in its effects. The *Muh-rh* are a numerous class of parasitic fungi growing on trees. They are much eaten. They come from Ching-ting fu in Peh-chihli, Shun-king fu and Sui-ting fu in Szch'uen, Li-p'ing fu in Kweichau, Yun-yang fu in Hupeh, and from Shang chau and Han-chung fu in Shen si. Manchuria and the Amur country supply a portion of this food. The *Shih-rh* is a Polyporus brought from Fung-t'ien fu in Shingking, Hwui-chau fu in Nganhwui, Nan-kang fu in Kiang-si, and from Lai chau in Hunan. *T'u-k'o'an*, or *Ti-tan*, are Agarics or Amanitas, or answer to the "toad-stools" and other injurious fungi. Some of them are said to cause irrepressible laughter. Alum and chicory are reported to be antidotal to their poison. Japanese mushrooms appear in the tariff as *Tung-yang-hiang-ku*.

MUCH interest was excited in the scientific journals some time ago by the accounts given in the Panama papers of the flights of a beautiful butterfly, the *Urania leilus*. By late advices from Panama we learn that these insects were passing over that city, from west to east, in July last, in very large numbers, and in some cases were attracted into houses by the light so as to almost fill the apartments. They are said to be accompanied during the day by swallows and swifts, and in the night by the different species of goat-sucker, which probably destroy large numbers. Nothing is at present known, however, of the place whence they came, nor the region to which they are ultimately bound.

## *SCIENTIFIC INTELLIGENCE FROM AMERICA* *

SOME of our readers are probably aware of the important archæological discoveries made a few years ago in the island of Cyprus by Mr. L. Di Cesnola, United States consul at that island, and of the interest which they excited throughout the civilised world. This consisted in the finding of a buried city, and of numerous graves of the ancient Phœnicians and other early races of the island of Cyprus, previously entirely unknown. Excavations were prosecuted by him at great expense, and resulted in the accumulation of an enormous mass of treasures of art of gold, silver, bronze, pottery, &c. Various government authorities and public museums of Europe have, it is understood, opened negotiations for the acquisition of the entire collection, and it was stated that an offer had been made from Boston for their purchase; but nothing definite appears to have been accomplished. It is said that of the various offers, one on the part of the French Government was most satisfactory, but that the consummation of the purchase was prevented by the late war. The value of these treasures will be shown by the following enumeration of the specimens of the collection, especially when we bear in mind that many of them are most exquisite specimens of art, and all are of undoubted authenticity and great antiquity:—

| | |
|---|---|
| Antique Greek, Phœnician, and Roman glass-ware unguentaries, bottles, bracelets, tear-bottles . . . | 1200 |
| Phœnician, Assyrian, Egyptian, and Greek vases from three feet in height to two inches . . . . | 4000 |
| Greek and Roman and Byzantine lamps, with and without bas-reliefs and inscriptions . . . . . . | 1400 |
| Bronzes of every kind, strigiles, pateras, fibulas, speculas, spear-heads, &c. . . . . . . . . | 420 |
| Phœnician, Greek, and Cypriote (?) inscriptions . . | 96 |
| Stone statues of every size (Temple of Venus) . . | 204 |
| Stone heads of every size (Temple of Venus) . . . | 790 |
| Terra-cotta statuettes, votive offerings, &c., . . . | 320 |
| Gold objects, cylinders, scarabees, &c., . . . . | 130 |
| | 8560 |

These were obtained by excavating at least 8,000 graves, and from the Temple of Venus at Golgos, the discovery of which by Mr. Cesnola was scarcely inferior in archæological importance to that of ancient Nineveh by Mr. Layard. In this were found numerous inscriptions in an unknown Semitic language (Cypriote?).—In previous numbers we have given an account of certain deep-water explorations in the great lakes, which resulted in the detection of species of crustaceans and of fishes new to science, and belonging to marine rather than to fresh-water types. This, of course, does not prove the occurrence of other marine conditions at the bottom of the lakes, such as salinity of the water, &c., although it may perhaps excite a suspicion to that effect. Additional researches have been prosecuted during the present season in this direction, two parties being engaged in them—namely, Mr. James W. Milner, of Waukegan, and Mr. Sidney J. Smith, of Yale College, the former working principally in Lake Michigan, and the latter under the auspices of the Engineer Department, in Lake Superior. Both these gentlemen have carried on their labours at depths exceeding 100 fathoms, and have determined the existence of various novel forms of animal life, of which due mention will be made hereafter.—Professor J. D. Whitney, in a recent communication to the Academy of Sciences of San Francisco upon the use of the barometer in determining altitudes, remarked upon the effect which temperature exerts upon the instrument, and stated that the difference between the cold of winter and the heat of summer would sometimes, in the same instrument, involve a difference in the estimate of a given height of as much as seventeen feet. He hoped in time to have tables prepared which should give the allowances that must be made for each day of the year, and for different times in the day, an observation at 9 A.M. sometimes giving a different result from one taken at 2 P.M. at the same altitude on the same day. He also expressed his dissatisfaction with the aneroid barometer as a means of measuring altitudes, although he had experimented with the best that were offered in the market. He found them reliable for a certain time only, and they appeared to have spells of irregularity from which they recovered very slowly. He did not find any upon which he could rely for heights above 1,000 feet.—From the *Alaska Herald* we learn that M. Alphonse Pinart had reached Nushigak on the 31st of May, where he was received very cordially by the authorities. While there he made numerous photographic pictures of the scenery, and gathered collections in ethnology and palæontology. He left Nushigak on the 16th of June, on board the steamer *John Bright*, for the Yukon River, and expected to reach the interior in time to attend the great July fair held by the Yukon Indians.

* Communicated by the Scientific Editor of *Harper's Weekly*.

## *PROF. HUXLEY ON THE DUTIES OF THE STATE*

WE are able to give the following extracts from Prof. Huxley's address at Birmingham, to which we alluded last week:—The higher the state of civilisation the more completely did and must the action of one member of the social body influence all the rest, and the less possible was it for any one man to do a wrong thing without interfering more or less with the freedom of all his fellow-citizens. So that, even in its narrowest views, the functions of the State, it must be admitted, should have a wider power than even those who, without this doctrine of administration, were willing to admit. It was urged, he was aware, that if the right of the State was conceded to assign limits at all, there would be no stopping it, and that the principles which justified the State in enforcing vaccination and education also justify it in prescribing his religious belief, and mode of carrying on his trade or profession, or in determining the number of courses he should have for his dinner, or the pattern of his waistcoat. But surely the answer was obvious, that on similar grounds the right of a man to eat when hungry might be disputed, because if he were allowed to eat at all he must be allowed to use that faculty which told him he must not surfeit himself. But in practice every one knew that a man left off when reason told him that he had had enough. And so, properly argued, the State, or governing body, would find out when reason was carried far enough. But so far as his acquaintance with those who carried on the business of Government went, it was that they were far less eager to interfere with the people while the people were keenly sensitive. He could not discover that Locke affected to put the doctrine of modern liberation—that the toleration of error was a good thing in itself, to be reckoned amongst the cardinal virtues; on the contrary, he was strongly opposed to this, and he laid it down that whenever it was necessary for the preservation of civil society that toleration should be withdrawn it ought to be withdrawn. . . . There must be strong and cogent reasons for legislation on abstract matters, before the governing body entered upon such a course of legislative action as that of which he had spoken, and which might tend towards that state contemplated by the champions of Nihilism. He then quoted the doctrine laid down by Mr. Herbert Spencer, to the effect that the relations of political bodies bore a strong resemblance to vertebrate animals in their organisation, and that as the brain was the guiding power of the animal, so in communities the Government answered the same purpose. . . . . . In fact, much of our social relations were based upon this simple law—that one man established his right to the one thing, and in another direction to abstain from doing another thing. In many cases government degenerated, and became a recognised system for effecting fraud and plunder; but wherever sound social relationships existed between different members composing the social life of a country, this was impossible. But to reach this every man, and the aggregation of men in communities, limited their independence. He next spoke on individual responsibility, and said that it was the duty of the individual to protect society; if the individual breaks all bonds, then society perishes. The welfare of the social organisation depended not only on the brain, or the government, but on the members; but unquestionably a good deal depended on what the functions of the Government were. This touched at the root of social organism, and the problem which had presented itself to many minds was one not easy to solve. John Locke had furnished them with an answer which for a time sets the matter at rest. The end of a Government is the good of mankind. The good of mankind was not something which was an absolute fixed thing for all men, whatever their capacities. It was possible to maintain the individual freedom, and promote the higher functions that the government has translated into another sphere; but what ought we men in our corporate capacity to do in the way of restraining the free individual in that which was contrary to the existence of nature? John Locke had furnished them with the solution—true *civitas Dei*—in which every man's faculty was such as to allow him to control all those desires which ran counter to the good of mankind, and cherish those only which would benefit the welfare of the whole of society, and which every man felt as sufficiently true to enable him to know what he ought to do. Society as now constituted consisted of a considerable number of the foolish and the ignorant—a small proportion of good genuine knaves and a sprinkling of capable and honest men, by whose efforts the former were kept in a reasonable restraint. Such being the case, he could not see how the limit could be laid down as to the question which, under some circumstances, the action of Government might be rightfully carried on, The question was where they ought to draw the line between those things which a State ought to do, and which they ought not to do. The difficulty which met the statesmen was the same as that which met all of them in individual life. Moore and Owen, and all the great modern Socialists, bear witness that Government might attain its end for the good of the people by some more effectual process than the very simple and easy one of letting all matters of enterprise alone. He thought that the science of politics was but imperfectly known; and that perhaps they would be able to get clearer notions of what a State might or might not do, if they estimated the truth of the proposition, that the end of government is the good of mankind. It was necessary to consider a little what the good of mankind really was. The good of mankind meant the admission of every man to all the happiness which he could enjoy without diminishing the happiness of his fellow men. Having dwelt at some length on this point, Mr. Huxley went on to say that it was universally agreed that it would be useless to admit the freedom of sympathy between man and man directly; but he could see no reason why the State might not do many things towards that end indirectly. He was not going to argue that there should be a State science, or a State organisation, such as they had seen in France, by which all scientific teaching was to be properly regulated. On the contrary, the State had left local enterprise to work out its own ends as soon as local intelligence and energy proved itself equal to the task. These local efforts not only benefited the localities; but every means of teaching, every stimulus given to intellectual life was so much positively added to the wealth and welfare of the nation, and as such deserved some equivalent modicum of support from the general purse. But if the positive advancement of the peace, wealth, and intellectual and moral development of its members were the objects which the representative of the corporate authority of society, the Government, might justly strive after in the fulfilment of its end, which was the good of mankind, then it was clear that the Government might undertake the education of the people, for education promoted peace by teaching men the realities of life, and the obligations which were involved in the very existence of society; and promoted the intellectual development, not only by training the individual intellect, but by sifting out from the mass of ordinary or inferior capacities those which were competent to increase the general welfare by occupying higher positions; and lastly, it promoted morality and refinement by teaching men to discipline themselves, and leading them to see that the highest, as it was the only permanent, content was to be attained not by groveling in the rank stream of the foulest sense, but by continually striving towards those higher peaks where, resting in eternal calm, reason discerned the undefined but bright ideal of the highest good, "a cloud by day, a pillar of fire by night."

## ON THE STRUCTURE OF THE PALÆOZOIC CRINOIDS *

THE best known living representatives of the Echinoderm class Crinoidea are the genera *Antedon* and *Pentacrinus*—the former the feather stars, tolerably common in all seas; the latter the stalked sea-lilies, whose only ascertained habitat, until lately, was the deeper portion of the sea of the Antilles, whence they were rarely recovered by being accidentally entangled on fishing-lines. Within the last few years Mr. Robert Damon, the well-known dealer in natural history objects in Weymouth, has procured a considerable number of specimens of the two West Indian *Pentacrini*, and Dr. Carpenter and the author had an opportunity of making very detailed observations both on the hard and the soft parts. These observations will shortly be published.

* Abstract of a paper read before the Royal Society of Edinburgh, by Prof. Wyville Thomson, April 3, 1871.

The genera *Antedon* and *Pentacrinus* resemble one another in all essential particulars of internal structure. The great distinction between them is, that while *Antedon* swims freely in the water, and anchors itself at will by means of a set of "dorsal cirri," *Pentacrinus* is attached to a jointed stem, which is either permanently fixed to some foreign body, or, as in the case of a fine species procured off the coast of Portugal during the cruise of the *Porcupine* in the summer of 1870, loosely rooted by a whorl of terminal cirri in soft mud. Setting aside the stalk, in *Antedon* and *Pentacrinus* the body consists of a rounded central disc and ten or more pinnated arms. A ciliated groove runs along the "oral" or "ventral" surface of the pinnules and arms, and these tributary brachial grooves gradually coalescing, terminate in five radial grooves, which end in an oral opening, usually subcentral, sometimes very excentric. The œsophagus, stomach, and intestine coil round a central axis, formed of dense connective tissue, apparently continuous with the stroma of the ovary, and of involutions of the perivisceral membrane; and the intestine ends in an anal tube, which opens excentrically in one of the inter-radial spaces, and usually projects considerably above the surface of the disc. The contents of the stomach are found uniformly to consist of a pulp composed of particles of organic matter, the shields of diatoms, and the shells of minute foraminifera. The mode of nutrition may be readily observed in *Antedon*, which will live for months in a tank. The animal rests attached by its dorsal cirri, with its arms expanded like the petals of a full-blown flower. A current of sea water, bearing organic particles, is carried by the cilia along the brachial grooves into the mouth, the water is exhausted of its assimilable matter in the alimentary canal, and is finally ejected at the anal orifice. The length and direction of the anal tube prevent the exhausted water and the fœcal matter from returning at once into the ciliated passages.

In the probably extinct family Cyathocrinidæ, and notably in the genus *Cyathocrinus*, which the author took as the type of the Palæozoic group, the so-called Crinoidea Tessellata, the arrangement, up to a certain point, is much the same. There is a widely-expanded crown of branching arms, deeply grooved, which doubtless performed the same functions as the grooved arms of *Pentacrinus*; but the grooves stop short at the edge of the disc, and there is no central opening, the only visible apertures being a tube, sometimes of extreme length, rising from the surface of the disc in one of the inter-radial spaces, which is usually greatly enlarged for its accommodation by the intercalation of additional perisomatic plates, and a small tunnel-like opening through the perisom of the edge of the disc opposite the base of each of the arms, in continuation of the groove of the arm. The functions of these openings, and the mode of nutrition of the crinoid having this structure, have been the subject of much controversy.

The author had lately had an opportunity of examining some very remarkable specimens of *Cyathocrinus arthriticus*, procured by Mr. Charles Ketley from the upper Silurians of Wenlock, and a number of wonderfully perfect examples of species of the genera *Actinocrinus*, *Platycrinus*, and others, for which he was indebted to the liberality of Mr. Charles Wachsmuth, of Burlington, Ohio, and Mr. Sidney Lyon, of Jeffersonville, Indiana; and he had also had the advantage of studying photographs of plates, showing the internal structure of fossil crinoids, about to be published by Messrs. Meek and Worthen, State Geologists for Illinois. A careful examination of all these, taken in connection with the description by Prof. Lovén, of *Hyponome Sarsii*, a recent crinoid lately procured from Torres Strait, had led him to the following general conclusions.

In accordance with the views of Dr. Schultze, Dr. Lütken, and Messrs. Meek and Worthen, he regarded the proboscis of the tesselated crinoids as the anal tube, corresponding in every respect with the anal tube in *Antedon* and *Pentacrinus*, and he maintained the opinion which he formerly published (Edin. New Phil. Jour. Jan. 1861), that the valvular "pyramid" of the Cystideans is also the anus. The true mouth in the tesselated crinoids is an internal opening vaulted over by the plates of the perisom, and situated in the axis of the radial system more or less in advance of the anal tube, in the position assigned by Mr. Billings to his "ambulacral opening." Five, ten, or more openings round the edge of the disc lead into channels continuous with the grooves on the ventral surface of the arms, either covered over like the mouth by perisomatic plates, the inner surface of which they more or less impress, and supported beneath by chains of ossicles; or, in rare cases (*Amphoracrinus*), tunnelled in the substance of the greatly thickened walls of the vault. These internal passages, usually reduced in number to five by uniting with one another, pass into the internal mouth, into which they doubtless lead the current from the ciliated brachial grooves.

In connection with different species of *Platyceras* with various crinoids, over whose anal openings they fix themselves, moulding the edges of their shells to the form of shell of the crinoid, is a case of "commensalism," in which the mollusc takes advantage for nutrition and respiration of the current passing through the alimentary canal of the echinoderm. *Hyponome Sarsii* appears, from Prof. Lovén's description, to be a true crinoid, closely allied to *Antedon*, and does not seem in any way to resemble the Cystideans. It has, however, precisely the same arrangement as to its internal radial vessels and mouth which we find in the older crinoids. It bears the same structural relation to *Antedon* which *Extracrinus* bears to *Pentacrinus*.

Some examples of different tesselated crinoids from the Burlington limestone, most of them procured by Mr. Wachsmuth, and described by Messrs. Meek and Worthen, show a very remarkable convoluted plate, somewhat in form like the shell of a *Scaphander*, placed vertically in the centre of the cup, in the position occupied by the fibrous axis or columella in *Pentacrinus* and *Antedon*. Mr. Billings, the distinguished palæontologist to the Survey of Canada, in a very valuable paper on the structure of the Crinoidea, Cystidea, and Blastoidea (*Silliman's Journal*, January 1870), advocates the view that the plate is connected with the apparatus of respiration, and that it is homologous with the pectinated rhombs of Cystideans, the tube apparatus of Pentremites, and the sand-canal of Asterids. Messrs. Meek and Worthen and Dr. Lütken, on the other hand, regard it as associated in some way with the alimentary canal and the function of nutrition.

The author strongly supported the latter opinion. The perivisceral membrane in *Antedon* and *Pentacrinus* already alluded to, which lines the whole calyx, and whose involutions, supporting the coils of the alimentary canal, contribute to the formation of the central columella, is crowded with miliary grains and small plates of carbonate of line; and a very slight modification would convert the whole into a delicate fenestrated calcareous plate. Some of the specimens in Mr. Wachsmuth's collection show the open reticulated tissue of the central coil continuous over the whole of the interior of the calyx, and rising on the walls of the vault, thus following almost exactly the course of the perivisceral membrane in the recent forms. In all likelihood, therefore, the internal calcareous network in the crinoids, whether rising into a convoluted plate or lining the cavity of the crinoid head, is simply a calcified condition of the perivisceral sac.

The author was inclined to agree with Mr. Rofe and Mr. Billings in attributing the functions of respiration to the pectinated rhombs of the Cystideans and the tube apparatus of the Blastoids. He did not see, however, that any equivalent arrangement was either necessary or probable in the crinoids with expanded arms, in which the provisions for respiration, in the form of tubular tentacles and respiratory films and lobes over the whole extent of the arms and pinnules, are so elaborate and complete.

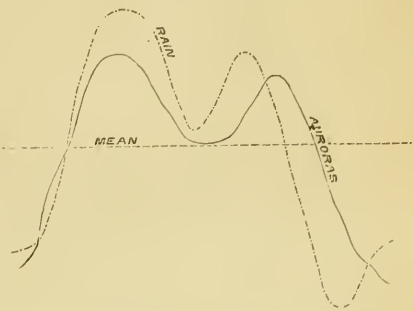
## *ON THE RELATION OF AURORAS TO GRAVITATING CURRENTS**

PROF. LOOMIS'S observations of the number of Auroras in each month of 1869 and 1870 (*American Journal of Science*, 3rd S., i. 309) are specially noteworthy, both because of the careful accuracy of the observer, and because they are the first published observations which furnish satisfactory data for an approximate determination of the laws of auroral distribution.

If the auroras are, as is now generally believed, luminous manifestations of terrestrial magnetism, it seems reasonable to look to them for some additional evidence upon the question of the relation between magnetic and gravitating currents. Messrs. Baxendell and Bloxam have already pointed out some resemblances between hyetal and magnetic curves (see Proc. A. P. S., x. 368) and if analogous resemblances can be traced between hyetal and auroral curves, they will be interesting and suggestive.

I have not found the similarity between the annual distribution of rainfalls and auroras sufficiently striking to impress any one who has not made a special study of the causes of resemblance and difference. But, as I have repeatedly urged, currents are subject to an increased number of disguising disturbances, in proportion to the sluggishness of their motion, and the time which is consequently required for their formation and change. We may very reasonably look for analogies between the daily and the annual auroral or magnetic curves, of a character for which we could hope to find no parallel in wind, rain, or ocean current curves.

If we desire, therefore, to find evidence of the joint influence of solar expansion and gravitating equilibrium, we should look where it is most likely to be found, and to the best of the observations which may be supposed to be fairly comparable. There are similar variations of solar altitude, and consequently increasing and diminishing solar force, in the day and in the year, but the effects of these variations upon the precipitation of vapour are more likely to be shown in their greatest simplicity by the means of observations at different hours of the day than at different seasons of the year. I know of no published observations of this character at New Haven, but there are some extending over a long series of years at Philadelphia and at Greenwich, the curves at each station indicating minima of rainfall at noon and midnight, and maxima in the morning and evening. The difference of longitude between Philadelphia and New Haven being less than $2\frac{1}{2}°$, it is not likely that there is any material difference in the daily rain-curves at the two places.




In order to make the curves fairly comparable, both in regard to the times and the magnitudes of deviation, I treated the auroral observations in the same manner as those of rainfall (Proc. A. P. S, x. 526). Both in the magnetic and in the hyetal phenomena, the greatest effects accompany the greatest atmospheric changes. But in the magnetic disturbances the principal maxima occur in the spring of the year and the morning of the day, while the general evaporation is increasing; whereas, in the daily rains at Philadelphia, the principal maximum occurs in the afternoon, when evaporation is diminishing. I have, therefore, compared the midwinter ordinate of the auroral with the noon ordinate of the rain curve, and the midsummer auroral with the midnight hyetal ordinate.

The auroral observations and the normal ordinates of the accompanying curves are given in the following table. I presume no one will doubt that the condensation of vapour, which is represented by the rain-curve, is occasioned by the simple operation of gravitation in blending currents of different temperatures, and I see no reason for postulating any different law for the development of electricity and magnetism in the aurora.

*Comparative Table of Auroras and Rainfalls*

| Month. | No. of Aurorals. | Normals. | Hours. | Normals of Rain. |
|---|---|---|---|---|
| | | 88 | 0 | 91 |
| January . . | 32 | 90 | 1 | 91 |
| | | 94 | 2 | 93 |
| February . . | 31 | 98 | 3 | 98 |
| | | 103 | 4 | 105 |
| March . . . | 41 | 107 | 5 | 110 |
| | | 109 | 6 | 113 |
| April . . . | 44 | 109 | 7 | 113 |
| | | 108 | 8 | 112 |

* Read before the American Philosophical Society, May 5, 1871, by Pliny Earle Chase.

| Month. | No. of Aurorals. | Normals. | Hours. | Normals of Rain. |
|---|---|---|---|---|
| May . . . | 36 | 106 | 9 | 109 |
| | | 103 | 10 | 105 |
| June . . . | 31 | 101 | 11 | 102 |
| | | 100 | 12 | 103 |
| July . . . | 38 | 101 | 13 | 106 |
| | | 103 | 14 | 109 |
| August . . | 34 | 105 | 15 | 108 |
| | | 107 | 16 | 104 |
| September . . | 43 | 106 | 17 | 98 |
| | | 103 | 18 | 92 |
| October . . | 38 | 100 | 19 | 87 |
| | | 95 | 20 | 85 |
| November . . | 27 | 91 | 21 | 87 |
| | | 89 | 22 | 90 |
| December . . | 30 | 87 | 23 | 91 |

## SCIENTIFIC SERIALS

*Jahrbuch der kaiserlich-königlichen geologischen Reichsanstalt.* Vol. xvi. No. 1. (Vienna.) The first paper in this part of the *Jahrbuch* is one by Prof. Kreuz, "Das Vihorlat-Gutin-Trachytgebirge." This is one of those painstaking lithological papers which are less commonly met with in our own scientific journals than one could wish. The author has carefully examined under the microscope the trachytic rocks of the Vihorlat-Gutin mountains of North-eastern Hungary, a range which stretches from north-west to south-east in the same direction as the Carpathian Sandstones. He groups the rocks under three divisions:—(1) Augite andesite; (2) Sanidine-oligoclase-trachyte; (3) Breccias and Tuffs; and his descriptions of the two former are particularly full and interesting. The breccias and tuffs are necessarily less susceptible of clear concise description; they appear to vary as much and in as short a space as similar volcanic accumulations elsewhere.—Prof. Koch, of Ofen, contributes "Beitrag zur Kentniss der geognostischen Beschaffenheit des Urdniker Gebirges," an isolated little mountain range, which stretches between the Danube and the Save in East Sclavonia. He describes the Tertiary strata he examined in his last visit to that district as being grouped round the foot of the hills. The beds are of marine, fresh, and brackish-water origin. He does not determine their exact geological horizon, but gives lists of the fossils he obtained. The paper concludes with an account of a mass of sanidine-trachyte, which the author believes to be of Tertiary age.—A paper on *Aulococeras Fr. V. Hauer*, by Dr. Edm. von Mosjsisores, is illustrated with four lithographic plates. This and the following paper "On the Tertiary Formation of the Vienna Basin," by Theodor Fuchs and Felix Karrer we recommend to the attention of our palæontologists. Fuchs' and Karrer's paper is most elaborate, and contains copious lists of fossils which, besides being interesting in themselves, are useful for purposes of comparison. The *Jahrbuch* concludes with "Studien aus dem Salinargebiete Siebenbürgens," by F. Posepny; this, however, is only the second part of the paper, the first part having been published so far back as 1867. These saliferous regions are described in considerable detail, and numerous chemical analyses are given. A map, and sections, &c., accompany the paper. We should mention that the *Jahrbuch* includes obituary notices of two former members of the Institute, the well-known Wilhelm Haidinger, and Urban Schloenbach, an enthusiastic palæontologist and geologist who was cut off at the early age of thirty-one.

The three numbers of the *Quarterly Journal of Microscopical Science* of the present year contain a number of valuable original contributions to science, besides transactions, chronicles of the progress of histology and micro-zoology, and various reviews and short notes and memoranda. In the January number Prof. Allman describes a new mode of reproduction by fission in a new hydroid polyp, which he figures in a plate.—Haeckel's researches on the nature of Coccoliths and Bathybius are noticed at length, and the remarkable Radiolarian *Myxobrachia* is figured in a tinted plate.—Mr. Archer, of Dublin, to whose researches published in the same journal in 1869 we owe our knowledge of a most beautiful and interesting group of fresh water Protista—the Heliozoa—contributes to the April number a further account of new fresh water rhizopods, illustrated with two coloured plates.—In the same number Mr. Moseley figures and describes the nerves of the cornea, and Mr. Lankester gives a minute account of the structure and mode of formation of the sperm-ropes of the river Annelids.—In the July number an exceedingly valuable memoir by Dr. Van Beneden appears "On the Development of a Species of Gregarina," which he described last year (also in the Journal). It appears that the Gregarinæ exhibit a young stage when they are devoid of nucleus, and have great activity and worm-like form; to this stage Dr. Van Beneden applies the name *pseudo-filarian*.—In the same number Mr. Sorby gives an elaborate paper on the colouring matters of leaves, which has an appropriate place in a journal devoted to microscopy, since it is only by the micro-spectroscope that many of those colouring matters can be studied on account of their small quantity, and, further, since the application of such methods of analysis to histology as the micro-spectroscope affords is of the very highest importance.—Various points relating to the instrument itself are discussed in these three parts by Dr. Royston Pigott, who figures his aplanatic searcher and its results on the Podura scale; by Messrs. Dudgeon, Newton, aud others, who describe new apparatus.—Mr. Moseley gives accounts of how to use gold chloride and silver nitrate in histological research, and how best to prepare and cut sections of the frog's egg for embryological study.—The original paper by Dr. Nitzsche, of Leipzig (illustrated), on the reproduction of the Bryozoa, and the reply to Mr. Hincks, are important, and on a very curious point. It is, however, to the chronicles and notes which we would especially call attention as of service to biological students. Long abstracts of all the important papers published in the German periodicals are to be found—in some cases illustrated by woodcuts; thus we have Neuman on the origin of the red blood corpucles, Kranse on connective tissue, Flemming on fatty tissue, Schöbl on the bat's wing and mouse's ear, Pflüger on the method of demonstrating nerve-endings in the liver and other glands, Exner on the Schneiderian membrane, Cienkowski on the sporogonia of *Noctiluca*, and many other such.

In the *Journal of Botany* for October, Dr. Braithwaite continues his Recent Additions to our Moss Flora. Mr. R. Tucker gives some Notes on the now well-defined Flora of the Isle of Wight; and Dr. Moore Notes on some Irish Plants. Mr. F. Stratton contributes an article on *Monotropa hypopitys*, confirming the statement of other recent observers that this plant is not truly parasitic. The remainder of the number is occupied by short notes, reviews, reports, and reprints.

The *Scottish Naturalist* for October opens with a timely reprint of an extract from Mr. Patrick Matthew's work on Naval Timber, published in 1831, and referred to in Darwin's "Origin of Species," in which he distinctly enunciates the theory that "circumstance and species have grown up together," or that new species have arisen from old species adapting themselves to altered circumstances. The most important original articles in the number are: The Baleens, or Whalebone Whales of the North-east of Scotland, by Mr. R. Walker; Notes on the Tetraonidæ of Perthshire, by Mr. R. Paton; On the Altitudes attained by Certain Plants (varying from those already recorded), by Dr. F. Buchanan White; and On Scottish Galls, by Mr. J. W. H. Traill.

## SOCIETIES AND ACADEMIES

### Paris

Academy of Sciences, October 2.—M. C. Jorden read a mathematical paper "On the Classification of Primary Groups." Two papers on subjects connected with physics were read, one by M. A. Cornu, "On the Determination of the Velocity of Light," in which he suggests an improvement in the method proposed by Fizeau for this purpose, and a note by M. G. Salet on the Spectra of Tin and its components, which he describes as the most singular he has ever seen.—On astronomical subjects several communications were made.—M. Chasles replied to a statement made by M. Bertrand at a previous meeting with regard to Aboul Wéfa's method of ca'culating the position of the moon. M. Yvon Villareau communicated a long paper, full of mathematical formulæ, "On the Determination of the true Figure of the Earth, without the necessity of actual levellings."—M. Delaunay read a note on the two recently discovered planets, Nos. 116 and 117, in which he indicated that the planet discovered at Versailles by M. Borelly, and named Lomia, must be numbered 117, as the planet discovered by M. Luther two days afterwards had been previously detected in America by Mr. C. H. F. Peters.—

Letters on these planets by MM. Luther and Peters were also communicated by M. Leverrier, and M. Delaunay presented a determination of the orbit of Lomia by M. Tisserand.—The same gentleman a note on the nebulæ discovered by M. Stephan at Marseilles, and a note by M. Loewy on a new equatorial instrument. The latter is mounted like a transit instrument, but its body is bent at a right angle, and the images are carried to the eye of the observer by means of prisms or mirrors. The advantage, according to the author, is that the observer can carry on his investigations without changing his place, and that the necessity for an expensive revolving dome is done away with. —A fourth letter from Father Secchi, on the protuberances and other remarkable portions of the surface of the sun, was read. It contains a classification of the phenomena in question, and notices the chromosphere, protuberances, and clouds. Of the second several kinds are described.—M. de Fonvielle presented the programme of an intended balloon-ascent for the purpose of noticing the meteors of November 1871, and MM. Regnault and Elie de Beaumont made some remarks upon the same subject.—A letter was read from M. A. Poëy on the law of similar evolution of meteorological phenomena, in which he indicates the existence of a connection between the periodicity of meteorological phenomena and the diurnal and annual movements of the earth.—M. G. Lemoine presented a second part of his investigation of the reciprocal transformation of the two allotropic states of phosphorus, and M. Berthelot a second part of his researches upon ammoniacal salts. In the latter the author treats of the compounds of ammonia with boracic and carbonic acids.—A paper was read by M. C. Mène, giving numerous analyses of clays belonging to the carboniferous formation.—The tables of meteorological observations made at the Paris Observatory during the month of September was also communicated to the meeting.

October 9.—M. Bertrand presented a note by M. Painvin on the determination of the rays of a curve at any point of a surface defined by its tangential equation.—M. P. A. Favre read a continuation of his thermic investigations upon voltaic energy, in which he gives the results obtained by him in experiments with batteries containing fuming nitric acid, permanganic and sulphuric acids mixed, and hypochlorous acid. In connection with this subject, M. F. Le Blanc also presented a note on the energy of piles with two liquids. In a note on the most economical arrangements of voltaic batteries with regard to their polar electrodes, M. T. Du Moncel discusses the question of the desirability of reducing the size of the positive electrode.—M. Ruhmkorff described an arrangement for obtaining an exceedingly intense induced magneto-electric current. — Several astronomical papers were read, and among them a notice by M. Faye of the history and present state of the theory of comets, in which he contends for the existence of a repulsive force (*solar repulsion*) manifested in the phenomena of comets.—M. Delaunay announced that M. Stephan had observed Encke's comet at Marseilles on the night of the 8-9th October. In searching for this comet M. Stephan had discovered some new nebulæ.—M. Bertrand presented a reply to the remarks made by M. Chasles at the last meeting of the Academy on the determination of the position of the moon by Aboul Wéfa, and MM. Leverrier and Chasles remarked upon the desirability of searching the Oriental libraries for the astronomical writings of that author. — M. Delaunay communicated a note by M. Tisserand containing the determination of the orbit of the planet No. 116 (discovered by Mr. C. H. F. Peters) —M. Laugier presented a paper by M. Pagel, containing observations of the determination of the magnetic needle made at the Observatory of Toulon since the year 1866.—M. Roux presented an investigation of the artesian water of Rochefort, which comes up from a depth of nearly 857 metres. He gave a detailed analysis of the mineral contents of this water, and noticed the temperatures observed at various depths during the boring, which were considerably in excess of those recorded at Grenelle.—M. Billebault forwarded a note on the employment of gas-tar in the treatment of diseases of the vine, and especially against *Phylloxera vastatrix*. The destruction of this insect was also the subject of notes by MM. Peyrat and Deleuze.—M. E. Duclaux presented a note on a means of causing at will the hatching of silkworm eggs, which consists in exposing the eggs for a certain time to the action of cold.—In a note on the time which elapses between the excitation of the electric nerve of the torpedo and the discharge of its apparatus, M. Marey described some experiments made by him, from which it would appear that the nervous action is transmitted rather more slowly in the electric nerve than in the motor nerve of a muscle.—M. H. Sainte-Claire Deville communicated a note by M. A. Sanson on the theory of the early completion of the bones, in which the author replied to an objection to his theory made by a German writer.

PHILADELPHIA

Academy of Natural Sciences, February 6.—The President, Dr. Ruschenberger, in the chair. Prof. Leidy stated that he had recently received a small collection of fossils for examination from Prof. J. D. Whitney, who obtained them from California. The specimens are as follows:—A fragment of an inferior molar, apparently of *Mastodon americanus*. Of this specimen Prof. Whitney remarks that it was obtained from a depth of 80 feet beneath the basaltic lava of Table Mountain, Tuolumne County, Cal., where it was found in association with remains of human art. A much worn lower molar of a large horse, probably the *Equus pacificus*, from 16 feet on Gorden Gulch. The triturating surface of the crown measures 13½ lines fore and aft, and 10 lines transversely, inclusive of the cementum. Two equine molar teeth, which, according to the accompanying label, were obtained 350 feet below the surface, at Soulsbyville, Tuolumne County, Cal. One is an unworn upper back molar, apparently of a species of *Protohippus*. It is moderately curved from behind forward and downward, but only slightly from within outward. It is 21 lines long in a straight line. Its greatest breadth above the middle, fore and aft, is nearly 9 lines; its thickness about 7 lines. The other tooth is a lower molar, about one-third worn, probably of the same species. The triturating surface is 10 lines fore and aft, and nearly 7 transversely. Two teeth labelled "Found ten feet below the surface at Dry Creek, near Bear Creek, Mercer County, Cal." One of the specimens appears to be the portion of a canine tooth, and the other is an incisor. They resemble in form the corresponding teeth of the lama, and probably belong to a species of the same genus. The incisor is about 1½ inch in length; the crown externally is 11 lines long and 4½ lines wide.

March 7.—The President, Dr. Ruschenberger, in the chair. Mr. Thomas Meehan referred to some observations he made before the Academy last autumn in regard to a peculiar storing up of turpentine in the common insect, *Reduvius novenarius*. Since then entomologists had been investigating the use for which this turpentine was employed, without success. He was now able to report that it was for the purpose of fastening its eggs on the branches of trees, and for sticking them together; also, in probability, as a means of protection against enemies and the weather. The eggs of the *Reduvius* were inserted in groups, and each set upright one against another with the turpentine, like the cell in a honeycomb. It had hitherto been supposed by entomologists that the matter used for this purpose was a secretion of the insect itself; but so far as he could judge by the senses, the matter used was merely turpentine, and no doubt the turpentine he had observed the insect storing up in the fall.—Mr. Meehan exhibited some flowers of the common *Bouvardia leiantha* of the greenhouses, and of the hardy *Deutzia gracilis*, and referred to his papers, published a few years ago in the Proceedings of the Academy, on practical diœcism in the trailing Arbutus (*Epigæa repens*) and *Mitchella repens*, in which he pointed out that these plants, though apparently hermaphrodite, had the stamens and pistils of different characters in separate plants, and were, therefore, subject to the laws of cross-fertilisation as indicated by Darwin. He had had his attention called to the *Bouvardia* by Mr. Tatnall, of Wilmington, Del., as furnishing a similar instance to that of *Epigæa* and *Mitchella*, to the same natural order as which, the *Cinchoneous* division of *Rubiaceæ*, the *Bouvardia* belonged. These had some plants with the pistils exserted, while in others only the stamens were visible at the mouth of the corolla tube. Mr. Tatnall had not had the matter suggested to him early enough to say that it was so in all cases; but he believed that these flowers, which practically might be termed pistillate and staminate, were found entirely on separate plants. This is a very important fact, as the *Bouvardia* is not raised from seeds in greenhouses, but from cuttings of the roots, and, therefore, all these plants with separate sexes must have been produced from one original individual, without the *intervention of seed*, and thus confirm the position advanced in a previous paper of the speaker on "Bud Variations," namely, that variations in form, and, by logical inference, new species, may arise without seminal intervention. In the specimens of *Deutzia gracilis* were two forms of flowers on the same plant. Besides the large ones with stamens and pistils apparently perfect, there were numerous small flowers in which the petals were only partially developed. The filaments were entirely wanting, but the anthers were as perfect, if not larger than in what we should call the perfect flowers. Any one could see that these small flowers were the result of deficient nutriment, and would be apt to pass the matter over with this simple reflection; but he wished to emphasise the fact that this defective nutrition rendered the female organs inoperative, while the male organs were still able to exercise their functions; thus affording another instance, if any more be needed, of the truth of his theory of sex, namely, that with defective nutrition, the female sex is the first to disappear, and that only under the highest conditions of vitality is the female sex formed. In the case of the *Bouvardia* a similar law was seen. The most vigorous stems, or, as they would technically be called, woody axes, produced the female flowers.—Prof. Cope made some observations on a Batrachian of the coal measures, *Sauropleura remex*, Cope. A specimen more perfect than the type recently obtained by Prof. Newberry exhibited posterior limbs such as has been ascribed to the *S. pectinata*. The vertebræ posterior to this point were perfectly preserved, and supported the remarkable processes to the end.

March 21.—Dr. Carson, vice-president, in the chair.—Prof. Leidy made the following remarks on *Tænia mediocanellata*. Recently, one of our ablest and most respected practitioners of medicine submitted to my examination a tapeworm which had been discharged from a young man, after the use of the *Aspidium filix-mas*. The physician, in giving an account of the case, stated that he had previously treated the patient for another affection, in which raw-beef sandwiches had been prescribed for food. After looking at the worm, I remarked that it appeared to be the *Tænia mediocanellata*, a species which I had not before seen, and added that the patient had probably become infected from a larva swallowed with the raw-beef sandwiches. The specimen consisted of the greater part of the worm, broken into several pieces. Including some lost portions, it was estimated to have been upwards of thirty feet in length. Unfortunately, the head proved to be absent; but, so far as characters could be obtained from the specimen, in the form of the segments, position of the genital orifices, and the condition of the ovaries, it agreed with the description given of *T. mediocanellata*, rather than with *T. solium*. From a want of acquaintance with the former, I did not feel entirely satisfied that the specimen actually belonged to that species. Subsequently, my friend brought to me the anterior part of the body, probably, of the same individual tapeworm. He observed that his patient continuing to complain, he had administered another dose of the male-fern, which was followed by the expulsion of the portion of the worm now presented. The head of the parasite was included, and it confirmed the view that it pertained to the *Tænia mediocanellata*. The case serves as another caution against the use of raw flesh as food. The description of the worm, as derived from the specimen, is as follows:—The head is white, without pigment-granules, obtusely rounded, unarmed with hooks, and unprovided with a rostellum, but furnished with a minute acetabuliform fovea at the summit. The four acetabula are spherical, and opaque white. The diameter of the head is three-fourths of a line. The neck, or unsegmented portion of the body immediately succeeding the head, is about four lines long by half a line in breadth. The most anterior indistinctly defined segments of the body, and those immediately succeeding them, but more distinctly separated, are about one fifth of a line long by two-fifths of a line broad. In a more posterior fragment of the body, the flat and nearly square segments measure half a line long and one line broad, to one-third line long and two-and-a-half lines broad. A succeeding fragment exhibits segments three-and-a-half lines long by four lines broad, and two lines long by five lines broad. Many of the segments in this piece are irregularly separated laterally by deep, wide notches. In a succeeding long portion of the worm, the segments are wider behind than in front, and measure two, five, and three lines long by five lines broad. In a long piece of the posterior part of the worm, the segments are first four lines long and broad; and in the last four feet of the same piece, the segments are clavate in outline, from six to ten lines long, and two and three lines broad. The genital apertures are conspicuous, and are situated behind the middle of the segments. They alternate irregularly. Thus, in the last two feet of the posterior fragment of the worm, the first two segments exhibit the aperture on the left margin; the succeeding segment presents the anomaly of an aperture on both margins; then follow three apertures on the right, next two on the left, then four on the right, then eight alternating in pairs, then one on the left, and so on. The ovaries are opaque white, and exhibit numerous closely crowded lateral branches. In the absence of pigment-granules to the head, and in the less robust character of the worm, the specimen differs from *T. mediocanellata* as described by Küchenmeister. The minute acetabular pit or fovea at the summit of the head is not mentioned by Küchenmeister and subsequent observers as a character of that species. It is a point, however, that might be readily overlooked, especially if the parts of the head are obscured by the presence of pigment-granules.—Prof. Cope exhibited a number of fishes from the Amazon above the mouth of the Rio Negro, which included some new and rare forms. Some of the latter were *Doras brachiatus*, *Plecostomus scopularius*, *Roeboides rubrivertex*, *Myletes albiscopus*, &c. He exhibited a specimen of *Pariodon microps*, Kner, describing the parasitic habits of *Stegophilus* and those ascribed to *Vandellia*. He thought the structure and colouration of the *Pariodon* indicated similar habits, and that it would be found to be an inhabitant, at times at least, of the cavity of the body of some other animal.

## BOOKS RECEIVED

English.—Contributions to the Flora of Mentone, Part 4: J. T Moggridge (L. Reeve and Co.).—Words from a Layman's Ministry at Barnard Castle.—Transactions and Proceedings of the Royal Society of Victoria, Vol. viii., Parts 1, 2; Vol. ix., Parts 1, 2.

Foreign.—Nachtrag zum 6 u. 7 Jahresbericht des Vereins für Erdkunde zu Dresden. (Through Williams and Norgate.)—Die feierliche Sitzung der kaiserlichen Akademie der Wissenschaften zu Wien, 30 Mai, 1871—Almanach der k Akademie der Wissenschaften zu Wien.—Oefversigt af k. Vetenskaps Akademiens Förhandlingar.

## PAMPHLETS RECEIVED

English.—Darwinism: Chauncey Wright.—The Cruise of the *Norna*: Marshal Hall.—The University of Durham College of Medicine, Syllabus for 1871-72—The College of Physical Science, Newcastle-on-Tyne, Syllabus for 1871-72.—Observations on the Corona: Hercules Ellis.—Flint: M. H. Johnson.—The Scottish Naturalist October—Proceedings of the Meteorological Society, No. 56.—The Portfolio, No 22.—Quarterly Weather Report of the Meteorological Office.—Journal of the Statistical Society for September.—On the Faults in Ironstone Seams: R. L. Jack.—The Phœnix, Vol. ii, No. 14.—Journal of the Iron and Steel Institute, Vol ii., No. 3—Journal of the Scottish Meteorological Society, No. 31.—The Quarterly Journal for Microscopical Science, October.

American and Colonial.—On the Influence of the Blue Colour of the Sky in developing Animal and Vegetable Life: Philadelphia.—On the Eozöonal Limestones of Eastern Massachusetts: L. S. Burbank.—On the Characteristics of the Primary Groups of the Class of Mammals: Dr. Th. Gill.—The Canadian Naturalist, Vol v., No. 4; Vol. vi., No. 1—Proceedings of the American Philosophical Society, Philadelphia, Jan.–June.—Extracts from the Proceedings of the Lyceum of Natural History, New York.—Proceedings of the Asiatic Society of Bengal, No. 7.—The Canadian Entomologist.—The Rural New Yorker, Vol. xxi., Nos. 21-24.

Foreign.—Jahrbuch der k. k. geologischen Reichsanstalt zu Wien, 1871, April–June.—Georg Gottfried Gervinus: Emil Lehmann.—Magazine d'Education et de Recréation, No. 162.—Sur la loi de l'Evolution similaire des Phénomènes Météorologiques: M. A. Poëy.

## CONTENTS

THURSDAY, OCTOBER 26, 1871

## SCIENCE IN AMERICA

*Proceedings of the Essex Institute.* Vols. I.-V. *Proceedings and Communications of the Essex Institute.* Vol. VI. *Bulletin of the Essex Institute.* Vols. I. and II. (Salem, Massachusetts. Published by the Essex Institute, 1856-1871.)

WE have, on various occasions, alluded to the large amount of encouragement to the pursuit of science afforded by the governing powers of the United States, both by the Central Federal Government at Washington, and by those of the individual States. The sums of money voted for such purposes by our American relations would make the hair of our economical Government officials in this country stand on end, and would be certain to provoke angry comment in our House of Commons; while the number of scientific men paid for carrying on investigations and preparing reports on various subjects of great practical value for the welfare of the country, would almost bear comparison with the number we pay for doing nothing or for obstructing all rational improvements.

When men of culture and science in this country attempt to advocate the claims of Science to national support from the Government, one of the arguments most relied on by their opponents is that such a course would have the effect of checking all scientific enterprise and research that was not paid for by the State. We should like these objectors to look over the publications now lying before us; and we think, if they were able to derive any lesson from it, it might have a tendency to modify their opinion.

New England is acknowledged to be the most highly educated portion of the United States, and among the New England States none occupies a more honourable position than Massachusetts for its high standard of cultivation, and for the public-spirited manner in which its citizens tax themselves for the support of education and the spread of knowledge, scientific and otherwise. The early New England settlers had a loving habit of perpetuating in their new settlements the names of familiar places in the old country, and thus we find one of its counties called Essex, with an area about equal to that of our Middlesex, possessing a scientific institute located in the thriving town of Salem. A sketch of the history of the Essex Institute since its foundation may convey some idea of the manner in which voluntary scientific effort is carried on in Massachusetts.

The Essex Institute was formed in 1847 by the union of the Essex Historical and the Essex County Natural History Societies; and the Institute, thus organised, consisted of three departments—the historical, having for its object the collection and preservation of whatever relates to the topography, antiquities, and civil and ecclesiastical history of Essex County; the natural history, for the formation of a cabinet of natural productions in general, and more particularly of those in the county, and for a library of standard works on the natural sciences; and the horticultural, for promoting a taste for the cultivation of choice fruits and flowers, and for collecting works on horticulture and agriculture in connection with the general library.

From 1848 to 1866 the Essex Institute published five volumes of "Proceedings," containing reports of papers read before the Institute on the various subjects included in the programme. Among the more important papers contained in these volumes may be mentioned the following:—List of Infusoria found chiefly in the neighbourhood of Salem, by Thomas Cole (1853); Catalogue of the Birds of Essex County, by F. W. Putnam, 235 species (1856); Account of the life, character, &c., of the Rev. Samuel Parris, of Salem Village, and of his connection with the Witchcraft delusion of 1692, by Samuel P. Fowler (1856), reminding one exceedingly of a history that will probably one day be written of certain similar delusions not unknown in our own day; On Indian Relics from Marblehead (1857); Noticeable traits of the Flora of Nahant, by C. M. Tracy (1858); On the changes produced by Civilisation in the habits of our common Birds, by S. D. Fowler (1860); Catalogue of the Birds found at Norway, Oxford County, Maine, by A. P. Verrill (1862); Report of the Army Worm (*Leucania unipuncta*, Ham.), by Carleton A. Shurtleff (1862); Catalogue of Birds found at Springfield, Mass., with Notes on their migrations, habits, &c., by J. A. Allen (1864); The Humble Bees of New England and their Parasites, by A. S. Packard, jun. (1864); A classification of Mollusca, based on the principle of Cephalisation, by Edward S. Morse (1865); Synopsis of the Polyps and Corals of the North Pacific Exploring Expedition under Commodore C. Ringgold and Captain John Rodgers, U.S.N., from 1853 to 1856, by A. P. Verrill (1865-66); Flora of the Hawaian Islands, by Horace Mann (1866).

From 1867 the Transactions of the Institute have been published in a slightly different form, under the title of "Proceedings and Communications of the Essex Institute," its contents consisting to a considerable extent of continuations of some of the elaborate and important papers commenced in the earlier volumes, especially of Prof. Verrill's "Synopsis of the Corals and Polyps of the North Pacific Exploring Expedition," and of the "Flora Hawaii," by Mr. Horace Mann, whose early death was an irreparable loss to American botanists. There are also a number of papers by Mr. A. S. Packard, whose services to embryology are so well known, and the very valuable contribution by Dr. Elliott Coues, "Catalogue of Birds of North America contained in the Museum of the Essex Institute; with which is incorporated a list of the Birds of New England, with brief critical and field notes." The following quotation from this paper will interest ornithological readers:—"Within the area of New England are represented portions of two faunæ, the Canadian and Alleghanian, which differ in many respects from each other. There seems to be a natural dividing line between the birds of Massachusetts and Southern New England generally, and those of the more northern portions of the Eastern States. Numerous species which enter New England in spring, to breed there, do not proceed, as a general rule, farther north than Massachusetts, and many others, properly to be regarded as stragglers from the south in summer and early autumn, are rarely, if ever, found beyond the latitude of this State. In like manner, many of the regular winter visitants of Maine are of rare and only occasional occurrence, and are not found at all much farther south. Again, many species hardly known in Massachusetts and southward, except as migratory species passing through in spring and autumn, are in Maine regular summer visitants, breeding abundantly. Other minor differences, resulting from latitude and physical geography, will readily be brought to mind by attentive consideration of the subject, and therefore need not be here detailed. It will be evident that a due regard for these important points has necessitated, in the case of almost every species in the list, remarks elucidative of the special part it plays in the composition of the avifauna."

The later numbers, especially of the Proceedings, are illustrated by admirably executed lithographs illustrative of the natural history pages, and a considerable amount of space is occupied by reports of the Field meetings of the members. It is interesting to read that the idea of these excursions, which have been productive of such valuable practical result, originated from a perusal of the Transactions of the Berwickshire Naturalists' Club.

While thus affording a medium for the publication of papers of sterling scientific value, the Essex Institute has not been unmindful of the no less imperative duty of scientific bodies, that of promoting a taste for science among the educated but unscientific public. We in this country have perhaps erred in too much ignoring the *profanum vulgus*. It becomes, however, yearly more and more manifest that science must be no esoteric religion, but that it must grasp, in its all-including embrace every section of the community. It is doubtful, indeed, which class of scientific men deserves best of the republic, those who devote the whole of their time to actual work in the laboratory or the dissecting-room; or those who of the riches of their knowledge impart to the ignorant crowd in the lecture-room or by the popular treatise. With the names of the former will doubtless be connected the most important discoveries of the age; the latter will have the satisfaction of knowing that they have done their part towards making science really popular, towards spreading its blessings among the masses. The danger is when the instruction of the public is undertaken by those who have not practically made themselves masters of the mysteries which they presume to communicate to others.

Commencing with January 1869, the issue was commenced, in addition to the publications named above, of the "Bulletin of the Essex Institute," the object being to give to the public such portions of communications made to the Institute as are of popular interest. We find here, in language intelligible to non-scientific readers, reports of such proceedings and papers read at meetings of the Institute, in Natural History, Philology, and History, as are likely to interest the inhabitants of the county generally; and we look upon this as not the least valuable of its publications.

An interesting publication in connection with the Essex Institute is "The Naturalists' Directory," which is intended, when complete, to form a list of the addresses of the workers in each department of science all over the world. If ever the proposed union of our scientific societies is effected, we may get something of the kind in this country.

The above account of the Proceedings of the Essex Institute since its foundation may serve to show the zeal displayed by workers in Natural History in the United States, and may also be useful in pointing out some hitherto unrecognised fields of usefulness to similar bodies at home.

## OUR BOOK SHELF

*Contributions to the Flora of Mentone, and to a Winter Flora of the Riviera, including the Coast from Marseilles to Genoa.* By J. Traherne Moggridge, F.L.S. 100 coloured plates. (London: L. Reeve and Co., 1871.)

MR. MOGGRIDGE has collected in this splendid volume drawings and descriptions of one hundred of the most striking plants of the Mediterranean coast of France. We have no preface to inform us on what principles the selection has been made, nor are they self-evident. But few of the species are new, though some of them are doubtful plants of which precise characters were much wanted. Mr. Moggridge is well known to English botanists as an accurate and careful observer, who has paid great attention to the botany of this district; and he has here produced a volume which is not only a useful contribution to science, but is surpassed by few that we know as a *livre de luxe* to lie on the drawing-room table. The illustrations are beautifully drawn by the author himself, and are exquisitely coloured. Mr. Moggridge has made himself thoroughly acquainted with the beautiful but difficult species or varieties of Orchis of the south of France related to our Bee-orchis. It is remarkable that, while on our chalk-hills the bee and fly orchis, *Ophrys apifera* and *muscifera*, remain perfectly distinct, in the south of Europe they approximate to one another by innumerable intermediate forms, which may all be considered as varieties of Linnæus's *O. insectifera*. These are here worked out with great care, and we have plates of a number of the most interesting forms. A. W. B.

*Zeitschrift der österreichischen Gesellschaft für Meteorologie.* Redigirt von Dr. C. Jelinek und Dr. J. Hann, v. Band, mit 3 lithographirten Tafeln, pp. 644; vi. Band, pp. 1—224 (Wien, 1870-71.)

THE fifth volume of the Journal of the Austrian Meteorological Society, published fortnightly, and extending in one year to 644 octavo pages, shows at once the extraordinary energy with which this society conducts its operations, and the high estimation in which meteorology is held in Austria. What strikes one as the most remarkable feature of this periodical is the broad and catholic spirit in which the science is treated. Whilst the articles are mostly written by members of the society, the pages of the journal are open to meteorologists in all parts of the globe. Reprints or abstracts, accompanied where necessary with tabular matter, of the more important meteorological papers which have been published in other journals, appear from time to time. A few of the more important of these are the following, viz:—Dove's "Non-periodic Changes of the Distribution of Temperature over the Earth's Surface," D. Milne Home's "Increasing the Supply of Spring Water at Malta, and Improving the Climate of the Island," Glaisher's "Temperature of the Air at Different Heights," Buchan's "Mean Pressure and Prevailing Winds over the Globe," Wojeikof's "Mean Temperature of Russia," Raulin's "Rainfall of Algiers," Rayet's "Climates of the Isthmus of Suez," Jelinek's "Distribution of Thunderstorms in Austria," Petermann's "Monogram on the Gulf-Stream," Mohn's "Temperature of the Sea," and Angus Smith's "Composition of the Atmosphere." The abstracts are not bald productions, but ably written and readable articles. Another admirable feature is the papers on the climates of places in different parts of the globe, by Dr. Hann, one of the editors and unquestionably one of the greatest of the younger meteorologists on the Continent. These papers are accompanied by tables giving the mean pressure, temperature, humidity, rainfall, wind, and cloud; and their very great value will be recognised when it is stated that they embrace places whose meteorology was little, if at all known, such as Rio Janeiro, Parana, Mendoza, Monte Video, Buenos-Ayres, Punta-Arenas, Puerto Montt, Santiago, Valdivia, Valparaiso, Serena, Copiapo, and Lima, in South America; Bagdad and Samaua in Mesopotamia; Kuldscha in West China; St. Anna, near Manila, Philippine Islands; and Said, Ismailia, and Suez. Since broad and just views of the atmosphere and its movements can be attained only through the accumulation of such facts and an intelligent discussion of them, our best thanks are due to the Austrian meteorologists for these invaluable contributions. If meteorology were prosecuted more in this spirit than, unhappily, has been the case, it would be marred by fewer crude and hastily-formed theories; and particularly inquiries into local climates and weather over limited portions of the earth's surface would be conducted on sounder principles, and be productive of results which could be accepted as solid contributions to science. We heartily recommend this journal, especially since in this country we have nothing to compare with it,—no periodical which so well puts meteorologists and physicists *au courant* with this rapidly-advancing science.

*Das Leben der Erde: Blicke in ihre Geschichte, nebst Darstellung der wichtigsten und interessantesten Fragen ihres Natur- und Kultur-lebens.* Ein Volksbuch von A. Hummel. (Leipzig: F. Fleischer; London: Williams and Norgate, 1870.)

It is always a question of doubtful expediency whether it is wise to compress into one work by one writer a complete history of Nature, even in a popular treatise. This has been attempted by Herr Hummel in this volume of 424 pages, and, as far as such an attempt can succeed, not unsuccessfully. We have first a glimpse of the origin of the earth, and of its relations to the solar system. Then follows a chapter on the physical geography of the land, describing the main physical features of the solid crust of the globe. Next we have a treatise on water, and the part it has played in the formation of the existing surface of the earth. To this succeeds a chapter on the atmosphere and its phenomena. In conclusion we have a general sketch of the vegetation of the earth, and of the forms of animal life, in which the author declares against the Darwinian theory of the origin of species. Written occasionally in the inflated language in which continental popular writers too much indulge, the work is, nevertheless, a good one to put in the hands of young people with the double purpose of giving them some knowledge of natural science and of German. It was published on the hundredth birthday of Alexander von Humboldt, as a tribute to the memory of the great naturalist.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Sun's Parallax

Is there nobody who will perform an act of justice, and ask those who seem to have never known or to have forgotten my doings, to be kind enough not to deprive me of my just claims? When, A.D. 1857, my old method of determining the sun's parallax was again publicly proposed, I thought it somewhat strange, and wondered what could be the reason that it should be treated as if it were some new and not a very old acquaintance of Science. When, some time later, a stir was made about what was represented as a new method of investigating the motion of the solar system in space, and, instead of a new, there was brought forward an old acquaintance (known to Science since the times of your grandfathers), only dressed anew, and engaged to perform some truly "astounding" antics, I wondered indeed that no friendly hand should have prevented such an exhibition, but I also comprehended the true state of affairs. And since then I have had to shrug my ghostly shoulders so often when learning further news about your curious knowledge of Science, and your strange opinions, and your queer notions of honour, and justice, and fairness, that I have long ceased to wonder at anything some of you may say or do. However, as it is only right that I should be allowed to retain what belongs to me, and as nobody appears to remember my claims, you will probably raise no objection, if I, myself, enlighten you a little, and remind you how, A.D. 1672, I determined the sun's parallax.

Read in the History of my Life (Baily's Account, &c. p. 32):—

"Whilst I was inquiring for the planets' appulses to the fixed stars by the help of Hecker's ephemerides, I found that, in September 1672, the planet Mars, then newly past his perihelion and opposition to the sun, would pass amongst three contiguous fixed stars in the water of *Aquarius*; and that by reason he was then very near the earth, this would be the most convenient opportunity that would be afforded of many years for determining his, and consequently the sun's, horizontal parallax. I drew up a *monitum* of this appearance, and sent it with a letter to Mr. Oldenburg, who printed it in his *Transactions*, No. 86, August 19th, 1672, having before sent my admonition into France, where the gentlemen of their Academy took care to have it observed in several places. My father's affairs caused me to take a journey into Lancashire the very day I had designed to begin my observations, but God's Providence so ordered it that they gave me an opportunity to visit Townley, where I was kindly received and entertained by Mr. Townley, with whose instruments I saw Mars near the middlemost of the three adjacent fixed stars. My stay in Lancashire was short. At my return from thence I took his distances from two of them at distant times of the night. Whence I determined his parallax then 25″, equal to his visible diameter; which, therefore, must be its constant measure, and, consequently, the sun's horizontal parallax not more than 10″. This I gave notice of in the *Transactions*, No. 96; and the French soon after declared that from their observations they had found the same. Whether they will give you such exactness I leave to those who are skilled in these things to determine."

This extract is, I hope, sufficient, and I will leave it to you to search further. Perhaps you may consider my language a little quaint, but then, remember, I lived two centuries ago.

Now, the planet Mars performs 109 sidereal revolutions in 205 sidereal years and $3\frac{1}{4}$ days, so that its appearance in the year 1877 will not be very different from what it was in 1672. Accordingly I enjoin you to make then the most of your opportunity, and do your best to prove the goodness of my old method, and I wish you thorough success. And when you watch the planet pass amongst the stars in the water of Aquarius, you will, perhaps, remember with kindly feelings an old astronomer, who in life had to endure great injustice and sore trials, and will bless and honour his memory.

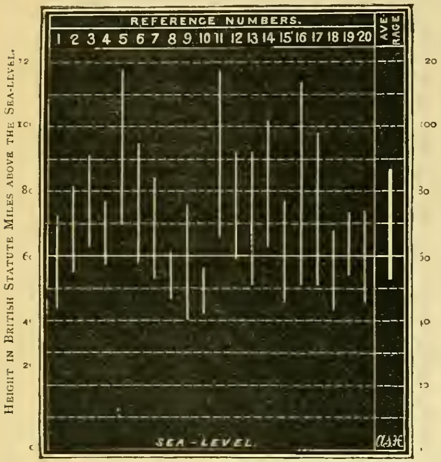
The Ghost of John Flamsteed, M.R.

Walhalla

### The Marseilles Meteorite

It will probably occur to most of your readers, as it immediately suggested itself to me, on reading in your journal of the 5th inst. a description from *Les Mondes* of a remarkable meteorite observed at Marseilles by M. Coggia, on the 1st of August last, that the bright object having an apparent diameter, at first of about 15′, and at last of a little over 4′, whose uncertain course was noted for eighteen minutes by the stars, was really nothing more extraordinary than a fire-balloon; or it may, possibly, have been some description of brighter signal-light. The planet Saturn, and the other stars named in the description, were all at the low altitude above the horizon, at which a fire-balloon, and other bright signal-lights of ordinary size, floating at an ordinary height in the air, would have about the apparent diameter of the "meteorite." Its apparent diminution in size was, also, perhaps, either the effect of its increasing distance, or of its gradually fading light. After alternately remaining stationary, and changing its apparent course two or three times, it at last fell rapidly in a perpendicular direction. The burning tow, or other inflamed substance with which it was inflated, appears to have detached itself from, or, it may be, to have set fire to the balloon, since it was remarked that during its perpendicular fall to the horizon it gave out vivid scintillations.

It is difficult, from the exaggerated language of native narratives in the East, to suppose that the destruction of life and property described, from the *Times of India*, as an unprecedented catastrophe in Sind, in the next paragraph of NATURE, was occasioned by an unusual fall of meteorites. In the absence of




HEIGHTS OF TWENTY SHOOTING-STARS DOUBLY OBSERVED AT EIGHT BRITISH ASSOCIATION STATIONS IN ENGLAND ON THE NIGHTS OF THE 9TH TO 12TH OF AUGUST, 1871.

any evidence that a loud report, and other aërolitic phenomena perceived at a great distance, accompanied the occurrence, its unusually disastrous effects may rather, doubtless, be ascribed to devastations produced by lightning of extraordinary violence.

On the accompanying diagram the real heights of some shooting stars are represented which were simultaneously recorded by observers of the annual meteor-shower in August last, at eight British Association stations in England. A. S. HERSCHEL

Newcastle College of Physical Science, Oct. 16

## Exogenous Structure in Coal-Plants

PROF. WILLIAMSON criticises my want of certainty with respect to the exogenous mode of growth of extinct Lycopodiaceæ. But surely his reference to the Dixonfold trees does not prove more than that the diameter of their stems was greater near the roots than higher up. The same thing is true of many palms, but I think Prof. Williamson would be the last person to say that it was evidence of *their* being exogenous. Nevertheless, as I have already said, I am inclined to think that Prof. Williamson is right in supposing that the stems of extinct arborescent Lycopodiaceæ increased in thickness, although I do not see my way to asserting off hand that this was the case. Even admitting, with all Prof. Williamson's confidence, that it was so, I can see no classificatory value in the fact to justify overriding reproductive characters in his new classification.

I said in my former letter (and the argument still appears to me a good one) that this increase was in any case "nothing more than an adjustment to an arborescent habit dropped when the arborescent habit was lost." Prof. Williamson finds some difficulty in understanding this, and believes me to imply "that these exogenous conditions were merely adventitious growths assumed for a season and thrown off at the earliest opportunity; that they had no true affinity with the plants in which they were found." He confesses that he sees no ground for so remarkable a conclusion, and I may certainly say that as far as I comprehend it, neither do I.

What I did mean to imply was, that in comparing the stems of existing with those of extinct Lycopodiaceæ, allowance must be made for such adaptations of structure as would be likely to be correlated with enormous size. To make the matter clearer by an illustration:—Suppose we compare a nearly allied woody and herbaceous plant, say a lupin and a laburnum, we shall find in their stems (both "exogens") the same kind of differences as exist between the stem of a herbaceous *Selaginella* and that of the nearly allied arborescent *Lepidodendron*. The lupin may have had arborescent ancestors; if so, it has dropped all such adaptations of the structure of its stem to an arborescent habit as we find existing in laburnum. Assuming (what is of course *only* an assumption) that *Selaginella* is a descendant of *Lepidodendron* or its allies, the parsimony of nature has also suppressed in it all those peculiarities of stem structure which were merely correlated with vast size, and in *Selaginella* and recent *Lycopodiaceæ* we have the residuum. In *Isoetes*, which is only a few inches high, there is a kind of lingering reminiscence of circumferential growth.

Prof. Williamson says that "herbs if they belong to the exogenous group are as truly exogenous in their type as the most gigantic trees of the same class. Size has nothing to do with the matter." With these statements I altogether disagree. I look upon the terms exogen, endogen, and acrogen as altogether obsolete from a classificatory point of view. Mohl pointed this out more than twenty years ago. Compare the following remarks from one of his memoirs with Prof. Williamson's: "The course of the vascular bundles in the palm stem and in the one-year-old shoot of the dicotyledons is exactly similar, and the conception of a different mode of growth, and the division of plants into endogens and exogens formed on it is altogether opposed to nature."

Size, in fact, has everything to do with the matter. It is the persistent growth of the ends of the branches which makes the strengthening of the main stem by circumferential growth a mechanical necessity. Palms not being branched do not require the voluminous stem of an oak, and they exhibit on an enlarged scale only the structure of a one-year-old herbaceous shoot. But in the dragon-tree of Teneriffe an "endogen," which becomes extensively branched, there is a true circumferential growth of the main stem, which increases *pari passu* with the development of the branches. All herbaceous stems, on the contrary, among flowering plants, whether belonging to the exogenous or endogenous group, have practically the same type of structure. Where is the exogenous type in the stem of the common artichoke, or in *Ferula communis*, figured by De Candolle in his "Organographie Végétale," pl. 3, fig. 3, "pour montrer à quel point elle simule les tiges de monocotylèdones" (endogens)?

I think these remarks make it plain that circumferential (which is a preferable expression to exogenous) growth in stems is simply a necessary accompaniment of a branched arborescent habit. As far as the affinities of plants are concerned, it is purely accidental and of no classificatory value. *Lupinus* being herbaceous and *Laburnum* arborescent does not prevent their being placed in the same tribe of a natural family. Since Mohl has shown that one-year-old (herbaceous) stems conform to the endogenous type, while such woody stems as *Laburnum* possesses are of course exogenous, it is clear that Prof. Williamson's views would overthrow all the work of modern systematists, and bring us back, as I pointed out in my former letter, to the primitive division of plants into trees and herbs (not trees and *shrubs* as Prof. Williamson makes me say).

The interpretation of the actual structure of the stems of the extinct Lycopodiaceæ is of course another matter. Prof. Williamson illustrated his views at Edinburgh by referring to *Lepidodendron selaginoides*; every botanist who took part in the discussion, however, objected to his explanation. It may be true that this is only one form of such stems, but of course I can hardly be expected to be acquainted with the unpublished material which Prof. Williamson still has in hand. There is, I think myself, good reason for believing that *Lepidodendron*, *Sigillaria*, and *Ulodendron* all belong to a common type of stem structure; differences in fragments of different age of growth must be expected and allowed for. Of course, as I do not accept the existence of a pith in these plants, the pith or medullary rays must be rejected as well. Mr. Carruthers has shown, I think, conclusive reasons for disagreeing with Dr. Hooker with respect to the spaces which he identified with those structures. I was already familiar with the view of these stems taken by Prof. Williamson in his last paper. Those who are interested in the matter must judge for themselves who is right.

This communication has run on to so great a length that I am unable to touch upon other points in which I find myself totally disagreeing with Prof. Williamson. I cannot, however refrain from expressing my astonishment at the persistence of the histological views implied by the description of the "cambium," or growing cellular tissues of plants, as "some protoplasmic element," or again as "some protoplasmic layer." Similar expressions were used by Nehemiah Grew about 200 years ago, and employed for some time by writers subsequent to him. At the present I imagined their interest was wholly historical.

W. T. THISELTON DYER

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THE points at issue between Prof. Williamson and myself remain in the same position as at first. He has not yet answered one of my objections. He still holds that in Lepidodendron we have a vascular medulla, outside which is a series of fibro-vascular bundles which are not closed, but go on forming new tissues by means of a cambium layer like a dicotyledonous stem. From my own observations, and from the study of recent Continental authorities, I have no hesitation in stating that the central "medulla" of Prof. Williamson consists of the united closed fibro-vascular bundles, while the investing cylinder is the modified primitive tissue which increases in diameter by means of the *meristem* layer of Nägeli. If Prof. Williamson will refer to Sachs' Lehrbuch, Ed. 2, p. 397, he will find good reasons given for the statement there made, that *Isoëtes* contains *no cambium* in the stem; but that the stem increases in the same way as *Dracæna*, *i.e.* by a meristem layer in the primitive tissue. As long as Prof. Williamson believes in a central vascular medulla in these Lycopodiaceous stems, all his other conclusions must likewise be false.

W. R. M'NAB

Royal Agricultural College, Cirencester, Oct. 21

[*** We would suggest that this controversy be now closed, until the publication of Prof. Williamson's new material.—ED.]

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### Blood-Spectrum

IN the account of the Progress of Science in Italy in NATURE for October 12, Mr. W. Mattieu Williams says that Prof. C. Campani has shown that the spectrum of an ammoniacal solution of carmine is undistinguishable from that of blood, and that perhaps I should be able to tell whether any difference can be distinguished by more minute examination. In my first paper on this subject, so long ago as 1865*, I alluded to this similarity, and in subsequent papers † I have shown how the colouring matter of blood can be distinguished from that of cochineal, and even a small quantity recognised when mixed with a relatively considerable quantity of that dye. I have always argued that in such inquiries we must not rely on the spectrum, but compare the action of various reagents. On adding a little boric acid to an aqueous solution of blood, no change takes place in its spectrum, whereas that of cochineal is completely altered. This effect is not produced in the case of carmine suspended in water, but the absorption-bands of blood are at once removed by deoxidising the solution with a ferrous salt, which, on the contrary, has no effect in the case of carmine or cochineal. Weak acids decompose hœmoglobin into hæmatin, which gives entirely different spectra, but they do not cause any permanent change in the colouring matter of cochineal or carmine. In my opinion there is no more probability of an experienced observer mistaking these substances for blood, because the ammoniacal solutions give nearly the same spectrum, than of a chemist confounding aluminium bronze with gold, because they are of nearly the same colour.

H. C. SORBY

Broomfield, Sheffield, Oct. 23

* *Quat. Journ. of Science*, vol. ii. p. 208.
† *Medical Press and Circular*, New Series, vol. xii. p. 67; *Monthly Micros. Journ.*, vol. vi. p. 15.

### Are Auroras Periodical?

THE following note on auroras is transcribed from the *Iowa Instructor and School Journal* for April, 1866. As it suggests a hypothesis similar to that proposed by Mr. Wilson, in your journal for September 7, it may not be destitute of interest.

DANIEL KIRKWOOD

Bloomington, Ind., Oct. 4

"*The Aurora Borealis of February* 20, 1866

"Those who witnessed the grand auroral display of the 20th inst., and especially those who have kept a record of similar exhibitions, may have remarked the frequency with which the phenomena have occurred about the same epoch, viz., from February 15 to February 23. Some of the most brilliant that have occurred at this period during the last century are the following:—

| | | | | | |
|---|---|---|---|---|---|
| 1773 | February | 17 | 1848 | February | 20 |
| 1784 | ,, | 23 | 1851 | ,, | 18 |
| 1794 | ,, | 15 | 1852 | ,, | 18 |
| 1838 | ,, | 21 | 1866 | ,, | 20 |

Besides the February epoch, any extended list of auroras will indicate two or three others, the most remarkable of which is that of November 13—18 (See Olmsted's paper in the 'Smithsonian Contributions,' vol. viii.) Fifty-three brilliant auroras have been observed since 1770. Of these, an accidental distribution would assign but *one* to the interval between the 13th and 18th of November; whereas *eight* of the number have actually occurred at that epoch. Are such coincidences accidental, or do they warrant the conjecture that, as in the case of shooting stars, there are particular periods at which the grand displays of the phenomenon most frequently occur?"

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### Forms of Cloud

THE form of cloud represented by Prof. Poëy in his figure *a*, in this week's NATURE, is very similar to that described by the Rev. C. Clouston, LL.D., in his "Explanation of the Popular Weather Prognostics of Scotland," published by A. and C. Black in 1867, and also in Dr. Mitchell's paper "On the Popular Weather Prognostics of Scotland," Edin. New Phil. Journal, Oct. 1863.

Dr. Clouston says that, "when properly developed it was always followed by a storm or gale within twenty-four hours. It is called 'pocky cloud' by our sailors."

He gives a sketch from which, as he says, "it will be seen that this is a series of dark, cumulus-looking clouds, like festoons of dark drapery, over a considerable portion of the sky, with the lower edge well defined, as if each festoon or 'pock' was filled with something heavy, and generally one series of festoons lies over another, so that the light spaces between resemble an Alpine chain of white-peaked mountains. It is essential that the lower edge be well defined, for a somewhat similar cloud, with the lower edge of the festoons fringed, or shaded away, is sometimes seen, and followed by rain only."

Dr. Clouston concluded his notice by saying, "this cloud is well known, and much dreaded by Orkney sailors."

ROBERT H. SCOTT

Meteorological Office, London, Oct. 20

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### Elementary Geometry

IT is scarcely worth while for an anonymous writer to defend his opinions; but since a sentence in my letter of September 21 still continues to elicit remarks, I may be allowed to add an explanation of my meaning. I stated that "no child is capable of taking in a subject, especially if it involves logical thought, except by very slow degrees; and must at the beginning commit much to memory which he does not comprehend." And I called this "a fact." Mr. Wormell says in reply, that the purpose which geometry serves is not the exercise of the memory, and that it is useless if not understood. I entirely agree with him, and my words, if fairly interpreted, do not convey the contrary opinion.

In your last issue Mr. Cooley writes, that my principle, that "a child must of necessity commit much to memory which he does not comprehend," appears to him totally erroneous, and not entitled to be called a fact. But surely the order of Nature with children is to possess themselves of empyrical knowledge by the exercise of memory, and subsequently to get to comprehend what they have thus acquired. Would Mr. Cooley wait until he had made a child comprehend the principles of the decimal scale, before he taught him to add up two rows of figures, and to say, "five and seven are twelve; put down two, and carry one"? If he condescends to the usual course of a "hearer of lessons" in this one instance, he acts upon the admission of my principle.

To apply this to geometry (and perhaps I may be borne with if I use Euclid in illustration): I fancy that many a boy *at the beginning* understands the three first propositions, but not the whole of the fourth. My plan would be, not to keep him at it till he did, but to let him learn it fairly well by rote, and go on, applying the results of the fourth by an act of faith. The second time he went through the book, if he had been decently taught, his difficulties would vanish, and he would already know the proposition

All that I contend for is, that the new book on geometry ought to be capable of such usage. If it contains little more than the chief steps of the solutions, and those disguised (to the unpractised and tottering mind) under symbols, it will not satisfy the want now felt.

A FATHER

### The Beef Tapeworm

As an entozoologist and correspondent of the Academy of Natural Sciences of Philadelphia, I request permission to correct an error recorded in the report of the Academy as given in your columns (at p. 500) this week. Dr. Leidy is represented as having stated that "the minute acetabular pit or fovea at the summit of the head [of *Tænia mediocanellata*] is not mentioned by Kuchenmeister and subsequent observers as a character of that species." I beg to remark that I both figured and described this supplementary sucker-like structure in the first edition of my small work on "Tapeworms," published in 1866 (p. 33 *et seq*). At least two other observers have figured and described this central depression, not only in the adult but also in the measle or cysticercal stage of the worm. Even Bremser recognised it, but his description was for a time overlooked.

84, Wimpole Street, London, Oct. 21 T. S. COBBOLD

### Winter Fertilisation

IN the first number of NATURE, (for Nov. 4, 1869,) I ventured on a hypothesis, founded on a series of observations, that plants which flower in the winter have their organs of reproduction specially arranged to promote self-fertilisation. The following fact, which has just come under my notice, appears to confirm this theory. Plants belonging to the order *Caryophyllaceæ* are, as a rule, strongly protandrous (see my paper in the *Journal of Botany* for October 1870), the anthers discharging their pollen at so long an interval before the maturing of the stigma as to render cross-fertilisation almost inevitable. The other day, Oct. 21, I came across a late flowering patch of *Stellaria aquatica* Scop., in which the anthers were discharging their pollen simultaneously with the maturing of the stigmas, each of the five styles being curled in a singular manner round one of the stamens, so as to bring the stigmatic surface in actual contact with the dehiscing anther. This occurred in several flowers that were just opening, and there was abundance of seminiferous capsules on the plants.

ALFRED W. BENNETT

### Velocity of Sound in Coal

YOUR correspondent will find in Prof. Tyndall's beautiful work on "Sound" the data required for the exact determination of its velocity in different media. I believe that in coal it will be found to be between six and seven times that in air, or about 7,000 feet per second.

If Mr. D. Joseph places his ear against the solid coal of the "rib" or side of the "heading" or gallery, at a distance of some twenty to thirty yards from a collier at work, he will hear two sounds for each blow of the workman's pick or mandril—the first being transmitted through the coal, the second more slowly through the air, the impression being almost irresistible that two persons are at work.

This is probably the origin of the legend, common in more than one coal district, of a collier who always worked alone, did more work than his fellows, and whose diabolical assistant was often heard but not seen.

C. J.

### Changes in the Habits of Animals

YOUR correspondent Mr. Potts in the last number of NATURE furnishes us with a few interesting facts regarding the *Kea*. In a paper which I read about three years ago to the Dumfries Natural History Society, entitled "The Influence of the Human Period on the Sagacity of Animals," and subsequently in a letter published in NATURE, vol. i., on the "Mental Progress of Animals," I endeavoured to show from general considerations, and from the few facts which we possessed on this subject, that the habits and instincts of animals were not so fixed and definite as might be supposed. The general principle for which I contended was that whether we considered the globe to have received its human inhabitants according to the laws of evolution, or in some miraculous manner, the arrival of the human race produced great modifications and changes of surrounding circumstances. These changes were in the direction of increasing the fertility of all vegetable productions capable of sustaining life, and at the same time securing their use entirely for the human family. Hence arose, in the vicinity of man, two new factors; the superior attraction of better food for all kinds of animals, and at the same time the extinction of such animals whose greed was not overruled by sufficient wariness or cunning to become successful thieves. Hence a probable gradual increase in these qualities in the animals maintaining themselves against man.

Since my attention was drawn to this subject, we have had some interesting observations on modifications of swallow's nests by Pouchet, and a discussion as to the validity of his conclusions by Noulet, and now I have read with pleasure Mr. Potts's observations. Most likely the progress of development in the carnivorous habits of the Kea will meet with a check now that shepherds are alive to its depredations; but without the influence of the human period we can scarcely suppose that such development would have begun. I recollect a case of change of habits in weasels. They multiplied so thickly in a parish in the south of Dumfriesshire that some hungry philosopher among them took the initiative in sucking the blood from the cattle. Suspicion having been aroused, the fact was proved, but its discovery was fatal to the weasels, for the whole country-side arose against them, and all but extirpated them in that quarter. It is very interesting to observe what modifications are being produced in the habits of various species of sea-gulls since Glasgow, by its enormous increase of commerce, has wrought great changes in the River Clyde, filling it with all kinds of garbage. The conditions of existence having been favourable, the gull is steadily passing more and more time inland; ascending tributaries of the Clyde, and alighting in flocks on fields that used to have him very seldom.

A new amusement within my own recollection has been afforded the river passengers during the summer months in feeding these sea mews, &c, by throwing overboard food to them, and their increased tameness and boldness of approach in following the river steamers within the last thirty years have been frequently commented on.

J. SHAW

Oct. 23

### A Plane's Aspect

MR. LAUGHTON has hit the nail on the head. "Aspect" is exactly the word wanted. The aspect of a plane is the direction of its normal; and "parallel planes are defined as those which have the same aspect." Two aspects determine one direction, and two directions determine one aspect. Mr. Laughton deserves the thanks of geometers for suggesting so good a word.

Rugby, Oct. 23 J. M. WILSON

THE words "aspect" and "slope" have already a use in relation to the position of planes. They indicate two elements which *together* fix the position. Neither of them, taken alone, can indicate the position of a plane, unless a new and artificial meaning be assigned to one or other. Thus if I speak of the "aspect" of one of the faces of a roof as southerly, I have done something but not all that is necessary, towards describing the position of that face; if I add further that the "slope" is 30° I have definitely assigned the position. Again if I speak of the "slope" of Saturn's rings as 28° (the plane of reference being ecliptic), I have done something towards the description of their position; if I add further that their "aspect" is toward such and such a degree of the sign Gemini, I fully assign their position in space. And so on.

In the preceding sentences I have used the words "slope" and "aspect" as they are already understood. I apprehend that I have also used the word "position" as it is already understood, and that no other word could properly be used in the same sense in descriptive writing. I can see no reason why "position" should be dismissed from the position it has so long occupied, nor why "aspect" and "slope" should be regarded in a new and unfamiliar aspect.

It chances that I have long since had occasion to consider the question suggested last month by Mr. Wilson. In each of twelve books which I have written during the past six years, I have had repeated occasions to consider the slope and aspect, that is, the "position" of many important astronomical planes.

In a large proportion of the essays I have written, the same subject of plane position has had to be considered and described I am, therefore, somewhat seriously interested in opposing as well the disuse of the word "position," which no one can misunderstand, as the use of the words "aspect," "slope," "tilt," &c, in a sense not at present assigned (nor properly assignable) to them. RICHD. A. PROCTOR

### Sea-water Aquaria

I HAVE read with much gusto your article upon the Crystal Palace Aquarium. I am induced by it to put forward a caution with regard to the construction of rock-work in tanks.

Several weeks ago, casually looking over a heap of Bangor slaty rock, on the road bordering the Brighton Aquarium works, and being used for the rock-work of tanks, my attention was attracted by some bright green patches upon some of the stones, which appeared to me to be carbonate of copper, but was probably silicate. Looking further at one with a lens, I imagined that I could also distinguish particles of peacock ore. On attempting to purloin a specimen, I was very properly stopped from so criminal an act by the Cerberus in charge. I wrote to the chairman of the company, stating that, not having examined the stone, I might be only contributing a mare's nest to their zoological collection, but that if it contained much copper the fish would be in danger. I understand that upon receipt of my letter some rock was sent up to Dr. Percy, whose report, I am told, was to the effect that there was much sulphide of copper, and that the pretty green rock was therefore unfit for tank rock-work.

I think this will serve as a caution to the constructors of aquaria to examine all material which is to be in contact with water most carefully before using it. There are so many minerals which would be deleterious that I strongly advise an analysis and report in the case of every untried rock. The accident of my passing a heap of stones has saved the company, with which I am not in the least connected except as a fervent well-wisher, from a large expenditure and a serious scrape.

Allow me to ask those who are accustomed to the management of tanks, whether hydraulic pressure upon a small and strong one would be likely to assist in maintaining life in any of the deep-sea organisms, and whether it would be useful to make recesses for those loving darkness, with the axes opposite the plate glass side, so that a bull's-eye lantern could occasionally throw light upon their actions and mode of life?

Brighton, Oct. 21 MARSHALL HALL

## *ON HOMOPLASTIC AGREEMENTS IN PLANTS*

AT the recent meeting of the British Association I pointed out in a short communication the difference that existed between mimicry in animals and what has been spoken of under that name amongst plants. The distinction was sufficiently obvious, and must have occurred to everyone who had given the matter any consideration, but my object was to try to raise a discussion upon the whole subject as exhibited in plants.

I fancy it is hardly sufficiently understood how commonly this agreement of facies occur in plants widely differing in other respects. I will give a few illustrations of it. Humboldt remarks ("Views of Nature," p. 351): "In all European colonies the inhabitants have been led by resemblances of physiognomy (*habitus, facies*) to apply the names of European forms to certain tropical plants, which bear wholly different flowers and fruits from the genera to which these designations originally referred. Everywhere in both hemispheres the northern settler has believed he could recognise alders, poplars, apple, and olive trees, being misled for the most part by the form of the leaves and the direction of the branches." Nor has the popular eye alone been deceived by these resemblances. Schleiden states ("The Plant," p. 255) that Australia has in common with Europe a very common plant, the daisy, yet Dr. Hooker has pointed out (Flora of Tasmania, pl. 47) that the plant intended by Schleiden is the very similar but distinct *Brachycoma decipiens* Hook. fil. Again, true flowering plants belonging to the very curious family *Podostemaceæ* have been figured as liverworts and other cryptogamic plants (Berkeley, Intr. to Crypt. Bot., p. 5). Many other instances of similar errors might be given.*

Since I read my paper, I have met with an essay by Schouw, in which he enumerates facts of the same kind. "There is still," he says ("Earth, Plants, and Man," p. 61), "another kind of repetition which I might call habitual *repetition*, or denominate *mimicry*, if this expression was not at variance with the subjection to law which exists throughout nature, but to comprehend which our powers are often insufficient." After various illustrations he proceeds:—"In the genus *Mutisia* we have the remarkable sight of a compositous flower, with the tendrils of a leguminous plant." (This by an accidental coincidence was one of the instances which I, myself, used at Edinburgh.) "In *Begonia fuchsioides* the leaves are similar to a *Fuchsia*, and very different from the other forms of leaf among the begonias, and the colour of the blossom likewise reminds us of the fuchsias. We have another most striking example in certain Brazilian plants, which although possessed of perfectly developed flowers and fruits, mimic, as it were, in their leaves and stems, groups of plants of much lower rank." (He is alluding to the *Podostemaceæ* mentioned above.) "*Lacis fucoides* resembles certain seaweeds so much, that it might be mistaken for one by a person who did not see the flowers. *Mniopsis scaturiginum* strikingly resembles a *Jungermannia*."

I suggested that when a plant put on the characteristic facies of a distinct natural family, it might conveniently be spoken of as a pseudomorph, having in view an obvious analogy in the case of minerals. I do not, however, now think on further consideration, that this term, although convenient, includes all the cases. In small natural families it is not always easy to recognise any general habit or facies at all, and in the case of plants belonging to different families where this is the case, but having a similar habit, it would be purely arbitrary to fix the pseudomorphism on any of them. Again *all* the individuals of distinct groups of plants might have a similar habit, and the same remark would apply. The difficulty is, however, got over by speaking of the plants in these cases as *isomorphic*.

My friend, Mr. E. R. Lankester, has pointed out to me that agreements of this kind may all come under what he has termed homoplasy (Ann. and Mag. of Natural History, July 1870). This is the explanation he gives of this expression:—

"When identical or nearly similar forces, or environments, act on two or more parts of an organism which are exactly or nearly alike, the resulting modifications of the various parts will be exactly or nearly alike. Further, if, instead of similar parts in the same organism, we suppose the same forces to act on parts in two organisms, which parts are exactly or nearly alike and sometimes homogenetic, the resulting correspondences called forth in the several parts in the two organisms will be nearly or exactly alike. I propose to call this kind of agreement *homoplasis* or *homoplasy*. The fore legs have a homoplastic agreement with the hind legs, the four extremities being, in their simplest form (*e.g. Proteus*, which must have had ancestors with quite rudimentary hind legs), very closely similar in structure and function. . . . Homoplasy includes all cases of close resemblance of form not traceable to homogeny."

The resemblances, therefore, above described between the vegetative organ of plants with no close generic relations, may be described as homoplastic. The difficulty

* Perhaps one of the most striking is the Natal cycad *Stangeria paradoxa* having been published and described by Kunze as a species of *Lomaria*, a genus of Ferns.

still, of course, remains to show *how* the homoplasy has been brought about. In some cases, as in the homoplastic forms of American Cactaceæ and South African Euphorbias, or in the stipular bud scales of many wholly unrelated deciduous trees, the nature of the similar external conditions may possibly be made out with some correctness. Again, Dr. Seemann has pointed out that by the rivers in Nicaragua and in Viti. the vegetation, although composed of very different plants, puts on the willow form ("Dottings by the Roadside," p. 46). A phenomenon true of two distant places accidentally contrasted, might be expected to obtain more generally; at any rate, among our indigenous riparian plants *Lythrum Salicaria* and the willow-herb are, as their names indicate, additional illustrations. The band of vegetation that fringes a stream is always densely crowded with individual plants, and it is easy to see that elongated and vertically disposed leaves would be most advantageous, exactly as they are to the gregarious plants of meadows and plains. The homoplastic agreement of riparian plants may be therefore a direct result of selective effort due to the position in which they grow.

In other cases the operation of similar external moulding influences is not so easy to trace. It might, perhaps, however, be imagined that plants would hereditarily retain the effects when the influences had ceased to operate, and no new ones had come into operation precisely adapted to obliterate the work of those that preceded them. Suppose, for example, that willows got their habit and foliage from ancestors that were exclusively riparian, then any descendant that happened to be able to tolerate situations with less abundant supplies of moisture, would not necessarily lose their characteristic foliage on that account. Such races might be expected to occur near rivers subject to periodic droughts, since under these conditions any others would be likely to perish. Under such circumstances we should have cause and effect no longer in contiguity; the riparian habit surviving the riparian situation.

I suggested at Edinburgh that possibly similar habits in plants might be brought about by *different* causes. This was only a suggestion, and probably what has just been said is a truer account of the matter. At any rate the illustration I gave of my meaning has been quite misunderstood (as, for example, in the last number of the *Popular Science Review*). It is well known that there are a certain number of plants indigenous to the British Isles, which are found at a considerable height upon mountains and also upon the sea-shore, but not in the intervening space. In the latter situations they contain more sodium salts than in the former, and inasmuch as these salts are destructive to many plants, those that compose a strand flora must be able to tolerate them, and this of course is an advantage, because many of their competitors are poisoned off. Similarly plants of mountains must have a similar advantage over others in ability to tolerate mountain asperities of climate. Now, suppose a mountain submerged; its flora and certain portions of that of the strand come to coincide. Then if we suppose the mountain gradually to emerge, some of these plants will spread downwards under the uncovered surface, and travel over the whole of the interval that ultimately separates the mountain top and the strand. Why, then, do they not remain there? Simply, I believe, because they are elbowed out by other plants which, nevertheless, cannot tolerate the conditions of life either on the mountain or the shore, and leave these, therefore, as refuges which they are unable to invade. It is possible that the action of similar soil constituents might help to bring about homoplastic agreements in plants. The suggestion is not, however, one that occurred to me to make. My object was simply to show how two perfectly different causes might produce the same effect, namely, that of giving immunity from competition to a small group of plants. Except as an illustration of this point, the matter was quite irrelevant to the subject about which I was speaking.

W. T. THISELTON DYER

## ON THE DISCOVERY OF STEPHANURUS IN THE UNITED STATES AND IN AUSTRALIA

THE time has now arrived when a full statement of the facts relating to this interesting parasite *Stephanurus dentatus*, should be made more generally known; for not only is the progress of helminthological science likely to be checked by delay in this matter, but, in the absence of definite information, the several merits of the original discoverer and describer of this entozoon are likely to be altogether ignored. I therefore record the facts and inferences in the order in which they have recently come under my notice.

On the 10th of January last, through the firm of Messrs. Groombridge, I received an undated communication from Prof. W. B. Fletcher, of Indianapolis, Indiana, U.S.A. In that letter Dr. Fletcher announces that he has "found a worm" infesting the hog, and he helps one to realise its abundance by adding that he obtained it "in nine out of ten hogs" which he examined. After recording some other important facts respecting the tissues and organs which were most infested by the parasite, Dr. Fletcher remarks that he cannot find any description of the worm in the work on Entozoa issued by the publishers above mentioned, nor in the writings of Von Siebold and Küchenmeister, and he therefore encloses specimens for my determination, requesting a reply.

As I have already stated in my first letter recorded in the *British Medical Journal* (for January 14, p. 50, where many other particulars are given which I need not here recapitulate) I was instantly struck with the "strongyloid character" of the fragmentary and shrivelled up specimens, and I may also add that it at once occurred to me that I had had some previous acquaintance with a scientific description of the worm. Proceeding, therefore, to turn over a series of helminthological memoirs, for many of which I stand indebted to the late veteran, Dr. K. M. Diesing, of Vienna, I soon had the good fortune to find the desired record. The memoir in question forms part of the "Annalen des Wiener Museums" for 1839, the full title being "Neue Gattungen von Binnenwürmern, nebst einen Nachtrage zur Monographie der Amphistomen."

As this work is probably little, if at all, known in the countries now necessarily most interested in the history of this entozoon, I cannot, perhaps, do better than transcribe Dr. Diesing's brief notice of the original discovery, together with his description of the external characters presented by the worm. After naming the parasite *Stephanurus*, on account of the coronet-like figure of the tail of the male, and giving a technical description of the species, he continues as follows:—"At Barra do Rio Negro, on the 24th of March, 1834, Natterer discovered this peculiar genus occurring singly or several together in capsules situated amongst the layers of fat, in a Chinese race of *Sus scrofa domestica*. Placed in water or in spirits of wine, they stretched themselves considerably, and almost all moved up and down."

"The males measure from ten to thirteen lines in length, the females from fifteen to eighteen lines, the former being scarcely a line in breadth at the middle of the body, whilst the latter are almost a line-and-a-half in thickness. The curved body thickens towards the tail, is transversely ringed, and when viewed with a penetrating lens, is seen to be furnished with integumentary pores. The oral aperture opens widely, and is almost circular; it is supplied with six marginal teeth, two of which, standing opposed to one another, are larger and stronger than the rest. The tail of the male, when evenly spread out, is surrounded by a crown of five lancet-shaped flaps; the combined flaps being connected together from base to apex by means of a delicate transparent membrane. The single spiculum situated at the extreme end of the tail, projects slightly forwards, being surrounded by three skittle-shaped bodies. The tail of the female is curved upon itself, rounded off, and drawn out at the extreme end into a straight, beak-shaped point, whilst to both sides of the stumpy caudal extremity of the body, short vesicular elevations are attached. The female generative opening occurs at the commencement of the second half of the body.

"Judging by its external characters this genus is most closely allied to Strongylus."

The above description is supplemented by a more lengthened account of the internal organisation of the worm; this part of the record displaying in an especial manner those powers of accurate observation which so fully characterised the great systematist in helminthology prior to the time when he was deprived of his eye-sight.

Having communicated to Prof. Fletcher my views respecting the true history and identification of *Stephanurus*, he was pleased to supply me with some further particulars. Thus, (after receiving my reply) in his second communication (dated from Indianapolis, February 22), he says: "I at once renewed my researches, and was rewarded by finding the little saw-like teeth, upon a six-sided jaw, and, if I mistake not, two larger teeth or hooks. I also removed the lungs, heart, and liver, entire, from several hogs (just killed by shooting in the head) and found the worm, as before stated, in the liver, in all the hepatic vessels, and also in the vena cava. In some cases I found the eggs in abundance in the pelvis of the kidney, and in the urine, even when I could discover no cysts or worms about them."

Dr. Fletcher then alludes to the circumstance that he had since his first letter to me placed himself in correspondence with Prof. Verrill, who, it appears, had previously examined the worm. Prof. Fletcher also obligingly enclosed Prof. Verrill's paper, extracted from the *American Journal of Science and Arts* of September 1870, and, in so far as I may be guided by its contents, it would now appear that the very first specimens which were obtained in the United States were the "five" examples sent by Dr. M. C. White, of New Haven, U.S., to Prof. Verrill, who adds:—"In the second instance, at Middleton, Conn., Dr. N. Cressy found large numbers of the worms in the fat about the kidneys of a young Suffolk pig, brought from New Jersey."

The title of Prof. Verrill's paper is, "Description of *Sclerostoma pinguicola*, a new species of entozoa from the hog."

At this point I pause to remark on some of the more practical questions connected with *Stephanurus*, for it must be quite obvious that so large a parasite, comparatively speaking, must, when present in great numbers, give rise to a great amount of disease, even if it should not ordinarily prove fatal. Dr. Fletcher, indeed, does not hesitate to write as follows:—"It is my opinion that this parasite is the cause, in some way, of the hog cholera, which has created such sad havoc within the past ten years, over the pork-producing parts of America. One farmer told me a few days ago that within a month his loss alone from this cause was over one hundred head; and sometimes, in one neighbourhood, in a few days time, thousands have perished, although this season is not a cholera year, as our farmers say. I advised one farmer to burn or bury the dead animals; but he informed me that he believed that fewer hogs die of the disease after eating the dead animals than those kept from them. Unfortunately, in this State there is no law guarding the spread of disease, neither is there any reward of reputation or gain for pursuing any investigation that would bring pork and beef packers into disrepute. I myself could not get a pig's kidney or beef's liver in our city market, because I made investigations in some Texas cattle (being cut up in our market) which damaged their sale a few years ago." In a third letter Dr. Fletcher tells me that greater facilities for examining the carcases of hogs had since been accorded him through the liberality of a Liverpool firm of pork packers, who had already killed 75,000 hogs during the summer season, *i.e.*, up to the date of the first week in July. In hot weather the slaughtering is conducted in ice-houses.

These practical observations by Dr. Fletcher appear to me to be of the highest importance, even though it should eventually turn out that there is no immediate connection between the occurrence of *Stephanurus* and the hog cholera epidemics. That this opinion rests upon substantial data seems probable from the circumstance that we have now not only received evidence of the occurrence of *Stephanurus* in Australia, but we are further apprised that the pigs which harbour it die of the disease superinduced by their presence. As I have already stated, in my second letter, published in the pages of the *British Medical Journal*, our earliest intelligence on this point rests upon the evidence furnished by a series of unnamed slides transmitted from Sydney to the President of the Royal Microscopical Society of London. Through the kindness of the Society's able Secretary, Mr. Slack, F.G.S., I was permitted to examine, identify, and name all the specimens, and it was then that I recognised *Stephanurus* amongst the number.

On the 4th inst. Dr. Morris's paper, which accompanied the specimens, was read to the Society. In that paper the author, like Prof. Verrill, expresses his belief that he has found a new entozoon, "its habitat being the fat surrounding the kidney of the pig." He speaks of it as occurring both in the "free and encysted state, the encysted being its final stage of existence," and, he adds, "its solid parts ultimately disappear, leaving a greyish brown fluid containing thousands of eggs." Those who desire further particulars in reference to the parasitism of pigs and sheep in Australia should consult Dr. Morris's paper, which will appear in the forthcoming November number of the *Monthly Microscopical Journal*. Dr. Morris speaks of the pigs as dying from some mysterious disease, and thinks "it is possible that this worm or its brood may be the cause." In some cases their death takes place quite suddenly, and this he supposes to be due to peritonitis set up by the swarming and migrations of the progeny. Be this as it may, it is interesting to notice the remarkable corsespondency of the conclusions arrived at by Dr. Fletcher and Dr. Morris independently. It will probably not be difficult to ascertain hereafter whether or not the maladies respectively termed "Hog Cholera" and "Mysterious Disease" are one and the same disorder; but whatever happens in this respect, it is now quite clear that this parasite, hitherto little regarded, and for many years past persistently overlooked, is extraordinarily prevalent in the United States, and, perhaps, equally so in Australia, it being further evident that its presence in the flesh of swine is capable of producing both disease and death. The statement of the worthy American farmer that the swallowing of infested flesh by a pig does not necessarily involve the pig-eating hog in a bad attack of a so-called "Cholera disease" requires to be further tested, and it also remains to be proven whether or not the *Stephanurus* be capable of passing through all its developmental changes from the egg to the adult form within the body of the bearer without having at some time or other gained access to the outer world. The comparatively large size of the ova, which I find to be about $\frac{1}{300}''$, or more than four times the size of that of Trichina, is not without significance; but as yet we are unacquainted with the larval stages of growth. If no intermediary bearers are necessary to its development, we ought not to have to wait long for a complete record of the life-history of *Stephanurus dentatus*.

T. S. COBBOLD

## *BALL ON MECHANICS**

THE object of this book is to "prove the elementary laws of mechanics by means of experiments"—a method the exact opposite of that generally adopted. According to the usual method, a few very general principles are assumed as derived from experimental data, a group of intermediate principles is then obtained deductively, by the aid of which the action of forces in particular cases can be analysed. The particular cases may be such as have an interest from their bearing on practical questions, but they are only examples of a general method applicable to innumerable other cases. There are therefore two distinct objects for which mechanical experiments may be made—viz, either to verify the fundamental principles, or to verify the deductions drawn in particular cases. Experiments of the former kind are absolutely essential to the existence of the science. Unless, for instance, the conditions of the action of the force of friction are determined by experiment, no deductions as to cases into which that force enters have any but a theoretical value. The same is true in all similar cases; such questions as, whether quantity of matter is proportional to weight, whether gravity at a given station is sensibly a constant force, whether the elasticity of solid bodies follows Hooke's law, and if so within what limits, can be answered by experiment only. Such questions, on the other hand, as the tension of a tie-rod under given circumstances, the relation between the weights which keep a given lever at rest,

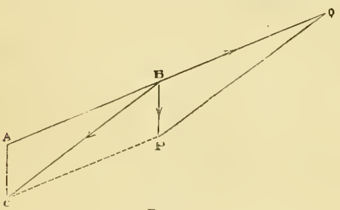

FIG. 1.

the relation between the power and the weight in a block and tackle, the form of the surface of a revolving liquid, admit of exact answers by deduction from the proper data, and, of course, the answers may be tested by experiment. Such experiments clearly have a different object from those of the former class. They have, indeed, this in common, that experiments of the latter kind also serve to verify fundamental principles, but they do so indirectly. It is, however, from the teacher's point of view that their value will be found greatest. In teaching the elementary parts of mechanics perhaps the greatest difficulty experienced is to make the learner feel that the diagrams drawn on the black board represent facts, that, for instance, the conclusion deduced from a triangle is really applicable to a crane. Put the experiment side by side with the deduction, and it will be seen that the experiment cannot fail to bring home to the mind of the learner that his reasoning relates to things and not merely to abstractions.

Let CB (Fig. 1) represent the jib or strut, and AB the tie-rod of a crane, the line AC being vertical. Let a weight P hang from A, and let it be required to determine the forces transmitted through the tie and the jib. P can be resolved into two forces acting along BC and AB produced, and an inspection of the figure will show that these forces bear to P the same ratio that the lines BC and AB bear to AC, and that the force along BC is a thrust, and that along AB a tension. This analysis is perfectly general.

* *Experimental Mechanics: a Course of Lectures delivered at the Royal College of Science for Ireland.* By Robert Stawell Ball, M.A. With Illustrations. (London and New York: Macmillan and Co., 1871.)

We will now give Mr. Ball's experiment in illustration of the same question:—"A piece of pine BC, 3′ 6″ long and 1″ × 1″ in section (Fig. 2) is capable of turning round its support at the bottom B by means of a joint or hinge; it is held up by a tie AC 3′ long, which is attached to the support exactly above the joint. AB is 1′ long. From the point C a wire descends, having a hook at the end, on which a weight can be hung. The tie is attached to the spring balance, the index of which shows the strain. The spring balance is supported by a wire strainer, by turning the nut of which the length of the wire can be shortened or lengthened as occasion requires. This is necessary, because when different weights are suspended from the hook the spring is stretched more or less, and the screw is then employed to keep the entire length of the tie at 3′. The remainder of the tie consists of copper wire" (p. 29). Mr. Ball then goes on to notice that when a weight of 20lbs. is placed on the hook, the strain, as determined by the spring balance, is 60lbs., thus verifying the analysis of the case given above.

As an example of an experiment of the former class we will take the following,—it is the form in which Mr. Ball

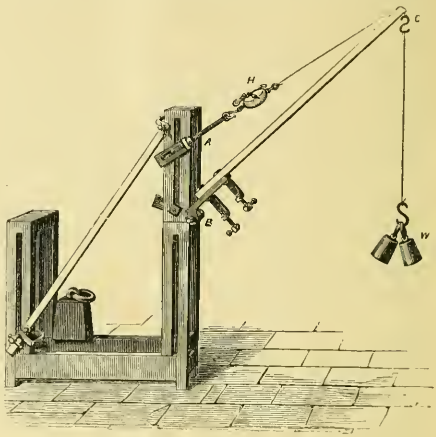

FIG. 2.

gives Galileo's experiment of dropping bodies from the top of the Tower of Pisa. The figure (Fig. 3) is so perfect that it scarcely requires explanation. So long as the current is in action, the horse-shoe G is magnetic, and a ball of iron F remains suspended from it. When the current is broken G is no longer magnetic and F falls. In this manner, by including the wire round both horse-shoes in the circuit, a ball of iron and one of wood, into which a flat-headed nail has been driven, can be kept suspended, and then by breaking the circuit they can be let fall at exactly the same instant, they are seen to reach the cushion at the same instant, and are thus shown to fall through equal spaces in the same time. Mr. Ball describes and discusses the experiment at some length, and shows how it proves that at a given station the attraction of gravitation on different bodies is proportional to their masses.

The above examples will give a better notion both of the contents and illustrations of the book than any long description. We may say, however, that the book contains a clear and correct exposition of the first principles of mechanics, and illustrates, by well-chosen experiments, all the points in the subject that can be fairly called elementary. The figures reproduce all the circumstances of the experiments with so much exactness that with moderate care the reader will understand the points illustrated nearly as well as if he saw the experiments themselves. In great part Mr. Ball has devised these experiments himself, and thus in the well-worked field of elementary mechanics he has introduced much that is original in treatment, and in some parts—particularly in his lecture on friction—there will be found something

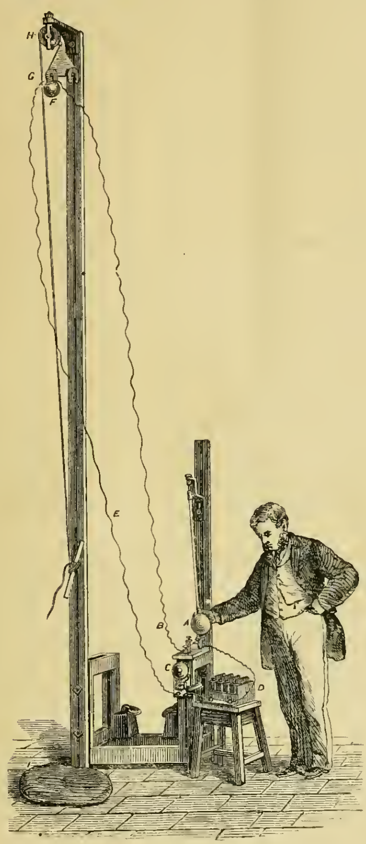


FIG. 3.

more. On the whole the work is one that will amply repay perusal, both by teacher and student, and is a most valuable supplement to works on the theory of mechanics. Nor must we take leave of the volume without adding that its general appearance—due to paper, printing, and illustrations—is truly beautiful, and, in fact, we cannot call to mind any English book of the same class which will bear comparison with it in these respects.

J. F. TWISDEN

## *ON THE BEST FORM OF COMPOUND PRISM FOR THE SPECTRUM MICROSCOPE*

IN studying the spectra of coloured solutions and solid substances by means of the spectrum microscope, it is most important to employ prisms having a suitable amount of dispersion. It would be a very great mistake to suppose that the result is better with a very wide dispersion. This, of course, makes the spectrum larger, but very greatly impairs the definition of the absorption-bands. Everyone who has had experience with an ordinary microscope must be well aware that a particular magnifying power is best for each particular class of object or kind of structure, and that in some cases nearly all the important characters would be lost by employing too high a power; but at the same time too low a power would be equally disadvantageous in other respects. This analogy holds good in the case of the dispersion of prisms. The power ought to be regulated by the character of the absorption-bands. If they are dark, narrow, well-defined, and lie close together, as in the case of partially opaque crystalline blow-pipe beads of borax containing deposited crystals of oxide of lanthanum with oxide of didymium, a somewhat powerful dispersion is not only admissible, but quite necessary to separate some of the bands. If, however, they are broad and faint like those seen in the spectra of many of the colouring matters found in animals and plants, a powerful dispersion spreads them over such a wide space, and makes the shading off so gradual, that the eye can scarcely appreciate the extra amount of absorption; whereas, when a lower dispersive power is used, a well-marked absorption-band can easily be seen. This is more especially the case with impure mixtures. I have found that when it was requisite to examine a mixed, somewhat turbid, coloured solution to detect, if possible, the presence of some substance which, when alone, gave a spectrum with distinct absorption-bands, no trace could be recognised by means of a prism of high dispersive power; but it could be detected without any difficulty with a lower. In carrying on practical investigations it is far more important to be able to succeed in such a case than to exhibit on a large and more imposing scale the spectra of a few substances which give dark and well-defined bands. There can be no doubt that it is a great advantage to have a number of prisms of different dispersive power, so that in all cases the most suitable may be used; but at the same time some observers might not wish to have more than one, and thus it becomes important to decide what amount of dispersion is the best for the generality of objects—is sufficiently great to divide narrow, closely-placed bands, and yet not so great as to prevent our seeing broad and fainter. No magnifying power whatever is applied to the spectrum itself in the instrument now under consideration.

As described in some of my former papers,* the compound, direct-vision prisms first made for me by Mr. Browning were composed of two rectangular prisms of not very dense flint glass, and three of crown glass, one being rectangular, and the others of an angle of about 75°. This combination gives a dispersive power, which shows faint bands very well; but is not enough to divide the narrow and close bands seen in the spectra of a few substances. Mr. Browning then made prisms of similar construction, only that very dense flint glass was employed; This combination gives about double the former dispersion, which divides narrow and close bands admirably, but sometimes shows broad and fainter bands so very badly that they can scarcely be recognised. It thus appeared to me that, if only one compound prism be supplied with the instrument, the best dispersive power would be intermediate between these two extremes. At the same time much would depend on the particular purpose to which the instrument was applied, and also, to some extent, on the individual differences between different observers.

Mr. Browning has described † the plan that he proposes for the measurement of the position of absorption-bands by means of a bright line, seen by reflection from the surface of the prism, moved backwards and forwards

* *Popular Science Review*, vol. v., 1866, pp. 66—77; Brit. As. Report, 1865 (pt. 2), p. 11.
† *Monthly Microscopical Journal*, vol. iii. p. 68.

by a micrometric screw with a graduated head. My objection to the original construction was that the bright line was photographed on a small piece of glass, and the background was so far from being black as to much impair the spectra of substances that will not transmit a bright light. I suggested that in place of this glass plate a small piece of tin-foil should be used, having a very minute hole in it. This shows far brighter than the line in the photograph, and the back-ground is quite black; and thus the bright dot can easily be seen even when in the brightest part of the spectrum, and there is no extraneous light to impair the faintest absorption-bands The only important objection to this method of measuring their position is, that a very slight movement in the apparatus, due to the loose fitting of moveable parts, alters the readings, and that the value of the measurements, as read off by the micrometer, depends on so many variable particulars, that nearly every instrument might have a different scale. The chief objection to my interference scales* is the difficulty of making all agree absolutely, but when accurately made they have not the above-named disadvantages. I therefore still adhere to that plan, but at the same time I have found the bright dot arrangement very useful, not only as an indicator in showing spectra to others, but also as a fixed point in comparing different spectra, or in counting the bands of the interference scale. Possibly without such help some observers might find this difficult, and would prefer in all cases to measure the position of bands by means of the graduations on the circular head of the micrometer, and therefore I was anxious to devise a prism that would have a dispersive power intermediate between the two extremes already mentioned, and at the same time have the upper face inclined at an angle of 45° to the axis, so that the bright dot micrometer might be employed conveniently. To accomplish this, Mr. Browning made for me a prism composed of two rectangulars of crown glass, one rectangular of very dense flint, and one of less dense, cut at such an angle as to give direct vision. This combination gives what I consider to be as good a medium dispersion as could be wished, and at the same time enables us to measure the position of the bands with the bright-dot micrometer as accurately as is requisite in nearly all practical applications. Subsequent trials have shown that the same advantages may be secured in a more satisfactory manner by replacing the less dense flint glass prism by two, one of flint and the other of crown, of such angles as give direct vision for the whole combination of five. The dispersion is very nearly the same as that of two prisms of ordinary flint glass of 60° angle.

I have been thus careful in explaining the advantages and disadvantages of various arrangements, because the successful use of the spectrum-microscope depends so much on such particulars, and because so many who have not had experience in the practical working of the instrument seem anxious to see a wide spectrum, and overlook the practical importance of being able to recognise obscure absorption-bands. My own experience of this question agrees with that of most of my friends who have worked with the instrument, and yet I am quite prepared to believe that a different amount of dispersion might better suit some observers, and to admit the truth of the German saying, "Eines schickt sich nicht für alle."

H. C. SORBY

* Proc. Roy. Soc., vol. xv. p. 434.

## NOTES

RIPE in years and in honours, his work done and his fame world-wide, amid the regrets of all ranks of his countrymen, Sir Roderick Murchison has gone to his rest. It is nearly a year since he was seized with an illness which disabled him from further active work. Yet in the interval he has shown all his old interest in the affairs of which he has so long been the heart and soul, keeping up his intercourse with the world of science by reading, and with many of his associates by personal interviews at his own residence, and by correspondence. To the last his wonderful memory remained true, even to trifling details of place and date. Within the last few weeks, however, the disease made sad progress, and though he continued to enjoy frequent carriage exercise, his physical strength became less able to withstand any malign effects which the chills of autumn bring with them. On Thursday last he was seized with bronchitis, and gradually sank under the attack, till he died at half-past eight on Sunday evening, the 22nd inst. We shall offer next week a fuller reference to Sir Roderick's life-work and scientific influence. For the present, and ere the earth closes over all of him that is mortal, let us only say that in him Science has lost a hard-working and distinguished cultivator, as well as an influential patron, and that to a narrower circle of mourners his loss is also one of a kindly large-hearted friendship.

WE have to record the death, on Saturday last, at the age of seventy-nine, of Mr. Charles Babbage, the eminent mathematician and mechanician. The most important events of his life, as well as some of the eccentricities of his character, are familiar to the public through his autobiographical volume, "Passages in the Life of a Philosopher." Born in 1792, he entered Trinity College, Cambridge, in 1810, and was transferred to St. Peter's the following year. At his B.A. degree he did not take honours in mathematics, not having specially pursued that subject of study as a student, and was understood to have been disappointed at not being elected a fellow. In 1828 he was however elected Lucasian Professor of Mathematics at Cambridge, a position once held by Sir Isaac Newton. He published no less than eighty volumes, but his claim to public notice rested chiefly on his invention of the Difference Engine, on which he spent immense labour and a large sum of money. Notwithstanding his eccentricities and his failings, Mr. Babbage was a mathematician and an inventor of whom England may be justly proud.

THE English Government Eclipse Expedition sailed this morning for Ceylon in the *Mirzapore* from Southampton, Mr. Lockyer in charge, expecting to reach Point de Galle on Nov. 27. They hope to confer with the Indian observers as soon as possible, and plan a concerted campaign. The experience of the last Expedition necessitated that the whole of the instructions should be rewritten; and the Eclipse Committee of the British Association, consisting of the following gentlemen:—Sir William Thomson, L.L.D., F.R.S., President, Prof. J. C. Adams, D.C.L., F.R.S., G. B. Airy, F.R.S., Astronomer Royal, Prof. Clifton, F.R.S., Warren de la Rue, D.C.L., F.R.S., Dr. Frankland, F.R.S., Captain Douglas Galton, C.B., F.R.S., George Griffith, M.A., J. R. Hind, F.R.S., W. Lassell, F.R.S., President R.A.S., Lord Lindsay, J. Norman Lockyer, F.R.S., General Sir Edward Sabine, K.C.B., President R.S., General Strachey, F.R.S., W. Spottiswoode, LL.D., F.R.S., Colonel Strange, F.R.S., Prof. Stokes, D.C.L., F.R.S., and Dr. Thomas Thomson, F.R.S., have had very hard work to get the arrangements completed, in which they have been most zealously assisted by the Government, and by the Peninsular and Oriental Steam Boat Company. Lord Lindsay placed at the disposal of the Expedition the whole of his valuable instruments, and has sent a photographic observer at his own expense. Several members of the Expedition have voluntarily given up a month of their time before starting to perfect themselves in spectroscopic and other observations at the Royal College of Chemistry, a most commendable example to others in similar situations. We have now only to wish the Expedition a prosperous voyage, and better fortune with regard to weather than was experienced in Sicily last year.

WE have to announce the return of Mr. Gwyn Jeffreys from

North America. He examined all the principal collections of shells in the United States and Canada, and especially those made in the deep-sea explorations of the Gulf of Florida and Gulf of St. Lawrence. The former was in the charge of Dr. Stimpson at Chicago; and Mr. Jeffreys was entrusted with specimens (some of them unique) of all the species which appeared to him the same as certain undescribed species dredged by him in the depths of the East Atlantic during the *Porcupine* expeditions of 1869 and 1870. These specimens may be the only ones saved from the Museum of the Academy of Sciences at Chicago, which it is greatly feared was destroyed by the late deplorable conflagration. Several species of North American land and fresh-water shells will also be found to inhabit the eastern hemisphere, although bearing different names. Through the kindness of Prof. Baird, Mr. Jeffreys had an opportunity of dredging on the coast of New England in a Government steamer; and he everywhere received great hospitality and attention. He particularly acknowledges his obligation to Mr. Anthony, Prof. Agassiz, and Prof. Shaler, of Cambridge; the Hon. S. Powel, of Newport, R.I.; Prof. Baird, of Washington; Prof. J. C. Draper, of New York; Mr. Binney, of Burlington, N.J.; Dr. Isaac Lea, of Philadelphia; Dr. Stimpson and Mr. Blatchford, of Chicago; and to Principal Dawson and Sir W. E. Logan, of Montreal.

*Les Mondes* records the death of M. Henri Lecocq, Professor in the Faculty of Sciences at Clermont Ferrand, eminent both as a botanist and geologist. His life has been spent in encouraging and assisting the cultivation of the sciences to which he especially devoted himself; and by his will he has devoted his property to the same end. He leaves to the town where he resided the sum of 150,000 francs, of which 50,000 is bestowed on the botanic garden established by him, 50,000 to the maintenance of water-supply and fountains, and 50,000 to the establishment of covered markets. M. Lecocq leaves besides to the town all his collections of natural history, zoology, botany, geology, and mineralogy, as well as all the cabinets which contained them.

Mr. Alfred Henry Garrod, scholar in natural science of St. John's College, Cambridge, and formerly Demonstrator in Physiology to Prof. Humphry, has been appointed Prosector to the Zoological Society of London. There is probably no other post in the world which affords opportunities for the study and advancement of animal physiology and comparative anatomy equal to those enjoyed by the possessor of this office, owing to the extraordinary extent of the Society's vivarium in the Regent's Park. Mr. Garrod, we understand, will not enter upon his duties until the beginning of the new year.

Mr. Davidson, of King William's College, Isle of Man, has been elected a Scholar in Natural Science at Sidney Sussex College, Cambridge.

A scholarship is announced at Balliol College, Oxford, on the foundation of Miss H. Brakenbury, "for the encouragement of natural science," of the value of 70*l.* for three years. This is open to all candidates who have not exceeded eight terms from matriculation. The examination begins on Tuesday, November 21. Papers will be sent in (1) Mechanics and Physics, (2) Chemistry, (3) Physiology, but no candidate will be expected to take up more than two subjects at the most. There will also be a practical examination in the above subjects. Further information can be obtained from the tutors.

We understand that the next number of the *Contemporary Review* will contain an important article by Prof. Huxley in comment on some portions of Mr. Mivart's "Genesis of Species."

The following are among the Publishers' announcements of scientific works for the approaching season:—From Messrs. Longmans:—The Royal Institution, its Founder and its First Professors by Dr. Bence Jones, Hon. Sec.; Spectrum Analysis in its Application to Terrestrial Substances and Physical Constitution of the Heavenly Bodies, familiarly explained by Dr. H. Schellen, with Notes by William Huggins, LL.D, D.C.L, F.R.S, 1 vol. 8vo. with coloured plates and other illustrations; a Smaller Star Atlas, for the use of schools and junior students of Astronomy, by R. A. Proctor, B.A., F.R.A.S., in twelve circular coloured maps and two index maps, with an introduction showing how the stars may be recognised and their motions studied and understood; Popular Lectures on Scientific Subjects, by Prof. Helmholtz, translated by H. Debus; Elements of Materia Medica and Therapeutics, being an abridgement of the late Dr. Pereira's Materia Medica, and comprising all the medicines of the British Pharmacopœia, together with such others as are frequently ordered in prescriptions, and required for the use of medical practitioners, edited by Robert Bentley. From Mr. Murray:—The Principles of Geology, or the Modern Changes of the Earth and its Inhabitants considered as illustrative of Geology, by Sir Charles Lyell, Bart., F.R.S., 11th edition, thoroughly revised, illustrations, Vol. 1; The Metallurgy of Copper, Zinc, and Brass, including Descriptions of Fuel, Wood, Peat, Coal, Charcoal, Coke, Fire-Clays, by John Percy, F.R.S., new edition, revised, many illustrations. From Messrs. Macmillans:—A Treatise on the Origin, Nature, and Varieties of Wine, being a complete manual of Viticulture and Œnology, by J. L. W. Thudichum, M.D., and August Dupré, with numerous illustrations; the Ministry of Nature, by the Rev. Hugh Macmillan; a series of Science Primers, under the joint editorship of Profs. Huxley, Roscoe, and Balfour Stewart; the following will be ready about Christmas: Introduction, by Prof. Huxley; Chemistry, by Prof. Roscoe; Physical Science, by Prof. Balfour Stewart. From Messrs. Bell and Daldy:—Alpine Plants, containing more than one hundred coloured illustrations of the most striking and beautiful Alpine flowers, with descinetions by D. Wooster. From Messrs. Deighton, Bell, and Co:—The Desert of the Exodus, Journeys on Foot in the Wilderness of the Forty Years' Wanderings, by E. H. Palmer, M.A., Fellow of St. John's College, with maps and illustrations from photographs and drawings taken on the spot by the Sinai Survey Expedition, and C. F. Tyrwhitt Drake. From Mr. Goodwin:—A new and greatly-improved edition of What are the Stars? a treatise on Astronomy, by Mary Storey Lyle. From Messrs. Rivingtons:—Elementary Statics, by Hamblin Smith, 3rd edition, revised and corrected; Geometrical Conic Sections, by G. Richardson, M.A., Assistant-Master at Winchester College, and late Fellow of St. John's College, Cambridge; Analytical Geometry of Two Dimensions, by H. E. Oakley, M.A., late Fellow and Senior Mathematical Lecturer of Jesus College, Cambridge, H.M. Inspector of Schools. From Messrs. Cassells:—Elementary Astronomy, by Richard A. Proctor, B.A., F.R.A.S., with nearly fifty original illustrations; Elementary Geography, by Prof. D. T. Ansted, M.A., F.R.S., Examiner in Physical Geography in the Department of Science and Art, illustrated with original diagrams.

We are glad to learn that the Rev. E. Smith and Mr. Irving are busy with a new and complete flora of the neighbourhood of Nottingham, which we hope will ultimately include the whole of that county. The work is being done under the auspices of the Literary and Philosophical Society of Nottingham.

Dr. Petermann has written to several German papers to announce the success of a new German Arctic Expedition. According to a telegram received by him a few days ago, and since confirmed, Lieutenants Weyprecht and Payer have penetrated to 79° N. latitude, and have actually discovered the open Arctic Sea, which has been so long searched for. They employed a Norwegian sloop, and penetrated northwards between Spitzbergen and Nova Zembla, and they report an open sea from 42° to 60° E. longitude, and that but little ice was to be seen northward. By these results the anticipations of Dr. Petermann, who long ago recommended an expedition to the North of Spitzbergen, are fully borne out.

THE number of entries at the London Medical Schools is this year unusually large. University College taking the lead with the largest number of fresh entries. We understand that the means of instruction at this admirable school are further enlarged by supplying the only disadvantage at which it has hitherto laboured—a deficiency in the number of beds. In future the students at this institution are to have the privilege of attending the clinical instruction at Middlesex Hospital.

MR. W. R. S. RALSTON is to give the Ilchester Lectures at Oxford this term. They will take place on November 4, 11, and 18, and will be upon Russian Mythology and Folklore.

THE first Evening Lecture at the London Institution, Finsbury Circus, will take place on Thursday, November 2, at half-past seven, the subject being "Michael Faraday: the Story of his Life," by J. H. Gladstone, Ph. D., F.R.S.

THE Council of the Hackney Scientific Association will hold a Conversazione on November 7, at 7.30 P.M., at their Rooms, The Tabernacle, Old Street, N.E.

ON Saturday a meeting of the Senatus Academicus of the University of Edinburgh was held for the purpose of considering what steps should be taken in regard to the admission of the female medical students to the examinations. After a long discussion the Senatus adopted a resolution to the effect that no further difficulties were to be placed in the way of the ladies as regarded either matriculation or preliminary examination.

WE are glad to learn that at Marlborough College 205 boys are studying Science, and about three fourths of these have two lectures a week. Thus since the commencement of the half no form has had more than seven lectures on one subject. The following averages were obtained out of a maximum of 100 marks for each subject:—

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| Sixths and Upper Fifth—*Chemistry* . . . . . . | 65·83 |
| Upper Voluntary (from the three Fifths)—*Chemistry* | 55·91 |
| ,, ,, ,, *Magnetism* | 66·91 |
| Upper Shell Form—*Chemistry* . . . . . . . | 64·20 |
| Upper Fourth, A— ,, . . . . . . . . | 56·28 |
| Upper Fourth, B— ,, . . . . . . . . | 56·29 |
| Lower Voluntary (from three preceding)—*Magnetism* | 61·85 |
| Modern School: Upper Division—*Chemistry* . . | 53·04 |
| ,, Middle Division— ,, . . | 36·79 |
| ,, Lower Division— ,, . . | 53·45 |
| ,, Upper Division—*Magnetism* . | 72·40 |
| ,, Middle Division— ,, . . | 45·45 |

MR. HENRY WALKER states, in a letter to the *Daily News*, that interesting relics of the Glacial period are now to be seen at the Finchley Station, on the Highgate and Edgware Railway, on the branch line to Barnet, where the boulder clay is now being revealed in a section of nearly thirty-feet deep. The clay seems to have a maximum thickness of nine feet, and is rich in fossils drifted hither from the liassic, oolitic, and chalk formations of the north. Ammonites and gryphenas are found in great variety and abundance.

PROF. COPE has lately published in the Indianopolis *Journal* an account of a visit to the Wyandotte Cave, and of the animal life occurring within its limits. He reports this cave to be as well worthy the popular favour as the Mammoth Cave of Kentucky, since, although lacking the large bodies of water of the latter, it is fully equal and even superior to it in the number and beauty of its stalactites. The gypsum regions in the more remote parts of the cave are especially beautiful, this substance occurring in amorphous masses of great purity, or in the form of loose crystals resembling snow. Fourteen species of animals were found in this cave, consisting of a blind fish similar to, if not identical with, that of the Mammoth Cave, seven species of insects, two of spiders, one of centipedes, and three crustaceans. Several of these species, as might be imagined, are destitute of eyes, such organs being unnecessary to them in their subterranean abode.

THE discussion in the Indian papers about the Bis-cobra or Bhyscuppra has at length brought out a description. It appears that the name Cobra is misleading, for the animal is a saurian and has nothing to do with the snake, but its supposed poisonous qualities may have furnished the version of Cobra from Cuppra. It is a lizard of no very remarkable appearance; nor does it differ materially in outline from others of its class. Its length is from twelve to twenty inches. It has four feet and an elongated tapering head, in shape not unlike that of the common bloodsucker. It lives on trees, particularly the peepul, and on old tumbledown buildings. It is carnivorous, seeking its prey at night. On provocation it can change its hue from one shade of green to another, but it is not true that it can do more. It is often confounded with the burrowing lizard, which it much resembles, though there is a clear distinction. The latter is a vegetarian, having much longer claws, is different in gait, and cannot change its colour. The natives have a superstitious dread of the Bhyscuppra, but their fears have of late been proved to be unfounded. The creature has no poison in its mouth, nor even fangs. The writer says he has seen a goat bitten by it without unfavourable results, and has heard of other cases.

THE Cundurango is a tree found in Ecuador, the young stems and roots of which are claimed to be a specific cure for cancer and other diseases. A quantity of this was sent by the Government of that country to the State Department in Washington to be experimented upon by some physicians of that city, and the result of the inquiry having been satisfactory, a special expedition was sent out to obtain an increased supply. Much controversy has arisen, however, as to the real virtue of the plant, many physicians denouncing the whole movement as savouring of quackery and humbug. The precise botanical relationships and character of the plant have been until recently unknown; but we now learn from the *Andes*, of Guayaquil, of July 29, in a communication from Dr. Buyon, that it belongs to the order *Eupatoriaceæ*, and species *Mikania guaco* of Endlicher, and that its name of cundurango in the Quichua language means vine of the condor. It is the same plant that is called the guaco in Colombia. According to the tradition of the country, when the condor is bitten by a poisonous serpent it swallows the leaves of the guaco plant, and experiences no harm. In Colombia there are said to be three varieties of the guaco—green, purple, and white—the purple variety being intensely bitter, the white less so and more aromatic, while the green has more astringency. Dr. Bliss, of Washington, is understood to be the great champion of the cundurango, and to have accomplished several notable cures upon prominent personages, and considers it to be as reliable a specific in cancer and scrofula and other blood diseases as cinchona and its alkaloids have proved to be in zymotic diseases. It is quite certain that for many years this plant has been brought forward in tropical America as an invaluable cure for a variety of diseases. As the *Mikania guaco* is found abundantly in South America, it can readily be obtained without going into the interior of Ecuador, should it answer all the expectations of its partisans.

THE total fall of rain in Calcutta to the 31st July was 64·24 inches, the average for 17 years being 37·13 inches. This rain was accompanied by heavy floods and much damage to crops.

COAL is reported as having been found in the Nizam's dominions in the Rajpore and Kummun districts. Attention is now being directed to Beerbhoom, Chota, Nagpore, Rowah, and Bundulcund as a great mineral region.

## THOUGHTS ON THE HIGHER EDUCATION OF WOMEN *

THERE are certain confusions of ideas as to the proper range and extent of the education of women, with other and vastly different questions as to the right of the softer sex to enter upon certain kinds of professional training. Let us endeavour to get rid of some of these misconceptions. In the first place, no one denies the right to an equality of the sexes in all the elementary education given in ordinary schools. This is admitted to be an essential preparation in the case of all persons of both sexes and of all grades of social position for the ordinary work of life. But when we leave the threshold of the common school, a divergence of opinion and practice at once manifests itself. Only a certain limited proportion either of men or women can go on to a higher education, and those who are thus selected are either those who by wealth and social position are enabled or obliged to do so, or those who intend to enter into professions which are believed to demand a larger amount of learning. The question of the higher education of women in any country depends very much on the relative numbers of these classes among men and women, and the views which may be generally held as to the importance of education for ordinary life, as contrasted with professional life. Now in Canada the number of young men who receive a higher education merely to fit them for occupying a high social position is very small. The greater number of the young men who pass through our colleges do so under the compulsion of a necessity to fit themselves for certain professions. On the other hand, with the exception of those young women who receive an education for the profession of teaching, the great majority of those who obtain what is regarded as higher culture, do so merely as a means of general improvement and to fit themselves better to take their proper place in society. Certain curious and important consequences flow from this. An education obtained for practical professional purposes is likely to partake of this character in its nature, and to run in the direction rather of hard utility than of ornament. An education obtained as a means of rendering its possessor agreeable is likely to be æsthetical in its character rather than practical or useful. An education pursued as a means of bread-winning is likely to be sought by the active and ambitious of very various social grades. An education which is thought merely to fit for a certain social position is likely to be sought almost exclusively by those who move in that position. An education intended for recognised practical uses, is likely to find public support, and at the utmost to bear a fair market price. An education supposed to have a merely conventional value as a branch of refined culture, is likely to be at a fancy price. Hence it happens that the young men who receive a higher education and by means of this attain to positions of respectability and eminence, are largely drawn from the humbler strata in society, while the young women of those social levels rarely aspire to similar advantages. On the other hand, while numbers of young men of wealthy families are sent into business with a merely commercial education at a very early age, their sisters are occupied with the pursuit of accomplishments of which their more practical brothers never dream. When to all this is added the frequency and rapidity in this country of changes in social standing, it is easy to see that an educational chaos must result, most amusing to any one who can philosophically contemplate it as an outsider, but most bewildering to all who have any practical concern with it; and more especially, I should suppose, to careful and thoughtful mothers, whose minds are occupied with the connections which their daughters may form, and the positions which they may fill in society. The educational problem which these facts present admits, I believe, of but two general solutions. If we could involve women in the same necessities for independent exertion and professional work with men, I have no doubt that in the struggle for existence they would secure to themselves an equal, perhaps greater, share of the more solid kinds of the higher education. Some strong-minded women and chivalrous men in our day favour this solution, which has, it must be confessed, more show of reason in older countries where, from unhealthy social conditions, great numbers of unmarried women have to contend for their own subsistence. But it is opposed by all the healthier instincts of our humanity. A better solution would be to separate in the case of both sexes professional from general education, and to secure a large amount of the former of a solid and practical character for both sexes, for its own sake, and because of its beneficial results in the promotion of our well-being considered as individuals, as well as in our family, social and professional relations. This solution also has its difficulties, and it can, I fear, never be fully worked out, until either a higher intellectual and moral tone be reached in society, or until nations visit with proper penalties the failure, on the part of those who have the means, to give to their children the highest attainable education, and with this also provide the means for educating all those who, in the lower schools, prove themselves to be possessed of eminent abilities. It may be long before such laws can be instituted, even in the more educated communities; and in the meantime in aid of that higher appreciation of the benefits of education which may supply a better if necessarily less effectual stimulus, I desire to direct your attention to a few considerations which show that young women, viewed not as future lawyers, physicians, politicians, or even teachers, but as future wives and mothers, should enjoy a high and liberal culture, and which may help us to understand the nature and means of such culture. . . . It is in the maternal relation that the importance of the education of woman appears most clearly. It requires no very extensive study of biography to learn that it is of less consequence to any one what sort of father he may have had than what sort of mother. It is indeed a popular impression that the children of clever fathers are likely to exhibit the opposite quality. This I do not believe, except in so far as it results from the fact that men in public positions or immersed in business are apt to neglect the oversight of their children. But it is a noteworthy fact that eminent qualities in men may almost always be traced to similar qualities in their mothers. Knowledge, it is true, is not hereditary, but training and culture and high mental qualities are so, and I believe that the transmission is chiefly through the mother's side. Further, it is often to the girls rather than to the boys, and it frequently happens that if a selection were to be made as to the members of a family most deserving of an elaborate and costly education, the young women would be chosen rather than the young men. But leaving this physiological view, let us look at the purely educational. Imagine an educated mother, training and moulding the powers of her children, giving to them in the years of infancy those gentle yet permanent tendencies which are of more account in the formation of character than any subsequent educational influences, selecting for them the best instructors, encouraging and aiding them in their difficulties, sympathising with them in their successes, able to take an intelligent interest in their progress in literature and science. How ennobling such an influence, how fruitful of good results, how certain to secure the warm and lasting gratitude of those who have received its benefits, when they look back in future life on the paths of wisdom along which they have been led. What a contrast to this is the position of an untaught mother—finding her few superficial accomplishments of no account in the work of life, unable wisely to guide the rapidly-developing mental life of her children, bringing them up to repeat her own failures and errors, or perhaps to despise her as ignorant of what they must learn. Truly the art and profession of a mother is the noblest and most far-reaching of all, and she who would worthily discharge its duties must be content with no mean preparation. It is perhaps worth while also to say here that these duties and responsibilities in the future are not to be measured altogether by those of the past. The young ladies of to-day will have greater demands made on their knowledge than those which were made on their predecessors.

But the question has still other aspects. A woman may be destined to dwell apart—to see the guides and friends of youth disappearing one by one, or entering on new relations that separate them from her, and with this isolation may come the hard necessity to earn bread. How many thus situated must sink into an unhappy and unloved dependence? How much better to be able to take some useful place in the world, and to gain an honourable subsistence! But to do so, there must be a foundation of early culture, and this of a sound and serviceable kind. Or take another picture. Imagine a woman possessing abundance of this world's goods, and free from engrossing cares. If idle and ignorant, she must either retire into an unworthy insignificance, or must expose herself to be the derision of the shrewd and clever and the companion of fools. Perhaps, worse than this, she may be a mere leader in thoughtless gaiety, a snare and a trap to the unwary, a leader of unsuspicious youth into the ways of dissipation. On the other hand, she may aspire to be a wise steward of the goods bestowed on her, a centre of influence, aid, and counsel in every good work, a shelter and support to the

* Extracted from the Introductory Lecture to the First Session of the Classes of the Ladies' Educational Association of Montreal, October, 1871, by Principal Dawson, LL.D., F.R.S.

falling and despairing, a helper and encourager of the useful and active; and she may be all this and more in a manner which no man, however able or gifted, can fully or effectually imitate. But to secure such fruits as these, she must have sown abundantly the good seed of mental and moral discipline in the sunny spring time of youth. Lastly, with reference to this branch of the subject, it may be maintained that liberal culture will fit a woman better even for the ordinary toils and responsibilities of household life. Even a domestic servant is of more value to her employer if sufficiently intelligent to understand the use and meaning of her work, to observe and reason about the best mode of arranging and managing it, to be thoughtful and careful with reference to the things committed to her charge. How much more does this apply to the head of the house, who, in the daily provisioning and clothing of her little household army, the care of their health, comfort, occupations and amusements, the due and orderly subordination of the duties and interests of servants, children, and friends, and the arrangement of the thousand difficulties and interferences that occur in these relations, has surely much need of system, tact, information, and clearness of thought. We realise the demands of her position only when we consider that she has to deal with all interests from the commonest to the highest, with all classes of minds from the youngest and most untutored to the most cultivated; and that she may be required at a moment's notice to divert her thoughts from the gravest and most serious concerns to the most trifling details, or to emerge from the practical performance of the most commonplace duties into the atmosphere of refined and cultivated society. But it would be altogether unfair to omit the consideration of still another aspect of this matter. Woman has surely the right to be happy as well as useful, and should have fully opened to her that exalted pleasure which arises from the development of the mind, from the exploration of new regions of thought, and from an enlarged acquaintance with the works and ways of God. The man who has enjoyed the gratification of exercising his mental powers in the fields of scientific investigation or literary study—of gathering their flowers and gems, and of breathing their pure and bracing atmosphere, would surely not close the avenues to such high enjoyment against woman. The desire to do so would be an evidence of sheer pedantry or moral obliquity, of which any man should be ashamed. On the contrary, every educated man and woman should in this respect be an educational missionary, most desirous that others should enjoy these pleasures and privileges, both as a means of happiness and as a most effectual preventive of low and pernicious tastes and pursuits.

## *RECENT RESEARCHES ON FLIGHT**

OF late the perplexing problem of flight has received a greater amount of attention from physiologists and savants than has been bestowed upon it for years, and the result of their researches and experiences is in a fair way of becoming remarkable for its fruit-bearing character. Whilst abroad, such men as Borelli, Straus-Durckheim, Chabrier, Girard, and Marey, have severally given to the world the gist of their labours in this branch of science; at home, the Duke of Argyll and Dr. J. Bell Pettigrew have awakened our deep interest by their views on natural and artificial flight. To the latter is due the honour of giving birth to the celebrated "figure-of-8 wave theory," that is now attracting so much notice in our aeronautical schools.

As early as 1867, Dr. Pettigrew delivered, before the Royal Institution of Great Britain, a lecture, in which he propounded that novel theory, and in 1868 he published in the "Transactions" of the Linnean Society an elaborate memoir on "The Mechanical Appliances by which Flight is attained in the Animal Kingdom." The year after, Prof. J. E. Marey, in the "Revue des Cours Scientifiques," bore out Dr Pettigrew's ideas, by the detail of his experiments with the sphygmograph, with which he succeeded in causing the wings of insects and birds to register their own movements. He says:—"But if the frequency of the movements of the wing vary, the form does not vary. It is invariably the same; it is always a double loop, a figure of 8. Whether this figure be more or less apparent, whether its branches be more or less equal, matters little: it exists and an attentive examination will not fail to reveal it." An indefatigable worker, Dr. Pettigrew continued, without pausing, the task to which he had set himself—and that to him is indeed a labour of love; and in this year's "Transactions" of the Royal Society of Edinburgh, we have from his pen a complete monograph on "The Physiology of Wings," in which he treats with equal felicity of both natural and artificial flight. The mass of interesting fact brought to light by the author is too copious to allow of lengthened discussion, but from it we abstract the following items:—

"The wing is generally triangular in form. It is finely graduated, and tapers from the root towards the tip. It is likewise slightly twisted upon itself, and this remark holds true also of the primary or rowing feathers of the wing of the bird. The wing is convex above and concave below; this shape, and the fact that in flight the wing is carried obliquely forward like a kite, enabling it to penetrate the air with its dorsal surface during the up stroke, and to seize it with its ventral one alike during the down and up strokes. The wing is moveable in all its parts; it is also elastic. Its power of changing form enables it to be wielded intelligently, even to its extremity; its elasticity prevents shock, and contributes to its continued play. The wing of the insect is usually in one piece, that of the bat and bird always in several. The curtain of the wing is continuous in the bat, because of a delicate elastic membrane which extends between the fingers of the hand and along the arm; that of the bird is non-continuous, owing to the presence of feathers, which open and close like so many valves during the up and down strokes.

"The posterior margin of the wing of the insect, bat, and bird, is rotated downwards and forwards during extension, and upwards and backwards during flexion. The wing during its vibration descends further below the body than it rises above it. This is necessary for elevating purposes. The distal portion of the wing is twisted in a downward and forward direction at the end of the down stroke, whereas at the end of the up stroke it is twisted downwards and backwards. The wing during its vibrations twists and untwists, so that it acts as a reversing reciprocating screw. The wing is consequently a screw, structurally and functionally. The blur or impression produced on the eye by the rapidly oscillating wing is twisted upon itself, and resembles the blade of an ordinary screw-propeller. The twisted configuration of the wing and its screwing action are due to the presence of figure-of-8 looped curves on its anterior and posterior margins; the curves, when the wing is vibrating, reversing and reciprocating in such a manner as to make the wing change form in all its parts."

We may further point out that Dr. Pettigrew has not based his ideas on the structure of wings on mere theoretical considerations. Besides elaborate anatomical examination, he has entered with a true experimental spirit into a close study of the visible movements of most of the winged tribe. The very excellent diagramatic views with which his paper is elaborately illustrated convey at a glance much that it is difficult to express in words. In proof of this the reader need but compare those figures bearing on the wing movements of the butterfly, the dragon-fly, and the bird.

On these and similar deductions from the practical study of natural history, Dr. Pettigrew bases his elements of artificial flight.

J. MURIE

* Communicated by the Author from *Land and Water*.

## *INSTRUCTIONS FOR OBSERVERS, AT THE ENGLISH GOVERNMENT ECLIPSE EXPEDITION*, 1871

### SPECTROSCOPIC OBSERVATIONS

THE instruments used should, if possible, be of the following forms; and experience has shown that they should all be equatorially mounted and driven by clockwork (E of course excepted):—

*Instrument A.*—An analysing spectroscope showing the whole spectrum in one field, with reference spectra, or some means of rapid record, and with long slit and long collimator mounted at right angles to the axis of a reflecting telescope of large aperture and short focal length, with large finder, the slit of the spectroscope, of course, lying in the focus of the speculum. This combination enables us to obtain a small bright image of the corona, and by throwing this small image on the long slit, to observe the spectrum of the corona on both sides the dark moon—the long collimator permitting the slit to be as wide as possible, so that the maximum of light is admitted. The prism throwing the reference-spectrum into the collimator slides along a bar, so that the reference-spectrum may be made to occupy any part of the field, the height of this spectrum forming a standard of measurement.

*Instrument B.*—An integrating spectroscope, mounted equatorially, with clockwork or some equivalent arrangement, with long collimator, and object-glass of such aperture that an angle of about 3° is taken in. This, by means of a finder, should be directed to the sun's centre. It should be furnished with a reference-spectrum, or ready means of record.

*Instrument C.*—An analysing spectroscope of great dispersive power and automatic arrangement, attached to a clockwork-driven equatorial reflector or refractor, in the manner used for observations on the Janssen-Lockyer method.

*Instrument D.*—An analysing spectroscope of moderate or small dispersive power, attached to an equatorial in the same manner.

*Instrument E.*—A hand integrating spectroscope.

INSTRUCTIONS FOR USE OF INSTRUMENT A.—With this instrument it should be sought to determine, if possible :—

1. The spectrum of corona visible before and after totality.
2. The spectrum of any bright rays visible before or after totality.
3. The spectrum of lowest layer of chromosphere *just* preceding and following totality.
4. Whether there are any dark lines (attention to be directed to D, E, *b*, F only) in the spectrum of the corona during totality, at any distance from the sun; and, if so, at what distance?
5. The spectrum of any bright streamer seen during totality.
6. The existence and position of bright lines between D and 1474 (Kirchhoff) during totality.
7. Comparative heights of C, $D^3$, F, and 1474 (Kirchhoff), and lines, if any, between D and 1474 during totality.
8. The spectrum of the dark moon.

The observations should be conducted as follows :—

1. If reference-spectra are used, connect the bar carrying the spark-tube, by means of a piece of string, to the eye piece, so that the amount of light thrown by the small lens on to the slit, and parallel to it, may be controlled.
2. Carefully adjust the finder.
3. Have a slit of such width that on a very faint cloud, with the whole aperture of the reflector, the principal lines of the solar spectrum are alone visible (the three lines of *b* should appear as two).
4. Rotate the telescope-tube in order that the slit may lie along the line joining the points of contact at beginning and end of totality.
5. The observer at the finder should bring the streamer on to the slit, without rotating the telescope-tube.
6. If there are clouds near the sun, on no account move the telescope in right ascension, or meddle with the clock.

INSTRUCTIONS FOR USE OF INSTRUMENTS B AND E.—With these instruments it is possible to examine the changes which take place in the quality of the total amount of light proceeding from the circumsolar regions during the whole time of greatest obscuration. The observations may be made, say, from half an hour before totality to half an hour after; during totality, and if rapid changes are discovered before or after, the observations should be recorded every ten seconds or so, the times of commencement and end of totality being carefully noted, as it is important to connect the changes observed with the amount of chromosphere visible.

The observations should be conducted as follows :—

1. Adjust the driving-clock carefully to solar motion.
2. Adjust the slit so that most of the principal lines can be seen in the spectrum of a faint cloud.
3. Point the instrument to sun's centre three-quarters of an hour before totality, and connect the clock.
4. As totality approaches, observe which dark lines fade out and then brighten, noting the relative intensity of such bright lines.
5. *At the instant of totality* pay particular attention to the spectrum, in order to ascertain whether all lines are reversed, or only some.
6. During totality note every ten seconds all changes, especially of intensities, *e.g.*

1474 (Kirchhoff) as compared with . . . . . F
,, ,, ,, . . . . . C
,, ,, ,, . . . . . $D^3$
,, ,, ,, . . . . . *b*
,, ,, ,, any lines between D & 1474
C as compared with . . . . . . . . . F

7. Note whether C entirely disappears.

Repeat the foregoing observations in reverse order till all changes cease.

INSTRUCTIONS FOR USE OF INSTRUMENTS C AND D.—It is important not only to compare observations made with instruments of large dispersion and those of smaller dispersive power, but to determine the obliterating effect of the atmospheric illumination, so as to render observations made during eclipses comparable with those made on the Janssen-Lockyer method.

The observations with these instruments should be conducted as follows :—

1. Before the eclipse commences sweep round the sun, and giving angles from the true north through the true east of the sun from 0° to 360°,* make a diagram of the chromosphere, its height being determined by the height of the C line; especially note all prominences of both kinds (that is, the diffused and eruptive ones), and the amount of motion indicated by changes in wave-length.
2. Find, near the point at which the sun will disappear, an average plane-topped region of the chromosphere where a little motion on either side does not brighten, or thicken, or lengthen the lines.
3. Observe the spectrum of this, and of the gradually narrowing underlying region of photosphere, and note the effect of the diminution of the photospheric light; such effect may probably be apparent before totality; note increased number of lines, their relative heights, and whether there is a diffused band over *b*; note also the width of F. Note also lines between D and 1474, and which lines disappear as the moon covers the lowest layers of the chromosphere. The spectrum should be swept from C to G before totality, and from G to C immediately totality has commenced.
4. During totality determine how far from the dark moon any spectrum is visible, and what that spectrum is. The brightest part of the *Outer* Corona should be chosen for this observation, the telescope being directed to it by means of the finder.
5. Observe the spectrum of a streamer, also, if there be time.
6. Just before the end of totality watch the chromosphere above the point at which the sun will reappear. Observe the phenomena in reverse order. See III.

PHOTOGRAPHIC OBSERVATIONS.—The instrument recommended to be employed during the eclipse of this year is a camera of about 4in. aperture and 30in. focal length, equatorially mounted, and driven by clockwork.

The pictures taken with such instruments should be exposed as follows, and negatives only should be taken :—

Where the totality is about 2 minutes.

| Beginning. | | | Middle. | | | End. |
|---|---|---|---|---|---|---|
| $5^s$ | $7^s$ | $10^s$ | $15^s$ | $10^s$ | $7^s$ | $5^s$ |

Where the totality is about 4 minutes.

| Beginning. | | | | Middle. | | | | End. |
|---|---|---|---|---|---|---|---|---|
| $5^s$ | $7^s$ | $10^s$ | $20^s$ | $15^s$ | $20^s$ | $10^s$ | $7^s$ | $5^s$ |

To insure a given number of pictures, it is desirable to have the same number of baths and plate-holders.

If this system be carried out we shall be able—

1. To use equal exposures for the examination of the Corona on either side the middle point of totality *in each series.*
2. To compare pictures of equal exposure taken at the same time with reference to the middle of the phenomenon *at all stations.*
3. To determine the effect of exposure.

TELESCOPIC AND NAKED-EYE OBSERVATIONS.†—*Before totality.*

1. Note how long the corona is visible before totality along the edge of the dark moon opposite the point at which the sun is about to disappear.
2. Sketch any rays visible before totality; give length, colour, and structure, as well as position.

*At commencement of totality.*

3. Sketch general outline and any rays (streamers) or rifts.
4. Note if there be a blaze of light or glare where the sun has just disappeared.

*Middle of totality.*

* Diagrams should be prepared showing the N. and E. points of sun as seen (1) with the naked eye, (2) in an inverting telescope, and (3) *on the slit plate of the spectroscope*

† These observations should be made by observers *in pairs*, and they should not compare notes. If telescopes are thus used, 2 inches aperture and a power of 20 should be employed.

5. Sketch general outline, rays (steamers), and rifts.

*Near end of totality.*

6. Sketch general outline and any rays of streamers or rifts.

7. Note if there be a blaze of light or glare where the sun is about to reappear.

*After totality.*

8. Sketch any rays that may be visible; give length, colour, and structure, as well as position.

*Questions to be answered in writing immediately totality is over.*

*a.* Has there been any change? if so, specify what change.

*b.* Have especially the *dark rays* or rifts changed?

*c.* Describe what has been constant throughout, and define its structure.

*d.* State the colours you observed outside the red prominences.

*e.* Were the colours anywhere arranged *in layers* round the sun?

*f.* Were the colours anywhere arranged *radially?*

*g.* As the moon passed over the sun were the colours similar to those successively thrown over any one portion of the landscape?

*h.* State colours of rays and of spaces between them.

*i.* Did the dark rifts extend down to the moon, or did they stop short above the denser layers of the chromosphere?

*k.* Were the rays brightest near or far away from the moon?

*l.* What was the comparative brightness of the rays, chromosphere, and outer corona?

N.B.—Cards should be prepared, 8 inches square, with a circle 2 inches in diameter, filled in with some dark colour, in the centre. Round this circle the sketches should be made, the north point (or the vertex, as the case may be) being shown, and whether the sketches were made by means of an inverting telescope or with the naked eye.

SUGGESTIONS FOR TIMING THE PROGRESS OF THE ECLIPSE.—In Sicily, last year, the following method of recording the lapse of time during totality was found to prevent all excitement, and made the 80 seconds seem a very long time.

Determine the number of seconds of totality at the station—say 120.

Then, at the moment of totality, let one person attached to each party of observers, carefully observing the face of a chronometer or watch, say—

"You have now 120 seconds."

After 5 seconds,

"You have still 115 seconds."

After another 5 seconds,

"There are still 110 seconds remaining;"

and so on.

This may be done in a very steadying manner.

*The times at which any of the phenomena occur must be noted by another observer.* J. N. L.

## HISTOLOGY

### The Auditory Organ of Gasteropoda

DR. F. LEYDIG, of Tübingen, gives an interesting account of the Auditory Organ of Gasteropoda in the last part of Max Schultze's *Archiv fur Mikroskopische Anatomie.* After a short historical introduction, in which the labours of previous observers are referred to, Prof. Leydig describes the form and divisions of the brain or cerebral ganglia in this class, and shows that these are fundamentally the same in *Limax, Arion, Vitrina, Helix, Clausilia, Carychium, Succinea, Physa, Planorbis, Ancylus.* This supra-œsophageal or cerebral ganglion in these animals consists of two superior ganglionic lateral masses united by a commissure. The subœsophageal ganglion consists of an anterior portion, the ganglion pedale, and a posterior, the ganglion viscerale, which again are connected with the supraœsophageal ganglion by commissural bands. The ring thus formed is traversed by the œsophagus, the excretory ducts of the salivary glands, and the aorta. The anterior lobes of the cerebral ganglion give off the nerves of the tentacles and the optic nerves, and four other pairs. The auditory organ is *apparently* connected with the anterior division of the subœsophageal ganglion. It varies but little in size in different species, whatever may be their difference in magnitude. The organ is of spherical form, as seen from above, but flattened when seen in profile, where it is in contact with the ganglion. It is composed of a connective tissue capsule, made up of two layers—an outer looser investment, and an inner firmer tissue; between the two is a plexiform arrangement of muscular fibres and fasciculi. The inner capsule is lined by a layer of epithelium, which is thicker opposite the point of attachment of the nerve than elsewhere, and when perfectly fresh presents a very indistinct division into cells; of these there appear to be two varieties characterised by their nuclei; one form of nucleus being small, and lying near the attached surface of the cells, that is to say, externally; the other large and round, with a fusiform nucleus. Cilia appear to be always present, but are so extremely fine as to be occasionally scarcely visible. It is most distinct in *Ancylus fluviatilis,* and in this animal the trembling movement of the otoliths is most perceptible. He has seen appearances in *Helix hortensis* and *Clausilia similis,* which lead him to think that the large nucleated cells have bristles attached to them, instead of cilia like the smaller cells. The otoliths exhibit some, though insignificant, variations in size, form, and number. The majority approximate to an oval form, as in the *Helicinidæ;* they are more pointed in *Ancylus* and *Planorbis.* Smaller animals, as *Carychium minimum,* have very small otoliths. They are rounder in young than in older specimens of *Helix,* and at a later period they assume a cell-like appearance, the central part being clearer than the periphery, or a space forming in it which resembles a nucleolus; but he has no doubt, from his previous observations on the embryos of *Paludina vivipara,* that they crystallise out from the fluid of the auditory vesicle; being at first punctiform bodies, then become pointed at their extremities, and increasing by the deposition of successive laminæ on their surface. The idea suggested that they gain entrance from without is quite erroneous. His examinations of the real connections of the auditory nerve succeeded best in *Vitrina diaphana,* and these showed that the lateral commissures of the brain connecting the supra- and infra-œsophageal masses consist of the two commissures themselves, of a sympathetic nerve, of the auditory nerve or canal, and a blood vessel, all connected together by loose connective tissue. The auditory nerve, after leaving the capsule, first runs obliquely outwards to follow the curvature of the anterior division of the infra-œsophageal ganglion, then suddenly bends upwards, and thus ultimately reaches, not the infra-, but the supra-œsophageal ganglion with which it is really in connection. Though holding the same relation to the ear that the optic nerve does to the eye, it differs from ordinary nerves in being hollow; hence its name of ear canal. The wall consists, like that of a nerve, of a homogeneous membrane, surrounded by looser connective tissue, and lined by epithelium. The interior is not filled with nerve fibrillæ. Prof. Leydig then notices the relations of this nerve to the passage leading from the ear towards the skin in *Cephalopoda,* in connection with which, however, no external opening has been found, though searched for, by Owsjannikow, Kowalewsky, and Boll.

## SCIENTIFIC SERIALS

IN the *Quarterly Journal of Science* for October, three of the articles are continuations of papers which have appeared in previous numbers of the Journal. Mr Mungo Ponton concludes his discussion of "Molecules, Ultimates, Atoms, and Waves." Lieut. S. P. Oliver gives another paper "On Modern British Ordnance and Ammunition," detailing the structure of some recently manufactured ordnance; and from the editor we have "Some further Experiments of Psychic Force." After replying to adverse criticisms on his previous paper, Mr. Crookes details some fresh experiments which he considers to "confirm beyond doubt the conclusions at which he arrived in his former paper, namely the existence of a force associated in some measure not yet explained, with the human organisation, by which force increased weight is capable of being imparted to solid bodies without physical contact." The experiments detailed were all performed in the presence of Mr D. D. Home, or of a lady in whom this force is stated to be remarkably developed; the accordion is no longer employed, while in the balance experiments the operator's hands, instead of lying on the board attached to the balance, are placed in a vessel of water laid on the board. Mr. W. Mattieu Williams gives a useful abstract of the views advanced in his "Fuel of the Sun," for the benefit of those who have not time to read the larger work. The author of an anonymous paper "On the recent Gun-cotton Explosion" condemns the reaction against the use of gun-cotton, which has set in since the Stowmarket catastrophe, and attributes the explosion to culpable carelessness in the process of washing the free acid out of the cotton, and the extent of the disaster to the fact of the utterly needless stowage of large quantities of the manufactured article in the factory itself.

THE *Zeitschrift für Ethnologie* 1871, Heft iii.) begins with the second number of a series of papers by A. Ermann, entitled "Ethnographic Observations on the Coasts of Bering's Sea." The author passes in review the various articles of food and the vessels and methods employed for its preparation among the numerous tribes inhabiting the islands, coastlands, and interior of North-Western America; and he endeavours from his observations of this phase of domestic life to deduce conclusions in reference to the identity, or differences of origin, of these races. Herr Ermann draws attention to the fact that the Aleutians, like the people of Kamtschatka, subject some kind of fish to a process of fermentation before eating it, and that these, as well as all the neighbouring races, show a decided repugnance to the use of salted food, and ascribe to their abstinence from such a diet their superiority over the Russians both as regards length of vision and the continuance of unimpaired sight to old age. Heated stones thrown by means of wooden tongs into a wooden, or basket-work vessel, were everywhere found to be in frequent if not universal use as a substitute for our methods of boiling; and where vessels of a large size were required for preparing blubber, their wooden boats were used for the purpose, and the oil poured into bladders to be kept, not only to light their dwellings, but also to heat them by means of their bone lamps, known as *zirniki*. The Aleutians are the most advanced of all the tribes, and have amalgamated so thoroughly with the Russians, among whom they have lived, that it is difficult at first sight to detect their national characteristics from those which they have borrowed from their conquerors. In physiognomy they differ, however, very strikingly, and their dark, yellowish-brown skins and obliquely cut eyes remind one of the Mongolian type. The author treats of the sexual relations of the Aleutians, their early marriages, and the forms of polygamy and diandry practised among them; and describes their singular social houses, in which from 50 to 200 individuals live together in one house of considerable size (180 feet in length), sunk ten or fifteen feet below the surface of the ground. This paper, which is exhaustive as far as it goes, concludes with a notice of the boats, modes of navigation, and hunting, and of the weapons of these people, whose history has, in the present day, acquired special interest since the purchase of Russian America by the Americans. We have next a paper on the "Archæological Remains of Brandenburg," by the Prussian district judge, Ernst Friedel. It possesses scarcely more than a local interest, except in regard to the notice of the lost town in Blumenthal, the name and age of which are unknown, and whose history seems clouded in mystery. In 1689 the walls were still standing six feet above the ground, and the foundations of a church, of two large buildings conjectured to have been a castle, and of a townhall, could still be traced, with the well-defined positions of outer walls, gates, fosses, main and transverse streets, &c; yet long before that time all knowledge of the place had been lost. When Prof. Beckman recorded its condition in 1751 (in his "History of the Margravate of Brandenburg"), a thick growth of trees had nearly obliterated the stone outlines of this lost town. Judge Friedel last visited the spot in 1870, at which time the walls had disappeared, but there remained traces of graves and of the foundations of Cyclopean walls, which, together with the presence of the so-called Semnonian stone, known in the district as the "Stone of the Marches," inclined him to the opinion that we have here the site of a prehistoric seat of worship, with its surrounding habitations. The stone, which lies at the foot of an oak, is about seven feet in length and six feet in width. The author is of opinion that the traditions and remains of the lost town of Blumenthal may refer to two widely separated periods; and that it may be a station of the ancient Semnones re-occupied in the 10th, 11th, and 12th centuries, or later; many German villages having become extinct in those troubled times, and even during the Hussite and other religious wars.

THE *K. Danske vid. Selsk Forh.* contains an interesting paper, incorporated in the *Z. f. Ethnologie*, by Prof. A. C. Oersted, on the *Silphium* of the ancients, which formed the staple commerce of the Roman colony of Cyrene, in North Africa, the present Barka. It was esteemed a great luxury by Greek and Roman epicures, who used its milky juice, mixed with meal, to give piquancy to their food, and employed it likewise medicinally. Under the rule of the Ptolemies the trade fell off, until at length, under the Emperor Nero, the consignment of one plant of the Silphium was deemed worthy of record. From a careful examination of the representations of this plant with its fruit on the coins of Cyrene, Prof. Oersted is led to infer that the much coveted Silphium was nearly allied to the *Narthex asafœtida*, found by Falconer in Northern Kaschmir, and since cultivated with success in the Botanical Gardens in Edinburgh, and he gives two plates in illustration of his opinion, one of which shows the Narthex reduced to the size of the plant delineated on the Cyrenian coin, the other being a *facsimile* of the coin itself. The resemblance between the two is most striking.

## *SOCIETIES AND ACADEMIES*

### PARIS

Academy of Sciences, October 16.—M. Chasles communicated some theorems concerning the determination of a series of groups of points on a geometrical curve.—M. P. A. Favre read a continuation of his thermic investigations upon voltaic energy, in which he described the results obtained under various conditions with batteries consisting of a plate of the alloy of palladium and hydrogen in dilute sulphuric acid and of a plate of platinum in solution of sulphate of copper. He tabulates his results, and also gives the results of experiments on the electrolysis of the acids employed.—M. Lecoq de Boisbaudran presented a memoir on some points of spectrum-analysis and on the constitution of induction-sparks.—M. F. M. Raoult presented a memoir giving the results of investigations of the calorific coefficients of the hydro-electric and thermo-electric currents, from which he concludes that the heat evolved by an electric current is independent of the nature of the battery employed, the calorific coefficient $Ke$ being the same for all sources of voltaic electricity.—M. Delaunay noticed the reappearance of Tuttle's comet, which was discovered at Marseilles by M. Borelly on the night of the 12th 13th October. The reappearance of this comet at the time calculated is important as confirming its supposed identity with the second comet of 1790.—M. Chasles replied to the remarks made by M. Bertrand at the last meeting on the subject of the Arabian astronomer, Aboul Wéfa.—M. Berthelot communicated a further note on his researches upon the ammoniacal salts of the weak acids, relating chiefly to certain thermic phenomena observed when a solution of carbonate of ammonia is mixed with solutions of other alkaline carbonates.—A note by MM. C. Friedel and R. D. Silva on the action of chlorine upon various bodies of the series in $C^3$ and on the isomers of trichlorhydrine was read.—M. Marey communicated a note on the duration of the electric discharge in the torpedo, in continuation of a note presented by him to the last meeting.

### PHILADELPHIA

Academy of Natural Sciences.—April 18.—Mr. Vaux, Vice-President, in the chair.—Prof. Leidy made the following remarks on some extinct turtles from Wyoming Territory. Several species of extinct turtles from the tertiary deposits of Wyoming differ from those previously described by me from the same formation. They are indicated by imperfect, though sufficiently characteristic, remains, sent to me by Dr. J. Van A. Carter, of Fort Bridger; and by others obtained during Prof. Hayden's exploring expedition the last year.—"Anosteira ornata." One of the turtles is founded upon a number of isolated plates and fragments of others of the carapace of about four different individuals, obtained from Church Buttes and Grizzly Buttes, Wyoming. The specimens are mainly marginal, including two pygal plates. The latter are remarkably thick at the fore part, where they are hollowed into a concavity directed forward, and bounded below by a projecting ledge. This concavity continues outward and forward upon the contiguous marginal plates as a groove, bounded by an inferior ledge, which would appear gradually to become narrower, and disappear at the third marginal plates in advance. The upper part of the pygal plate slopes on each side from a median acute ridge, or carina, which subsides at the posterior third. The marginal and pygal have all been conjoined with the costal plates by suture, and the former in addition by gomphosis, as in living emydes. The free surfaces of the plates are closely covered with radiant elevations. These centrally form rounded tubercles and peripherally more or less interrupted ridges with more or less interrupted branches. Apparently in younger plates the elevations form more continuous radiant and branching ridges, which would appear in older animals to have become more and more broken so as to form rounded tubercles. In some specimens the radiant ridged appearance is more conspicuous on the under surface of the marginal plates, while the rounded tuberculous condition is more obvious above. In two marginal plates, conspicuously tuberculated above, the lower surfaces are perfectly smooth. These probably pertain to a different species. None of the plates exhibit scute impressions, generally so evident in the emydes. Anosteira ornata was almost the size of the palm or middle hand. A pygal plate measures about eleven lines in length and breadth; and its height or thickness in front is seven lines. Another plate from a younger animal measures about seven lines long, eight broad, and four lines thick in front.—"Hybemys arenarius." The second turtle, almost as large as our common *Emys picta*, is founded on two specimens obtained by Prof. Hayden from a tertiary formation on Little Sandy Creek. They consist of a marginal plate and the portion of a costal plate. The bones are proportionately thicker than in our common emydes, but like them are smooth and deeply impressed by the scutes. The marginal plates appear to be the ninth of the series. From the groove of the costal scute impression it is directed quite as abruptly outwardly as in any recent emys. Its peculiarity, upon which I have founded the genus, is a striking character. The surfaces, separated by the groove of the marginal scute impressions, present each a half circular boss at the fore and aft borders of the bone. Thus from this specimen we may infer that the margin of the carapace was ornamented with a series of hemispherical bosses, each of which was situated in the position of and divided by the sutures of the marginal plates. The breadth of the specimen fore and aft and transversely is half an inch.

April 25.—"Morphology of Carpellary Scales in Larix, by Thomas Meehan. The facts which I have from time to time contributed, verbally or in papers, to the Academy, in regard to longitudinal series of axillary buds, and adnated and free leaves in coniferous plants, will, I believe explain something of the structure of the flowers of coniferæ, which, if not quite distinct from any view before taken, will at least have reached the conclusion by an original line of argument. I have shown that in the cases where there are longitudinal series of buds, one of the buds, and generally the upper supra-axillary one, is the largest. So far as this longitudinal series of buds is concerned, I find by extensive observation that there are very few of our American trees or shrubs which do not produce them under some circumstances, although they are more generally apparent in some than in other. In many cases they do not break quite through the cortical layer, but continue to grow from year to year, just as the wood grows, always remaining just under the outer bark. It is from these concealed but living buds that the flowers of the *Cercis*, or the spines of *Gleditschia*, will often appear from trunks many years old. In *Magnolia* and *Liriodendron* these concealed buds are easily detected by a thin shave of the outer bark with a sharp knife. In very vigorous shoots of the latter, a series of two—one supra-axillary—is not rarely found prominently above the bark. In many cases one of these buds, usually the lower, and really axillary one, never pushes into growth. In *Gymnocladus* neither upper nor lower would probably ever push, only for the fact that it matures no terminal bud, and thus the laterals have to renew the next season's growth. But for this, *Gymnocladus* would go up like a palm, or, more familiarly, as *Aralia spinosa*, does, without a single branch. Failing in the terminal, but two laterals push, giving the branches their dichotomous character. The two which push are always the upper ones in the series of 2, 3, or 4, which appear in this species. The purpose of this duplication of axillary buds will interest all who study this part of botany. I find that they are not for the duplication of parts, but are separately organised from one another. Thus in *Cratægus* and *Gleditschia*, the upper bud produces a spine, the lower is organised to grow as an axillary shoot the next season. But the best illustration of the distinctive organisation is in those cases where both upper and lower buds sometimes push the same season, as in *Itea*, *Lonicera*, *Caprifolium*, or *Halesia*. Here we find that one is organised for floral organs, and the other for axillary prolongation. The upper bud always has the same function, and the lower its own, in the same species. A flower being a modified branch, in which the bract is the leaf and the peduncle the axillary bud, it follows that the laws of axillary stem-production will be more or less reproduced in the inflorescence. What I have proposed to myself in this paper is simply to show that the scales in the male catkin of *Larix* are modified true leaves; while in the female they arise from buds of another organisation, being the morphologised secondary leaves, or phylloidal shoots, as I term them, of other coniferous genera.

## BOOKS RECEIVED

ENGLISH.—A Systematic Handbook of Volumetric Analysis, 2nd edition: F. Sutton (Churchill).—Text-book of Geometry, Part 1: J. S. Aldis (Deighton and Bell).—Introductory Notes on Lying-in Hospitals: F. Nightingale (Longmans).—Notes of a Naturalist in the Nile Valley and Malta: A. L. Adams (Edmonston and Douglas).—The Science of Arithmetic: Cornwell and Fitch (Simpkin and Marshall).—The School Arithmetic: Cornwell and Fitch (Simpkin and Marshall).—Partial Differential Equations: an Essay: S. Earnshaw (Macmillan and Co.).—Thoughts on Life Science: E. Thring (Macmillan and Co.).

AMERICAN.—Earthquakes, Volcanoes, and Mountain Building: J. D. Whitney (Cambridge, University Press).

FOREIGN.—Physique Sociale, ou essai sur le développement des facultés de l'homme: A. Quetelet (Brussels, Muquardt).—Anthropométrie, ou mesure des différentes facultés de l'homme: A. Quetelet (Brussels, Muquardt).—Medizinische Jahrbücher: S. Stricker.

## DIARY

*MONDAY*, OCTOBER 30.

LONDON INSTITUTION, at 4.—On Elementary Physiology (I.): Prof. Huxley, LL.D., F.R.S.

*THURSDAY*, NOVEMBER 2.

LONDON INSTITUTION, at 7.30.—On Michael Faraday; the Story of his Life: Dr. J. H. Gladstone, F.R.S.

CHEMICAL SOCIETY, at 8.—On Anthraplavic Acid: W. H. Perkin.

LINNEAN SOCIETY, at 8.—On the Origin of Insects: Sir John Lubbock, Bart., F.R.S.—Notes on the Natural History of the Flying Fish: Capt. Chimmo.—On a Chinese Gall, allied to the European Artichoke Gall: A. Müller, F.L.S.

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